MAN

THE JOURNAL OF THE ROYAL ANTHROPOLOGICAL INSTITUTE

NEW SERIES
VOLUME 10
1975

Published quarterly
by the
Royal Anthropological Institute of Great Britain and Ireland

World list abbreviation: *Man* (N.S.)

CONTENTS

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NEW SERIES, VOL. 10, NO 1 MARCH 1975

CONTENTS

INDEX OF BOOK REVIEWS

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THE PROMISING PRIMATE

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Assuming there is some substance to the idea of the social contract as central to theories of the origin and organisation of society, is there any way we can look at the evidence from primate evolution to support an argument for the genesis of the contract? The hypothesis presented shows one such way by showing how human reason is a possible outcome of human instinct, and how social life is a possible outcome of an instinct-based group life. The key point is to dispense with arguments from institutions and to argue from human relations and the relations between those relations. Meta-relations in other words

I

I want to venture a hypothesis about the evolution of social relations, i.e., the relations of social structure and social order. My starting point is the long tradition in western thought which utilises some version or other of the social contract as the basis of society. The classical theories of Hobbes, Locke, and Rousseau still provide modern social science with its foundation and one has only to cite Mauss's ideas about prestations and reciprocity, or Lévi-Strauss's rendition of conjugality as commodity exchange, or Barth's transactional analysis to see how persistent is the idea of the social contract. But the classical theories of contract presuppose the very facts they purport to explain: a capacity to reason about the supra-personal or society Hobbes, having asserted that men live in mutual fear of each other which results in a war of all men against all men, suggests that to surmount this state of nature men's reason dictates a search for peace, which was accomplished by the relinquishment of the right to resist invasion and the right to molest (Hobbes 1841: 11, 17–18). Whence comes this capacity to reason about oneself in relation to others? It cannot be self-preservation, for other species have preserved themselves very well without such reasoning powers. Similarly Rousseau argues that each must renounce his right to all in order to re-incorporate into the body politic and this renunciation and re-incorporation is sanctioned by a general will which will constrain the individual (Barker 1947: 257–61). Whence comes this general will and the individual's awareness of his participation in it? It is precisely this awareness of others that we want to account for, we want to know what conditions made it possible for the primate man to reason about himself and thence attain the self-consciousness that is summed up in society. Both Locke and Hume recognised this, the former noting that the first society was that between man and wife, which gave beginning to that between parents and children. The latter noted that the 'first and original principle of human society . . . is no other than that natural appetite betwixt the sexes which unites them together, and preserves their union till a new tye takes place in their concern for their common offspring. This new concern becomes also a principle of union betwixt the parents and offspring, and forms a more numerous society' (Hume 1888: 486). Hume goes on to argue that, given

man's overriding instinct for self-preservation, there arises a conflict between innate selfishness and affection for others which is resolved by the convention of the promise, which was one of the three fundamental laws of nature on the strict observance of which the peace and security of human society entirely depend (1888. 526). But perceptive though Hume's remarks are, it can still be argued that other species manifest a strong instinct for self-preservation and an affection (or at least solicitude for their young) without finding it necessary to make promises or to reason about their state.

The idea of a social contract, covenant or promise is central to our attempt to understanding society and is at the root of modern social theory. Although we can never adduce incontrovertible historical evidence for the origin of such an institution, or even come to know whether it is historically important, its philosophical importance is undoubted and there is a strong case for trying to establish the *possible* conditions in primate evolution for the origin of the promise and thence for the beginning of self-consciousness, reason and society.

II

Compared to other species most primates have a relatively long phase of immaturity resulting in a sustained dependency of infant upon adult (Jolly 1972: 214). Among primates, the dependency of the human infant on the human parent is the most lengthy and the most intense. The obvious corollary of this, which, surprisingly, seems not to have been fully recognised, is that the adult parent, notably the mother, must display a more sustained and intense concern for the infant, and is, in a sense, more dependent on the infant than is the case in other species.

The lengthy phase of human immaturity is made necessary primarily because bipedal motion limits pelvic size, and this in turn restricts the head size of the new-born and thence delays the development of the brain *in utero* (Jolly 1972 216) That the human infant, and especially the human brain in effect continues gestation outside the womb means also that certain brain determined *behaviour* will develop after birth. It is this factor in particular that has confused some theorists who tended to correlate total human endowment with what was evident only at birth [1] One of the socially most important of these extra-uterine developments is that of smiling, which generally begins at around six weeks (Jolly 1972 237).[2]

The significance of smiling is that it indicates recognition by the infant of another person, usually the mother, and marks a major step in his assumption of the initiative in *attachment behaviour* [3] Prior to the development of smiling the infant can do very little to establish and sustain attachment, apart from crying and sucking In the beginning, therefore, it is the adult, the mother who takes the initiative in establishing the attachment or *primary bond*. It would not, then, be too far out of line to conjecture that there is a biological basis in the adult human female for attachment behaviour which is triggered by the birth of the infant. And that accompanying such innate features are certain correlated *emotional* consequences. For example, attachment is reinforced by breast feeding and breast feeding is known to bestow considerable emotional gratification on the mother as well as the infant. In more general terms I want to suggest that in the establishing of the primary

bond it is not simply that the infant becomes dependent on the mother, but that the mother grows into a behavioural and emotional dependency on the infant, the object of her attachment.[4] The importance of this possibility can be appreciated if we suppose the case of the growing child whose maturity leads him to seek independence and the ageing mother who proves unable to forego her dependency.[5]

There is clear evidence of attachment behaviour among primates. Among rhesus monkeys, for example, the mother will pull her young infant by the tail if he shows signs of wandering off. And the same is true among macaques. But it is equally evident that among primates this attachment behaviour is not sustained as it is among humans, for after about twenty weeks or so the primate mother no longer insists on restraining her infant, and the bond between them starts to attenuate (Jolly 1972. 211–12; Rosenblum & Kaufman 1967). Only among chimpanzees, it seems, does a relation between mother and offspring persist into adult life (Reynolds 1968, quoting van Lawick Goodall 1967)

The emotional character of the primary bond of attachment is further reflected by the forms of communication Here the human mother and infant most closely resemble other primates, for communication is most importantly by touch: nursing, cuddling, grooming, kissing, stroking, ticking and smelling Now of all the senses touch is the most immediate, sensitive and delicate. It is by touch that the autonomy and individuality of the organism is taken away while at the same time two separate organisms are most sensibly joined Touch is the most ambivalent of all senses because it removes autonomy and institutes attachment and because it is the primary expression of both love and fear In the human situation the establishment of the primary bond through touch is homologous to and continuous with the innate attachment shown, to varying degrees, among all primates. But touch is not the only mode of communication. Auditory communication by crying, cooing, gurgling, babbling and lip smacking; visual communication by smiling, eye movement, facial expression and gesture and olfactory communication are all important. Early human communication then has the same complexity as non-human primate communication it takes many forms, or is *multi-modal* (Washburn *et al* 1972. 349) and it works by sign rather than symbol or is *analogic* (Bateson 1973: 343). What is communicated, what is meant by the information conveyed in non-human primates and in the human primary bond is, above all, the *emotional* state of the organism and these forms of communication have been shown to originate in the same limbic areas of the brain that generate the primitive drives (Washburn 1973: 129). This structure and its behavioural consequences remain in operation throughout the life of the human being while other structures, such as those for the capacity to reason or to use language are added on. This limbic system, or syndrome of drive, signalling and emotion is not only permanent, it may be regarded as the central, core genetic structure of behaviour and of evolution (cf. Popper 1972· 273).

Now let us turn to fathering behaviour Among primates there is considerable variation and certainly nothing of the constancy and continuity of mothering behaviour. This variation suggests that we must be a little more cautious in affirming continuity between primates as we may be dealing with analogy rather than homology. This is a caution that needs to be more carefully observed in all

evolutionary studies than it has been hitherto (cf. White *et al* 1973; Atz 1970).[6] The marmoset male, for instance, displays exaggerated paternal care, carrying around twin infants on his back until their combined weight equals his own, handing them over to the mother only at feeding time (Jolly 1972: 247–8). At the other extreme, the hanuman langur almost completely ignores the infant (Jolly 1972) Between these two extremes there are many variations, but it seems to be a fair general conclusion that most primate fathers could not claim their children (Jolly 1972). This means that we cannot speak of a primary bond between the non-human primate adult male and infant. Connexions between males, though, may come about through the medium of the primary bond, as when among Japanese macaques it occasionally happens that a young male of a high ranking mother may gain dominant status without having to fight for it (Reynolds 1968) Otherwise though, it seems generally true that male relationships come to be based on dominance and submission and on friendship, with adherence being achieved by defence against predators But even male alliance seems not to be sustained or exclusive, for among apes and monkeys males wander more or less freely, attaching and detaching themselves from bands

The relation between adult males and females among primates is, on the whole, a generalised one of protector and protected Mating may be quite promiscuous, as among chimpanzees, or relatively stable, as among the harem groups of hamadryas and gelada baboons Among the latter, sub-adult males are described as showing *quasi maternal* (rather than paternal) behaviour towards infants, especially young females. These are more or less adopted until they achieve sexual maturity and become part of the procreating harem group. This should not be mistaken for fathering behaviour, for upon reaching maturity 'several females are conditioned to follow one male at all times. Females wandering away are brought back by the group leader's neck bite'. The hamadryas harem leader's relations to the infants he sires seem, basically, to be the same as that among other primates—protector and aggressor (Kummer 1967 65).

One of the major changes that has occurred between humans and non-human primates is the loss of the estrus cycle in the former This means that human females remain sexually receptive year round and throughout the menstrual cycle Although Fox has argued that there are heightened periods of receptivity, and that this represents a continuity of estrus 'at the profound level in the human female' (1972: 302), the difference of degree remains a fact What this means is that sexual activity could be the basis of pair relating comparable to the intensity and durability of that among humans but possibly that in neither is the pair relationship comparable in intensity to that of the primary bond. Non-human primate females are not likely to be in estrus for more than twenty weeks of a twenty-year life span and often for not more than two weeks in two years. Even in a promiscuous group of chimpanzees sexual abandon is mitigated by the fact that estrus is seasonal and lasts for only three or four months of the year (Jolly 1972: 199–201). In any case promiscuity is the negation of friendship, kinship, status etc. There is a further difference between non-human pair relating species and humans, which is pointed up by Reynolds, who says 'that the pair lives (analytically) in opposition to others, as an independent unit, or within a community of similar units but without joint interaction with other pairs' (1968: 219). In other words, other species have

developed *only dyadic relationships*, which are closed, immediate and instinctive. The point of importance at present is that there is certainly evidence for the loss of estrus in human females and the concomitant extension of receptivity. This provides a basis for the development of a lengthened pair relationship which, though not different in kind from that of other species, is different in degree. And it is this extension of relating that, through time, paves the way for something new to happen among humans

In man there are two elementary relationships: a lengthy and intense primary bond of attachment, and a continuing sexual pair relationship. Each relation, considered separately, is a dyad, and as such is perfectly within the continuity of primate evolution As Simmel argued, dyads possess only the qualities of mortality, immediacy, triviality, individuality, and above all, emotion, both positive and negative (Simmel 1950: 126). At this level, as I have already pointed out, communication is multi-modal and analogic, and there is little to indicate the nature of the social discontinuities between man and other primates. It is only if we realise that in man both relations are *extended further in time* than is the case among other primates that the conditions exist for the establishment of a *relation between the primary and the pair relationship*, or what we might identify as the meta-relation. It is at this point that hitherto separate dyads can become interconnected and thereby transformed, in the first instance, into triadic relationships And it is only with the advent of triads and meta-relations that the structures we know as groups, societies and so on can come into being.

These structures, which make up human society, are not natural phenomena but are, as Hume calls them, conventions So too is the recognition and consciousness of time a 'convention' and not a natural phenomenon. It seems to me therefore that previous speculations about the origins of human society coming about through the division of space or territoriality are only partially correct, for cognition of space is evident in a vast number of species, primate and non-primate, while the advent of time seems far more a human invention. But this advent arises as an accident of evolution, namely the conjunction of the continuity of the pair relation and the duration of the primary bond. This conjunction makes for circumstances of co-operation and competition, for neither relation can sustain itself in isolation from the other, as it appears to do among other primates. We can explore this matter of the temporal organisation of human meta-relations in connexion with what we know of the earliest human ecology and economy and consider its goodness of fit.

III

Some form of this triad would seem to be the minimal *social* relationship and it is of considerable interest that in describing the Hadza, a hunting and gathering society of Tanzania, James Woodburn has recorded a society whose social cohesion seems to be at this minimum The Hadza are strikingly uncommitted to each other, do not depend on relationships for particular access to property and the like, are constantly changing their residence and their neighbours for no other reason than 'desires and whims of the moment' (Lee & DeVore 1972: 91, 107). They frequently come into conflict with each other, and use any excuse, mainly ecological,

to get away from each other Another hunting and gathering society, the Ik, share these characteristics and live in what Turnbull calls a constant state of flux, where they too are uncommitted to place or person (*ibid*: 135). And while the Mbuti pygmy net bands may live together for most of the year they too disband and re-assort themselves as they see fit and as suits them individually.

In the light of my subsequent argument, these characteristics of some African hunting and gathering societies are extremely interesting Thus, among the Hadza, while there is no permanent social grouping, there is a certain social stability which is based on a man's relation to his mother-in-law, for where she lives there, in most instances, will he live. Woodburn singles this out as the crucial relationship among Hadza, and notice how it is constituted · it is a relation between the relation of mother/daughter and the relation of wife and husband, it is, in other words, the relation between the primary bond and the pair relation (Lee & DeVore 1972: 109).

Another interesting feature of hunting and gathering societies which has some bearing on the present argument is that the recurring fission and fusion of people constantly changing their neighbours is for the most part the result of tensions and conflicts that build up between people when they are living together. Among the Mbuti, Turnbull thinks such tensions are for the most part the result of anxieties built up over marital problems, where brother-sister exchange marriage leads to some conflict of loyalties, or, more generally 'they represent simply the great internal tensions which build up during a period of hunting under very close conditions' (*ibid.*: 156) All these hunters and gatherers strike their observers as being very emotional people, constantly given to quarrels and fighting, though not necessarily with malice, as well as given greatly to laughter (*ibid* : 91). Now, hunters and gatherers by definition are free to roam within the bounds of an extensive range They live in an open, often benevolent environment which offers few barriers to people who wish to move around and away from those with whom they have some quarrel. Hunters can, with very little difficulty, separate from each other, and can very easily join with each other And though they may separate from each other, the ease with which they can do so suggests that such separation is not necessarily a severance, that at some future date those who separate will find little difficulty in rejoining each other.

It seems as if relations over and above the core relations are kept in some sort of equilibrium by varying the degree of presence and absence, thus ensuring some security for the future by keeping a 'wide range of strategic social ties warm' (*ibid* 155).

I would like to emphasise this co-existence of certain traits in hunter/gatherer society that such peoples are said to be very emotional, showing immediate propensity for laughter and crying. that they quarrel frequently among themselves, that following the outbreak of quarrels bands break up and social organisation appears very fluid, that there is but a narrow range of commitment of people to each other. Turnbull in his discussion of Mbuti, Ik and Hadza then makes another observation that appears to fit this cluster of traits. He says: 'these hunting communities are characterized by an almost complete lack of magic, witchcraft and sorcery' (*ibid.*: 91). Indeed, as he has so well described elsewhere the Mbuti pygmies not only lack such institutions, they openly deride them when they are

practised among the settled Negro tribesmen with whom they are affiliated (Turnbull 1965). The implication is that a settled community (agricultural, pastoral or industrial), one whose inhabitants are *not* free to separate from each other, is a community which may develop specific ways and means of resolving conflicts that arise through the oppression of social relations. These resolutions, while they may vary in cultural form, might all be said to be aimed at the same point: to effect some form of separation between people living in constant relation to each other The general mechanism by which this is done is taboo, and we know that by and large the advent of magic, witchcraft and sorcery, together with divination, is in part a reflection of the failure of people to observe taboos. A victim of magic or witchcraft is one who has all too often failed to observe a taboo, or one who is the victim of someone else's failure. Or he is the victim of ill will, of the covert ill feelings that, in a hunting and gathering society, would only briefly remain hidden and would normally flare up immediately and in the open.

The nature of some hunting and gathering societies suggests that they enjoy a social freedom, permitted by the perceived benevolence of the environment in which they live. The essence of this freedom is that social relations can be easily and painlessly disengaged as soon as strain erupts. There is no need for cultural artefacts such as a battery of taboos to serve in lieu of this necessity At the same time, while such societies are fluid in their social structure, they still express a sense of society and this is based on the existence of first order meta-relations, relations between relations or triads Not even the Hadza, who see social relations as simply expedient and who lack interpersonal commitment, live as individuals or entirely in discrete dyadic relationships with each other

There is yet another important factor of hunting and gathering societies that has clear bearing on our understanding of the evolutionary logic of human society. This is the matter of sharing (cf. Goldschmidt 1966: 87 sqq) Certainly it is true that not all kills are actually shared but may be consumed on the spot by the successful hunter(s), but, if we may instance the Hadza again, a society where people evidence very little commitment and responsibility towards each other, we do find that 'the meat of a large animal is by custom widely distributed through the camp and people tend to congregate in camps where there are skilled and successful hunters' (Woodburn in Lee & DeVore 1972: 106). Although some hunting, especially with bow and arrow, may be done by individuals, much hunting among African peoples is co-operative, involving nets, tracking and trapping Whenever the hunting is co-operative, the catch is shared If the social organisation of contemporary hunters and gatherers reveals the skeletal 'structure' of social relations, then it is possible that we can think of the economic activity of sharing as revealing the elements of the economic infrastructure that allows for the development of more complex cognitive and social structures.

There is little doubt that the earliest prehistoric human populations were hunters and gatherers—as indeed are the non-human primates, most of whom are simply gatherers. With the exception of the periodic sharing observed among meat eating chimpanzees, all non-human primates gather and feed as *individuals* (Teleki: 1973).[7] Not even a mother shares the food she has gathered with her infant (Jolly 1972. 55–69, 86–7; Washburn & Lancaster 1968). Non-human primates clearly show that hunting, and especially gathering can be carried on without

sharing, but human hunters and gatherers are distinguished by the fact of their *co-operation and sharing*.[8]

It has been suggested that hunters manifest a certain disdain for the morrow, the future This may either be an optimistic attitude, premissed on the belief that there is no need to worry about tomorrow because the environment is so bountiful. Or it has been interpreted as a pessimistic attitude, that fate will take care of tomorrow anyway (Lee & DeVore 1972: 89–92). Whether this is true or not it has certainly struck observers that there appears to be a minimum concern for the future in a general sense But if sharing is a feature of hunting life, then I would like to suggest that the basic elements of knowledge and conceptualisation of the future are certainly present, just as the telling of myths testifies to a knowing of the past. And hunting and gathering peoples such as the Australian Aborigines may well be classed as among the most knowledgeable of historians (or mythstorians). As with the singling out of the elementary structure design of social relations, so here I am concerned to argue the development among hunters of the elementary design of a cognition of the future, out of which the more elaborate complexes of time associated with settled agricultural and industrial people have evolved And while it may be risky to extrapolate categorically from contemporary hunters to prehistoric hunters, yet we have no other choice and given the ecological situation of such peoples, we must recognise such factors as the operation of social relations and the conception of a place in time if we are to account for the known stability of such peoples

Perhaps the first point to make about sharing is that in societies such as those of hunters and gatherers it does not constitute altruistic behaviour but it is obligatory behaviour. It was the power of compulsion in the gift, or in sharing, that struck Mauss so forcibly and sharing has the status of a rule in such societies. Lorna Marshall, for example, notes that among the !Kung Bushmen 'altruism, kindness, sympathy or genuine generosity were not qualities which I observed often in their behaviour' (Marshall 1961: 231–49). In fact other observers, such as Woodburn of the Hadza and Turnbull of the Ik have emphasised the highly individualistic behaviour of these people.[9]

Whatever is to be shared must obviously be partible, but beyond that sharing is a matter of time A single object can be broken up to be present in several places at the same time, or, in an extended period of time (through exchange) several objects can be fused into one. Both these features of sharing are brought together by exchange and reciprocity, which occur in the context of some sort of collective. In a hunting band, for instance, not only is an animal divided up amongst the members on one particular occasion (with those who actually killed the animal having to forgo a large part of the meat), it is expected that on any *future* occasions the killers will receive meat from a kill made by other members of the band. The animal is, in a sense, put together again. Sharing is not altruistic so much as it is a method of reintegration through time, through postponement, and incorporates an idea of the future of one's own condition.

Sharing and the promise implicit in sharing indicate rational cognitive activities. But at the same time it can be adduced from ethnographic descriptions of institutionalised sharing that it is closely bound up with the expression of emotion. We are perhaps more accustomed to think of gift giving as blessed, a means to

make us feel good by making recipients feel good. But as Lorna Marshall makes very explicit, among the !Kung Bushmen the giving of gifts helps them to avoid situations that are likely to arouse ill will and hostility. This, as I have suggested, seems to be confirmed by the mirror image of the Ik, who exhibit the utmost ill will and hostility to the point of not caring As Sartre has pointed out, to withdraw reciprocity *is* evil—and that is how the Ik strike one. The Bushmen, on the other hand are depicted as pre-eminently peaceful. If we consider such elaborate systems of sharing as potlatch or kula we can see magnified the close and complex weave of a socially rationalised institution with the fulfilment of emotions, especially negative emotions in which anxieties and tensions arising out of the social co-existence of people with each other, are resolved through sharing, through the sustained tension of making and fulfilling the promise—for in the gift a tension is simultaneously satisfied as another is generated, but it is diverted and embodied in objects rather than people. Bushmen exchange meat or arrows, the Kwakiutl blankets and coppers, the Trobrianders shells and necklaces and the Ik—people.

In sharing and the promise, we have one of those human activities where reason and emotion meet in an especially transparent way and where the public institution that binds a society is at one and the same time a private phenomenon that wells within the individual Thus it is possible that what can be seen from one angle as a sociological institution (the gift) can also be viewed without distortion as a factor of individuality. Yet, the individual does not share 'naturally'—it is the obligation to share that counts and the individual obeys this obligation because in some way his private emotions are fulfilled Sharing then is one of the few phenomena that cannot be understood unless it is examined in this way, as having an origin in whatever it is that is *both* individual and social. I suggest that the foundation of sharing lies in the connexion between the primary bond and pair relationship.

IV

Continuing sexual attraction and extended attachment creates the conditions for a relation between primary and pair relations which gives rise to possibilities for organisation and extension. Each of these relations is dependent on the other for their own separate realisation If, for example, the relation between mother and infant is to be fulfilled momentarily, then the pair bond and its demands must be *suspended for the time being* Conversely if it is the pair relationship that is to be fulfilled, then the primary bond must be held in abeyance for the time being. For the first time in primate behaviour emotions must be *voluntarily* postponed and gratification delayed. This, it would seem, is the first instance of self-control, of the willing of obligation through the surrender of rights in oneself to others.[10] The meta-relation is instrumental in the evolution of the control over the emotions and thence the growth of rationality. But it must also be seen as a circumstance in which new objects of emotion are engendered, which in turn will require control. For the design of the relations between relationships makes it necessary that those who at any moment are *attached* must at some time submit to a *separation* from each other that other attachments may be formed.

Now separation, especially of an attachment figure from an infant is a condition

that John Bowlby has convincingly argued is a primary source of *fear*, such a fear being 'an instinctive response to one of the naturally occurring clues to an increased risk of danger' (Bowlby 1973: 86). Such fear is, in turn, a principal source of anxiety, even though objectively the separation may not be dangerous. For separation actually means the inaccessibility of the attachment figure to the subject (1973: 23) so that, though the mother may be in view, she may be emotionally unresponsive perhaps because her attention is elsewhere, and the child, from his point of view, is involuntarily separated. Though separation anxiety is most acute between attachment figure and child, it is by no means confined to this relationship and may be felt between any two persons who are attached and who have to undergo separation, e g members of the pair relation. Though obviously not the only source of separation, the temporally conflicting demands of a co-existing pair relation and primary bond must be a fundamental and persistent source of potential separation The full accessibility of a mother to her child is incompatible with the full accessibility of a woman to her mate. Such inaccessibility is particularly fraught during the first year or two of an infant's life when he is unable to communicate or comprehend symbolically and is therefore unable to displace gratification in the knowledge of the future

The relation between primary bond and pair relation means that, in performance each must take account of the other. There must be *deference*, a suspension at any given time of at least some of the demands of one for the sake of the other At the same time the performance of any one bond must contain some *reference* to the other The individuals caught up in these related bonds must both defer and refer to each other if they are to gain maximum satisfaction

This, however, is an ideal situation and there is no reason to suppose that in countless instances of human existence this ideal is not approached. But these demands of deference and reference, which in behavioural terms require the willing delay of gratification are often so taxing that the imperative may be regarded as extremely onerous Nevertheless, the wellbeing, if not the survival of the individual as well as the survival of the meta-relational design that forms the basis of society is undoubtedly jeopardised if deference and reference are not observed Because this is so, and because the situation is a conventional one, an artifice, I venture to suggest that it was quickly marked and reinforced by other artefacts produced by the mind The two principal artefacts, which though distinct in contemporary thought appear to me virtually complementary, are *taboo* and the *promise*

The unique 'design' of human co-existence, the relation between primary bonds and pair relationships, brings about a situation between individuals which requires the deference of emotional demands and the reference of desires to another who is independent of the source of gratification. We are thus imputing to persons cognisance of themselves in the *future*, within which is entailed remembrance of the past and appreciation of the present The situation is also *mutual* in that each must recognise in the other a source of future performance and see himself as an agent of the other's future satisfaction. We therefore have *expectation* and trust.

We have now set the scene for the realisation of certain of those features of the mind that we know to be uniquely and importantly human without having to invoke or to suppose any mystical teleological capacity The relation between primary bond and pair relation is an accidental and external circumstance whose chief

consequences are the materialisation of the future and the supra-personal. I am here arguing that such a materialisation is the very germ of society and I am hinting that it was the spark that set off the development of human language, once the speech capacity of the brain was sufficiently developed (cf. Hockett 1960: 4).

V

What I have led up to is the proposition that the discriminating features of human behaviour may be summed up in the notion of the promise, for it partakes of all the factors I have spoken of

Hume considered the performance of promise to be one of the three fundamental laws of nature (the other two being stability of possession and transference by consent) (1888: 526) He argues that the promise is a human invention founded on the necessities and interests of society and is not a natural property of the mind (1888: 519) I have confirmed that it is not natural and have specified the preconditions of its formation. It does not grow out of the biology of the individual, but comes about accidentally as a consequence of the connexion between two biological factors. Hume notes that the promise always regards some future time (p 516) and reflects an interested commerce in the form of words (p 522). I have hinted that words and the concept of the future may be at least correlated in their origin and that the promise embodies the deference and reference to others in the future that I have discussed For it is only thus 'we might give each other security of our conduct in any particular incident' (p. 522) But this creates a new obligation which is not natural but artificial · 'We *feign* a new act of the mind, which we call the willing of an obligation' (p 523) or, to put it another way, the endless self-propagation of morality, culture and society is set in motion.

Hume confirms that the promise is a very elementary matter for 'there needs but very little practice of the world to make us perceive all these consequences and advantages The shortest experience of society discovers them to every mortal' (p 522). Again, we can now place this in evolutionary terms as an outgrowth of a very primitive, but uniquely human circumstance: the design of a meta-relation. But though expressed in words, the promise, to be effective, depends on the intent and this, together with the sanction, can be seen to be located in the postponement of self-gratification as the least destructive form of self-interest.

The sanction of the promise is the withdrawal of trust · 'when a man says he promises anything, he in effect expresses a resolution of performing it; and along with that, by making use of this form of words, subjects himself to the penalty of never being trusted again in case of failure' (p. 522). This is tantamount to the denial of reciprocity, the cessation of deference and reference and the destruction of the basic relational design.

Whether the promise was ever first conveyed in words, or even whether it is universal as verb or noun can never properly be known (cf. Needham 1972: 149). But this does not matter. What is at issue is the concept of future behaviour and the conveying or communicating of such a concept as an assurance, plus the means of ensuring. Functionally speaking it can be suggested that what would happen if the promise were neither recognised nor fulfilled is that the basic design of human social relating would collapse as immediate emotions would demand fulfilment and in so

doing would diminish the chances of the survival of *individuals* (particularly infants) as well as *groups* But since the design upon which this hinges is itself accidental, being neither directly of biological origin nor of tangible environmental origin, it is doubtful whether its imperatives could ever be directly perceived by those enmeshed in behaviour

These mysterious qualities associated with the promise (and with taboo), were noted by Hume, who explains it thus. 'Every new promise imposes a new obligation of morality on the person who promises, and since this new obligation arises from his will, 'tis one of the most mysterious and incomprehensible operations that can possibly be imagined, and may even be compared to *transubstantiation* or *holy orders*, where a certain form of words, along with a certain intention, changes entirely the nature of an external object, and even of a human creature' (p 524). The individual, in pursuit of his self-interest may well lose sight of the interests of others and thereby his own dependency on the well-being of others' self-interest. This can happen most easily when relations between individuals are most intense and immediate, as between members of the pair relation and primary bond.

It is the well-being of individuals in relation to each other that is guaranteed by the promise, but is is also the persistence of social life that is at stake. Thus the efficacy of the promise is a matter both of the integrity of the person, whose word is his bond, and of the publicity of the promise. The performance of the promise is backed by a similarly private and public concept, that of taboo If the promise is an injunction to perform in the future, then the taboo is an injunction not to do that which will destroy the promise. Taboo marks out danger, to which individuals may be exposed by separation, or it ensures separation when attachment may prevent it Taboo points to mysterious sanctions, external to human beings, by which promises may be kept, almost in spite of the individual himself, as if emanating from a transcendent or intrinsic power. Not to do is the essence of taboo. to do is the heart of the promise

VI

I have argued that among those primates that we call human beings the primary bond extends over a longer period of time than among other primates; and the pair relation, possibly correlated with a different pattern of sexual receptivity, is also more extended and possibly more intense. While each relationship is biologically determined and displays adaptive advantages, their co-existence results in an unforeseen and non-biological factor—the relation between the two relations, which I call the meta-relation, or the relational design. Bonding relations simply confirm the continuity of man with other primates and reiterate his biological nature in general, for bonding is to be found in numerous non-primate species. The extension of human bonding, an apparently minor difference of degree, may be seen to raise a new *possibility* in man

It is the *possibility* of a meta-relation that is important and what my argument is about. It does not matter whether in some instances it can be said that since infants sleep most of the time, they constitute no impediment to the sexual and emotional demands of the pair relation It is enough that there is the *possibility* that in the human condition the relation between primary bond and pair relation necessitates

the *regulation* of activity and feeling. This is a possibility that is not only phylogenetic, but ontogenetic as well, i e., it is a universal human possibility whose existence must be recognised. The eventuation of this possibility is, I argue, the beginning of systematic or reasoned relations between people, i.e., social structure, which concerns people as well as people in relation to objects. In social structure, if you like, is the origin of self-consciousness. Again, my argument points out that this possibility arises from the *conjunction* of biological facts rather than from the facts themselves, and so has a different sort of autonomy from biological facts. This conjunction, or overlapping, is a possible threat to the facts of bonding and the subsequent structure, the meta-relation is concerned with *separating* the bonds. Social structure then is the structure of separation in the first instance. At the same time it is the imposition of control, self-control, on the emotions and instincts. Simultaneously the possibility of the meta-relation realises social structure, or the interlinking of relations, and it compels man to reason about himself.

Although the capacity to reason must be dependent on the biological development of the brain, the brain must have something to reason about For the brain to develop successfully we must assume such development to be in adaptation I suggest that the capacity to reason comes in response to (or perhaps is co-terminous with) the advent of the possibility of the meta-relation and its demands which requires the postponement of emotional or instinctual gratification. This means that the relational design is also the design of reason itself: that social structure is the structure of reasoning to which the mind has adapted. In this case the structure of the mind is not so much an unconscious structure as it is an adaptation to a structure *of which it has remained unconscious* That this is likely can readily be appreciated from the fact that the relational design is a meta-relation which is neither the product of mind itself, nor an object that could provide a sense datum. It is the unseen and unforseen consequence of the extended relationship between persons.

If the meta-relation and its demands are simply a possibility it might be said nothing can be drawn from this unless it becomes an eventuality. How can we lay social structure and reason at the feet of a mere possibility? All I can say is that the weight of the evidence shows that from the existence of this possibility man's realisation appears to have followed. That his social structure and the self-imposed imperatives that govern his life in society all bear witness to taking into account the possibilities of his situation

To illustrate this I have discussed the promise, which I instance as the primal realisation in reason and social structure (i e , culture) of the possibility of the meta-relation. The original promise lay in the agreement to postpone the gratification inherent in one relation for the fulfilment of the other But the conduct of my argument is open to some objection, since I have used contemporary ethnographic evidence about hunting and gathering peoples By no stretch of the imagination can a contemporary Bushman or Pygmy or Australian Aborigine be taken to stand for a primordial ancestor of the human race But this is not my intention, nor is it even my intention to let contemporary hunters and gatherers represent the mode of existence of prehistoric man, although it is quite likely that palaeolithic man was a hunter and gatherer, even if he might also have been a gardener and a herdsman. No, the importance of the hunting and gathering mode of

existence is that of all human modes of existence it is the least encumbered, it places the fewest obstacles and the most permeable barriers between man and nature and between man and his own nature. As Stanner, speaking of the Australian Aborigines, has put it: they are the least materially minded and the most life minded of people. Those who are hunters and gatherers need be neither primitive nor civilised, neither simple nor complex. But from the perspective from which *we* think and live, they lay bare for us the elements of being human. And amongst these, the most life minded of men, I find the promise to be fundamental to their social existence It is but the development in reason of the meta-relation.

Reason and social structure are then one and the same with each being the exemplification of the same existence social structure and reason are founded on the meta-relation. When social structure breaks down, reason breaks down: when reason breaks down social structure breaks down. But we must look for what goes wrong to the condition of the meta-relation. In other words, in the aetiology of mental illness, which implies a break-down of reason, we must look for the cause not simply in the mental or bodily condition of the individual The mental illness of individuals may be a consequence of the malfunctioning of the relation between the relationships of persons Similarly when the group, or the community or the society seems to break down we must look for the aetiology in the condition of the meta-relation, the condition of separation between relationships that is the basis of any social structure [11]

His existence in a meta-relation gives a man the knowledge of his own being by subjecting and conditioning his being to the being of others This applies equally to man the individual and to man as society or community. Man's knowledge of the meta-relation is embodied by him, and for him, in the forms of the imperative we now call the promise and the taboo. These are the objective knowledge or re-presentation of the meta-relational design They constitute the 'minimum shared presupposition' that allows isolated, individual, instinctive men to understand one another and this is how synthetic *a priori* judgements are possible (cf. Goldmann 1972 152) From the possibility that arose by accident out of his evolution, man had to develop his reason and then to face the danger of losing his reason With his reason he has a sense of being, without his reason he suffers from an absence of a sense of being It is in this sense, surely that we understand Hobbesian man to have been living in a state of nature, a state to which we fear a return. For there is no suggestion that Hobbesian man was biologically unsound, only that he is socially unsound no suggestion that his emotions are invalid, only that he has no reason

NOTES

A first draft of this article was written and presented to a seminar at the Australian National University I am most grateful to Professor J D Freeman for his invitation to Canberra and to him and members of the Department of Anthropology for their stimulating company and criticism I should also like to thank Dr Paul Wilkinson for some most helpful criticisms

[1] Locke saw perfectly well the significance of infant dependency, but misconstrued it as evidence of the human being as a *tabula rasa* (in Barker 1947 66)

[2] There is evidence of a neuro-anatomical basis for smiling present in the fourteen-week-old foetus (see Humphrey 1969).

[3] Attachment behaviour is a convenient term developed principally by John Bowlby (1971). It refers to behaviour directed from one organism to another and back again which forms and reinforces the primary bond between mother, or mother figure and infant

[4] Hamburg mentions, but does not develop these emotional consequences (in Washburn & Jay 1968). It can, of course, be said that some mothers show little or no signs of attachment. But most human cultures and individuals would condemn such mothers.

[6] I have attempted to show how this possibility may lie at the root of certain sorts of mental illness in Wilson n.d.

[7] One of the dangers in adducing the evolution of human behaviour from primate evidence is indiscriminate comparison often arising from the confusion between analogy and homology. All too often traits are lifted out of context and the resemblance between primate and human in one respect is emphasised while ignoring the vast overall difference. Thus the hamadryas baboon resembles man quite closely in his conjugal harem habits, but overall he is an outstandingly non-social animal This one striking resemblance may be fortuitous and have no evolutionary significance The chimpanzee resembles man most startlingly in his hunting and group organisation, even to the extent of a form of sharing and co-operation. But his sex and family life is entirely promiscuous—unlike the hamadryas baboon, and man. It is in the cross-sex comparison between men and animals that the loosest arguments for a 'natural heritage' for man occur For example, it is well established that birds have intensive and durable pair bonds and that they protect themselves by defining a space This space is then called 'personal space' and its existence used to show a natural basis for man's need for privacy Similarly the innate, life-long pair bonds that may be established between birds is often taken as 'evidence' for the natural basis of human marriage! But while it is true that birds bond, their evolutionary distance from man makes it sensible, if not necessary, to see the occurrence of bonds in both species as simply an analogy. Both are alike in this respect, as they are because they are two-legged But there are many primates, the order of which man is a member, which do not have pair bonding

[7] Teleki (1973) points out the possibility 'that predation developed among primates long before the advent of ape-man Many primate species have evidently shared a number of adaptive features for some millions of years Might not the practice of preying on mammals, and on other primates in particular, be one of these features?' I regret that, at the time of writing I have not yet had access to Teleki's fuller account of chimpanzee behaviour

[8] Of course our most reliable evidence is from contemporary hunters and gatherers, from whom we can make inference, risky it is true, to prehistoric populations But archaeological finds are themselves subject to interpretative limitations which restrict the possible inferences that can be made about the *behaviour* of makers of artefacts I quote here an example of what may well be the earliest artefactual evidence of human contractual behaviour

> Occupation sites containing stone artifacts and introduced broken up bones probably imply significant evolutionary intensification of certain behaviour characteristics, *which may occur sporadically in non human primates but which become especially prominent characteristics of Pleistocene Hominidae*. These are manufacture of tools, hunting or scavenging (or both), *food sharing*, and the organization of movements around an archaeologically recognisable home base (Isaac 1971, my emphasis)

I am grateful to Professor Charles Higham for bringing this article to my notice

[9] The Ik perhaps constitute a special and very disturbing case of a people disintegrating. Nevertheless for all the shocking callousness described by Turnbull it is possible to view their behaviour as being what gift giving is all about Under the social rules of sharing each individual is obliged to give to another whatever comes into his possession Nothing 'belongs' to him The Ik consider it 'proper' simply to take, by force if convenient, anything from anyone Again Ik end up owning nothing they produce—unless, as seems possible, they manage to hide it in the caves (see Wilson 1974)

[10] It is obvious, I hope, that when I use the term pair relation I do not mean marriage but simply a relationship stemming from mating, or perhaps more specifically a fertile mating Ernst Mayr (1970) has recently suggested that polygamy may have preceded monogamy, thus not only introducing full-blown cultural institutions into palaeolithic life, but also reviving earlier evolutionary theories Even if we admitted marriage as opposed to mating, the argument for polygamy is difficult to sustain, for although it reduces tension between primary bond and pair relationship there is plenty of evidence to show that pluralism in sexual relationship raises new conflicts between co-wives or co-mates (see for example Lewis 1966; Wilson 1967) What is more Lévi-Strauss showed a long time ago that the ratio of the sexes would make a universal primordial polygamy very difficult to achieve (Lévi-Strauss 1960) It seems rather more commonsensical to see the rapid increase in breeding after the increase in brain size as a means of control which ensured a greater survival rate. Here such devices as the incest and

post partum taboos are clearly of the greatest demographic, as well as ritual importance (May 1970 386).

[11] I have developed this argument at great length in Wilson n.d.

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CASH, CREDIT AND GAMBLING IN A MODERN ESKIMO ECONOMY: SPECULATIONS ON ORIGINS OF SPHERES OF ECONOMIC EXCHANGE

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Where lies the rationality in ideologies of spheres of economic exchange? An examination of the economy of an isolated Canadian Eskimo community suggests that the discontinuities commonly reported in systems of commodity exchange could arise and persist through strategies of hoarding Thus, a quite reasonable decision to reserve cash exclusively for the purchase of certain unpredictably available luxuries, generates, in this Eskimo community, a fairly impermeable barrier in the flow of goods and services luxuries and the cash that secures them are set apart from the mundane goods bought on credit from the co-operative store: conversions between the 'cash' and 'credit' spheres so formed are then only regularly achieved through the pooling of disparate assets in gambling Upon examining the scarcity and social usage of certain of the commodities from selected primitive economies, one speculates that similar strategies may underly at least some of the spheres of exchange ideologies of other societies

I

As is well known, discontinuities in the flow of goods and services are common in primitive and peasant economies. Usually dubbed 'spheres of economic exchange', these discontinuities are of a substantive and contextual variety that has impeded generalisation and promoted divergent perspectives of discussion. Thus, Firth (1939) and Barth (1967) point to different standards of evaluation among the spheres of certain economies; the Bohannans (1968) focus on ranking among spheres, and on 'conversion', the exceptional exchange of goods and services from different spheres; Salisbury (1962) and Douglas (1967) relate spheres to aspects of social structure and status, and Barth (1967) shows that prohibitions on the direct exchange of a few goods or services can imply discontinuities in a far wider economy.

Recently, however, Ortiz (1973) has urged investigation of the rationalities of spheres of exchange ideologies. She thus speculates that the Tiv's disapproval of the trade of marriage wards for cows or brass rods may well relate to the 'advisability of exchanging one scarce factor of production for another readily available factor' (1973: 3; cf. Bohannan & Bohannan 1968). With her own Colombian material, Ortiz shows that Paez Indian disapproval of selling subsistence goods in the market is not rooted in tradition, it relates, rather, to a strategy for self-sufficiency in these vital commodities, and hence, particularly, for maintaining a low cost of food (Ortiz 1973: 5, 7). And though, strictly, the Paez economy is without spheres of exchange (1973: 238), one can certainly accept that the protection of the subsistence sector is a likely basis of the spheres of exchange ideologies of many peasant economies [1]

But in this article I present data that illustrate the genesis of spheres of economic exchange from a converse strategy: one in which individuals attempt to sustain

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and control limited supplies of 'special commodities', those required for the purchase of other valued or 'special' goods, or for payment of skilled services, or for presentation on important social or ceremonial occasions. It will be my suggestion that similar strategies underly the origin and, in part, the persistence of spheres of exchange in a number of primitive economies.

The article is in two main parts. In the first part I analyse the emergence of two spheres of exchange within the market economy of a contemporary Canadian Eskimo village. These spheres, the 'credit'and 'cash' spheres, are each associated with specific 'nexuses of activity' (Salisbury 1962). A third nexus, the 'gambling' nexus, importantly provides a context for regular conversion between the credit and cash spheres. In the second part I draw parallels, in terms of scarcity and social usage, between the goods and services of the Eskimo cash sphere and those goods and services of the non-subsistence spheres of exchange among the Tiv, Tikopia and Siane Finally, having speculated on the rationality of the spheres of exchange ideologies of these three well-known primitive economies, I conclude with brief assessments of some other explanations of spheres of economic exchange.

II

In 1959, the Eskimo economy of Port Burwell, a once-thriving, though long abandoned outpost of European agencies in the Canadian east arctic, was rejuvenated A federal government plan for the regional development of Ungava Bay in northern Quebec (Evans 1968) had emphasised the excellence of the natural resources around Killinek Island This island, on which Port Burwell lies, is politically in the Northwest Territories, but astride the northernmost mainland boundary between Quebec and Labrador. The outpost's twenty-five almost destitute inhabitants were to give up their nomadic life and be joined in a new settlement of prefabricated houses and welfare and educational amenities by 100 or so Eskimos from neighbouring regions with inferior resources By 1970, the average annual income of the twenty-seven Port Burwell Eskimo households (154 people in all) from trading seal skins, fox furs, fish and handicrafts, from wage labour and government welfare entitlements, had risen to $2,600 Though by Eskimo standards this is a comparatively large sum, it remains paltry in comparison with the incomes of three government-employed Canadian households. On average, these grossed about $12,000 p a in return for teaching, engineering, administrative and welfare services Most important of the Canadians is the Development Officer (D O) who supervises the Eskimos in the use of some sophisticated government-loaned hunting technologies, such as seal traps and large boats, and in running a producer and consumer co-operative to which all eighty-four adult Eskimos belong. Although ideally answerable both to his superiors in Frobisher Bay (an administrative town on Baffin Island) and to the Eskimo owners of the co-operative, the D.O in fact enjoys considerable local power, capitalising on the extreme isolation of the settlement and on the Eskimos' limited knowledge of matters Canadian (Riches in press)

In terms of exchange the market economy of Port Burwell is almost entirely discontinuous with the subsistence economy Unlike subsistence produce, market commodities (whether trade produce, Canadian goods, or cash) are not subject to

a generalised reciprocity (Sahlins 1965), and, with a rationality akin to the Paez Indians' disapproval of selling food, the Eskimos consider their exchange for locally secured meats (though not with fish) quite improper—even though it was precisely this sort of exchange that the co-operative was introduced to facilitate (Graburn 1971: 116; cf. Willmott 1961: 24–42).

It is solely with the market economy and the spheres of exchange that have developed *within* it that this article is concerned. Within the nexus of the co-operative there are thus the transactions of the 'credit' sphere, the exchange of trade produce for southern Canadian commodities (both food and durables) through credit-debit accounting. And outside the nexus of the co-operative there are the transactions of the 'cash' sphere, in which special luxuries unavailable in the store are bought with money The development of these spheres relates to the exigencies in Port Burwell of three factors: first, the Eskimos' sources of Canadian goods, second, the media of Eskimo income; third, the limitations on the disposal of these media.

The Eskimos of Port Burwell obtain Canadian goods from two sources. The bulk of the goods—worth $80,000 in 1970—are supplied through the co-operative retail store, and a smaller amount—worth about $1500—through 'external' channels The external channels provide special luxury goods usually unavailable in the store, typically fashionable clothing, confectionery and liquor; they comprise mail order houses, visiting Canadians and summer supply ships, and, for Eskimos who leave Port Burwell for holidays, hospital or conferences, the retail outlets of larger settlements and towns elsewhere in Canada

The bulk of the Eskimos' income is accredited them at the retail store Every adult holds an account at the store and payments from the trade of fish, fur and handicrafts are credited to these accounts, together with cheques for regular and casual labour, and from welfare and family allowance entitlements. Further, since local Canadians also hold accounts, their private payments to the Eskimos—for example, for handicrafts or domestic help—are usually made by credit transfer. An Eskimo could in theory demand that any of these payments be realised in cash, but, as I indicate later, the store's cash reserves are so frequently exhausted that it is understood that all will be discharged through credit. However, Canadian money is the usual medium for the small part of the Eskimos' income that the co-operative does not handle This income stems from the payments of Canadians visiting the settlement—and who are therefore without local store accounts—for both casual and domestic work, and for Eskimo clothing and handicrafts Also, Eskimos who attend conferences or go to hospital receive cash allowances, and any money that remains unspent will be brought back to Port Burwell. Overall, the Eskimos' cash income is very small, estimated at $900 in 1970.

The Eskimos suffer a crucial limitation on the media that may be used to purchase Canadian goods Whilst goods at the store may be secured either with cash or 'on credit', externally supplied goods may *only* be bought with cash. In consort with a high demand and uncertain availability of externally supplied goods, and with the small amount of cash in circulation in the settlement, this restriction is fundamental for the emergence of the spheres of exchange in the Port Burwell market economy.

It is precisely because of their high demand that special luxuries are externally

supplied. In an effort to restrain Eskimo purchasing, government officials have vetoed or heavily restricted their sale in the Port Burwell retail store. Little information reaches the settlement about these goods' external suppliers, Eskimos can only speculate when summer supply ships or visiting Canadians will arrive, or on the amounts or types of goods they will offer for sale That there is little cash for purchasing these luxuries when they *are* made available is in part due to the limited opportunities for earning a cash income, and in part to the D.O.'s reluctance for more than small amounts of cash to be held in the store for withdrawal against account credit—in September 1969, $1100 worth, ordered from the bank in Frobisher Bay, supposedly for a year, lasted only two months [2]

Theoretically, the consequent demand for cash might be controlled through the D O.'s ruling that Eskimos with credit to their accounts are the only co-operative members to be allowed to extract cash from the store. For the majority of Eskimos are always in debt—in March 1970, for example, 68 per cent. of co-operative members between them owed $6,000 But in practice the young store clerks ineffectively enforce the ruling, and usually, anyone is able to withdraw cash, if cash is available, whatever his financial circumstances (Riches in press). Therefore, there is a situation in which an almost unlimited amount of credit is vigorously chasing a commodity in very limited supply cash in the store reserves The value of this cash cannot be inflated It hardly occurs to the Eskimos to alter the face value of Canadian money; and if they were to do this, it would scarcely be tolerated by the co-operative's auditors to whom monthly records of all store transactions are sent.

The Eskimos' difficulty in securing cash is compounded further through the privileged access to store reserves of a few members of the community Local Canadians, who frequently mismanage their finances and find themselves without cash as they are about to leave the settlement for a conference or a holiday, are seldom denied co-operative cash (if any is available) if they are friendly with the D.O. Among the Eskimos, the two store clerks are similarly advantaged Early in March 1970, when the store cash reserves were exhausted, I paid in cash to the value of $200; within three days, 70 per cent had been withdrawn by the clerks, their siblings and parents (others withdrew the remaining 30 per cent).

Aware of the insufficiency of local cash, and unable to predict when they might need it, the Eskimos have realised that it is foolish ever to pay cash into the store Thus, those whose accounts register a debt will never reduce it with cash—the more so since they are additionally vulnerable to the ruling that forbids those in debt to withdraw cash from the store, and which occasionally is enforced when the D.O himself is working in the store. In summer 1970 a group of such Eskimos visited a supply ship, flush with dollar bills for the purchase of cigarettes, confectionery, and, they hoped, liquor; yet according to store accounts they languished in several hundred dollars worth of debt. Other Eskimos hold small credits in their accounts. In circumstances of a more ready supply of cash, they might have elected, when they could, to buy store goods with cash, and so sustain a level of credit sufficient to guarantee future purchases of necessities. But, in Port Burwell, the probability that cash so spent will not be remitted on demand (for example, when one's credit has further improved) obviously militates against such a strategy, particularly when buying on credit is generally permitted those with many

hundreds of dollars in debt. Yet other Eskimos (a very small group) hold large account credits and might have considered it convenient to use store reserves as a bank. But given the difficulties of withdrawal, they have decided, not surprisingly, to secrete their cash in boxes under their beds.[3]

For a range of financial circumstances, therefore, Port Burwell Eskimos hoard cash for the purchase of special luxuries. And by this strategy they have generated two spheres of exchange in their market economy: the first sphere, the 'credit' sphere, comprises commodities supplied by the store together with the trade produce and cheques that purchase these commodities through debit accounting, the second, the 'cash' sphere, involves externally supplied luxuries together with the cash necessary to buy them. In common with the discontinuities reported from most other economies, these spheres are not perfectly discrete. I have discussed the sporadic leakage of cash from the first sphere (i.e. from the store cash reserves) to the second. There are also occasions when the flow is reversed. Through his presence in the store, the D.O. forbids buying on credit to those with $150 or more debt, and demands, further, that half their incomes from trade or cheques (though not welfare cheques) be set aside to lower their debts to more acceptable levels. If the remaining half then proves insufficient to meet concurrent purchasing requirements, afflicted co-operative members have no choice but to hand over cash Such a forced conversion from the cash to credit spheres invokes annoyance, just as, conversely, the extraction of cash from store reserves invokes pleasure. But unlike some other societies, the Eskimos consider neither 'cash' nor 'credit' goods the morally superior, and thus do not evaluate conversions as either prestigious, or as a loss of face (cf. P. and L Bohannan 1968. 234–7, Salisbury 1962: 102).

That the boundaries between the Eskimo spheres are without moral support may be due to the spheres' recent development. I suspect that only with the increasing contact with Canadians of the last decade has the Eskimos' demand for cash reached an appreciable level and outstripped supply. In the absence of moral ideas, the *possibility* of conversion significantly supplies the very mechanism of sphere maintenance The regular, and usually unpleasant, *experience* of conversion —for example, the difficulty of withdrawing cash from the store, or the compulsion of surrendering cash set aside for purchasing special luxuries—*reinforces* the hoarding strategy the Eskimos have devised to handle high demands for limited cash, and which implies the existence of spheres of economic exchange

There is, however, a unique nexus of economic activity among Port Burwell Eskimos wherein small conversions from the credit to cash sphere may be achieved without difficulty, though with some anxiety. This is the 'gambling' nexus.

III

Gambling among Port Burwell Eskimos takes two forms: gambling with cards (*qijugait*) and gambling with the spinning top (*kaittaq*). Card gambling is considered the more prestigious. This is the medium for the high powered and much admired gambling of Eskimos and Canadians in larger and more sophisticated arctic settlements, in which stakes (*sagvataq*) have been known to reach several hundreds of dollars in value as first rifles and then even snowmobiles were gambled.

More modest financial circumstances have restricted the stakes of all Port Burwell gambling to a dollar or less, and seldom will a successful player win more than $25.

In Port Burwell, the two gambling games are rigidly assigned to separate contexts. In the settlement, Eskimos play cards; in hunting camps, they play with the spinning top—indeed, I was reprimanded for bringing cards to a camp. The allocation of types of gambling to types of situation relates to the procedures of the two games.

Eskimo card gambling is a kind of rummy called *patik* (literally, clap)—after the rule that a player should clap when one card only remains in his hand. *Patik* is a game of speed and panache It is played by groups of between three and six men or women who use bullets or cash as stakes, the successful player will be quick-witted Spinning top gambling is in contrast entirely a matter of chance. It is only played by men (even if women are present). The game has a number of variations, but the basic idea is for each player in turn to spin the top and predict which of its four differently marked sides will be exposed when it falls. Before spinning, the player adds his stake to a pool, all or part of which he stands to collect if his prediction is correct Since men do not generally carry cash when hunting, bullets only are the stakes for this game.

As a game of chance spinning top gambling is adaptive to the circumstances of the camp, it little threatens the harmony that must be preserved among men who later will be hunting together for the common good *Patik* gambling may be more prestigious, but some Eskimos play aggressively, and tensions can develop; this is a game better confined to the settlement, where the choice of hunting partners is still a matter for discussion

In the settlement, all men, and the majority of women, gamble On average most will join a game once a week, but there are those who are addicted and their houses have become regular gambling venues There are many motives for joining a party The desire to win sorely needed cash may be one, yet I have been to lively sessions where bullets only were in circulation It is perhaps better to view participation primarily as a manifestation of community integration: through joining a game a resident may assert membership, and a stranger, incorporation, in a community that otherwise lacks mandatory work or ritual groups

Eskimos will enter a gambling party with a variety of assets It may be shown that an individual's game strategy reflects the type of asset he holds, *in relation to the credit and cash spheres of exchange* A first asset is the bullet of the ·22 calibre rifle, the firearm the Eskimos use for hunting small animals, birds and seals Costing 2¢ each and bought in boxes of fifty, these bullets are usually in plentiful supply, even to those without store credit The D O considers hunting for seal skins to be the honourable way for an Eskimo to clear his debts, and hunting, of course, requires a prior supply of ammunition A second asset is the bullet of the heavy rifle, the more powerful firearm the Eskimos use for hunting large animals such as caribou There is no standardised calibre for the heavy rifle, and the Eskimos own a variety of sizes, from ·222 to ·308. The cost of the heavy rifle bullet—which is bought in boxes of twenty—ranges from 25¢ to 30¢, and like the light rifle bullet, the store supply is usually plentiful. A third asset is cash. Although for the most part hoarded for buying special luxuries, small amounts are set aside for

gambling, for, in imitation of more sophisticated settlements, cash is considered the proper gambling stake; in Port Burwell it therefore becomes the *prestige* asset.

The three types of asset may be pooled in any stake. Five light rifle bullets are considered equivalent to one heavy rifle bullet and to a 'quarter' (25¢); four heavy rifle bullets are equivalent to a dollar bill. These equivalences are of course highly inaccurate and favour the man who stakes light rifle bullets. But the inaccuracies are attributable to the Eskimos' impressionistic quantification rather than to any particular strategy. In any case, the Eskimos are not maximising absolute values in gambling—bullets, for example, cannot be regarded as chips to be converted into cash at the end of a session—but maximising scarce resources. Those with cash will thus stake it to maximise prestige; those without will stake bullets and hope to win some of the cash in circulation. Only some of this cash will they then bring to the next session; the remainder they will hoard for luxuries. There is, of course, a gambling etiquette which discourages blatant competition for cash. A man who wins cash early in the game should neither pocket it nor quit the session. He must restake at least some of it in later rounds. Generally, games continue for a pre-arranged time or until one player has won all the bullets or cash his opponents have brought to the party. These others are then said to be *kataktuq*, a word that in other contexts describes an object falling from a height or a man collapsing through thin ice.

Since few Eskimos are ever employed for direct cash wages, and since store clerks monopolise the store's cash reserves, gambling importantly promotes the distribution of cash throughout the community. It provides a bridge between credit and cash spheres of exchange that is available to all.

Gambling similarly reduces inequalities in the control of bullets. These inequalities stem from the variety of heavy rifles in use and the small quantities of each type of bullet thus ordered by the store. Occasionally, quantities prove insufficient and a particular type runs out (weeks can elapse before replacement stock arrives) For those whose supply of this bullet is almost exhausted, it becomes of paramount importance to gamble with others whose hoards remain well stocked. In their attempts to exchange light rifle bullets for the heavy rifle bullets they desire, Eskimos imitate the strategy for the conversion of bullets into cash. I was once the victim of such a strategy. Unaware that ·303 bullets were running short, I happily staked my supply in spinning top gambling, horrified, I soon realised that whenever I won, all I would collect was ·22 bullets—of no use to me since I did not own a light rifle [4]

IV

The process of the Port Burwell cash sphere's isolation prompts the following proposal for 'valued' commodities. that, provided they are free from extensive generalised reciprocity, commodities in short supply and required unpredictably as payments for special goods, services or ceremonial entitlements, will tend to be hoarded *especially* for these payments; such hoarding implies spheres of economic exchange.

One can of course only speculate on the origins of well established spheres of economic exchange, particularly those whose transactions are governed by

ideologies rather than rationalities. But it would seem significant that in terms of both scarcity and usage, nearly all the non-subsistence sphere commodities of the pre-European economies of the Tiv, Tikopia and Siane are akin to the commodities of the Port Burwell cash sphere; and like the Port Burwell credit sphere, the subsistence transactions of these economies might thus be viewed as comprising residual spheres, sectors remaining when the non-subsistence spheres have been cordoned off.

If one accepts that 'the logic of scarcity *is* operative over the whole range of economic phenomena' (Firth 1967: 4), it may be argued that a 'strategising' (Prattis 1973) perspective on spheres of exchange will facilitate comparison among economies of geographically, demographically and politically divergent societies. But before I thus examine the economies of the Tiv, Tikopia and Siane in the light of my proposal, a few qualifications are in order

First, in the sort of circumstances I have proposed, spheres of exchange will be most likely realised among sets of commodities whose payments are *limited purpose* Clearly, spheres of exchange will not develop where a web of required payments permeates virtually the entire economy—as among Northwest coast Indian societies and among the Maring and the Hageners of highland New Guinea, where 'special' valuables are exchanged not only for other valued goods, but for subsistence goods as well (Piddocke 1965; Rappaport 1968 106–8; Strathern 1971. 112).[5] An extreme case of limited purpose payments is of course provided in the Rossel Island economy Here, each of twenty-two ranks of *ndap* shell money exclusively buys specific goods and services (Armstrong 1928) In actual fact, only the five highest ranked shells are associated with discrete spheres of exchange Significantly, these are the shells of highest scarcity—or, at least, scarcity in relation to demand—and are those used for the most auspicious of payments (1928: 63). Thus, old men of high status—who apparently control these higher ranked shells (1928. 66)—may lend 'number 18' shells to younger men, perhaps to enlist their political support (Prattis 1973: 52). But they expect the eventual return of a shell *of the same rank*, whatever other ranks of shell they have collected in the interim as security or interest on their loan (Armstrong 1928: 66–7, 71) No such expectation is implied when the generally more plentiful middle rank shells are loaned (1928. 65), these shells may thus be viewed as mutually exchangeable (cf. Douglas 1967: 134–6)

But the relatively easy exchange of middle rank Rossel shells is undoubtedly achieved largely with the aid of brokers (*ndeb*), Islanders who specialise in and profit from facilitating just such exchanges. This brings me to a second point. That any number of social circumstances can impede the potential emergence of spheres of exchange. Without brokers, the various middle range Rossel shells would probably not be exchangeable. And without gambling, the Port Burwell credit and cash spheres would clearly manifest a higher level of discontinuity, a level that would then approach that found among most of the non-subsistence spheres of the Tiv, Tikopia and Siane. To these spheres we now turn.

In table 1, I have broken down the six non-subsistence spheres of the Tiv, Tikopia and Siane into sets of commodities and their usages; arrows denote the range of transactions within each sphere Thus categorised, one can appreciate that, like Port Burwell money, each of these commodities is at some time required for

socially or physically auspicious, *and* unpredictable payments: as cures for sickness (Firth 1959: 44; Bohannan & Bohannan 1968: 228), as gifts to visitors (Salisbury 1962: 88), and, ubiquitously, as presentations at ceremonials or rites of passage. Firth comments explicitly on their unpredictability of use: 'Sometimes a [pandanus] mat has to be plaited in a hurry owing to an unexpected death, or a death followed closely upon an initiation or other ceremony' (1939: 251).

TABLE 1. A breakdown of the non-subsistence spheres of exchange in the economies of the Tiv, Tikopia and Siane

Commodity	*Usage*
Tiv (Bohannan & Bohannan 1968)	
prestige sphere	
tugudu clothes	purchase of magical rites
brass rods (p. 239)	
cows	purchase of ritual offices
slaves (p 237)	
horses (p. 123)?	purchase of medicines
human dependents sphere	
rights in dependent women and children (mutual exchange—p. 231)	
Tikopia (Firth 1939, 1959)	
bark-cloth sinnet sphere	
bark-cloth (1939 252–3)	payment for: timber, grating stools, canoe and house specialists, damage to valuable tools, wooden bowls; presentation at ceremonials and rites of passage
sinnet (1939: 256)	
pandanus mats (1939 251, 256)	
canoe sphere	
bonito hooks (1939 339–42)	presentation at ceremonials and rites of passage
turmeric cylinders (1959 131, cf 1939 277)	
canoes (1939 246)	
Siane (Salisbury 1962)	
luxury sphere ('*nexus*')	
tobacco (p. 84)	presentation to visiting friends and relatives
rot palm oil (p. 84)	
pandanus nuts (p. 84)	
salt (p. 87)	
several other items of lesser importance	
valuable sphere ('*nexus*')	
pigs (p. 92)	presentation at ceremonials and rites of passage
shell pieces (p 90)	
ornamental stone axes (p 90)	
cassowary egg chips (p 90)	
dogs' tooth necklaces (p. 91)	
birds-of-paradise plumes (p. 91)	
cassowary feather headdresses (p 91)	

Again, like Port Burwell money, each purchasing commodity is itself in short supply and/or hard to procure. Of non-utilitarian commodities, there is evidence (references in table 1 where no comment here) for the scarcity of Siane valuables, Tikopian pandanus mats, and Tiv brass rods and horses. Tiv *tugudu* cloths, it may be inferred, are also scarce, for they are used more often as standards of evaluation than as means of payment (Bohannan & Bohannan 1968: 230). Of utilitarian commodities, there is evidence for the scarcity of Tikopian canoes, turmeric cylinders and bonito hooks[6]; also, for Tiv slaves, Tikopian sinnet, some Siane luxuries, and, over much of their country, Tiv cattle. *Contra* Ortiz (1973: 3), these latter are clearly considered scarce, since 'Every man wants to kill a cow for each of his wives, but few ever manage to do so' (Bohannan & Bohannan 1968: 123). Rights in dependent women and children—the sole commodities of the Tiv 'human dependants' sphere—are obviously similarly scarce. Finally, a few commodities can be considered scarce only in relation to their frequent requirement in large quantities, Tikopian bark-cloth and some Siane luxuries are the examples. To compound their scarcity, Tiv, Tikopia or Siane who aspire to high status, may be obliged to dispense many of these commodities with generosity (Salisbury 1962: 99; Firth 1939: 253; Bohannan & Bohannan 1968. 123) A generous presentation may also sustain important political relationships, or, in exchange, secure supplies of scarce luxuries from other groups (Salisbury 1962: 86)

Though one grants that a few usages listed in table 1 may be legitimately fulfilled with subsistence commodities—for example, Tiv medicine may be bought with chickens or goats (Bohannan & Bohannan 1968: 230)—in the main, I think it fair to speculate that the requirements and scarcity of all the tabulated commodities are such that in the absence of spheres of exchange ideologies, their hoarding would be engendered. Firth, indeed, provides examples of such a strategy; among the Tikopia, 'Usually half a dozen or a dozen [pandanus mats] in the wealthier households are kept tied up in a bundle ready for contingencies such as a funeral' (1939: 251) My suggestion, therefore, is that among the Tiv, Tikopia and Siane, at least, spheres of exchange ideologies reflect the *reservation* of sets of commodities (which may or may not have to be exchanged among themselves) for certain required auspicious usages

V

In summary, let me comment on some suggestions of other writers I would stress, first, that hoarding strategies are unlikely to underly the spheres of exchange of all economies Thus it is not surprising that the discontinuities in the economy of the Mountain Fur are inconsistent with a Tiv-based model for spheres of economic exchange (cf Barth 1967 156) Indeed, as with Paez prohibitions, Fur spheres of exchange probably reflect strategies to conserve subsistence resources (see footnote 1)

Second, I should emphasise that I am not speculating on why particular sets of commodities are required as payment for particular goods or services, or as presentations on particular occasions. Rather, I am asking why such commodities are *reserved exclusively* for these goods, services or occasions, and *never* informally exchanged with commodities required for other goods, services or occasions. I can

agree with Salisbury (1962) that if different sets of commodities are required for exchange or presentation on different occasions to express different types of social relationship, then a mixing of commodities from more than one set would, *on these occasions*, symbolically confuse the nature of the relationships. But, *contra* Salisbury, this argument cannot explain the prohibition—demanded, say, by Siane ideology—on the exchange of commodities from different sets *on other occasions*. Why, for example, should not a Siane and a co-member of his men's house privately barter 'luxury' for 'valuable' commodities when one of them has an excess of the former and too few of the latter? Salisbury indeed gives an example of just this type of strategy Siane, needing European store goods (newly categorised by them as 'luxuries') to entertain visitors, shamefacedly asked Salisbury to change their pound notes (newly categorised as 'valuables') into coins (newly categorised as 'luxuries') so that they might publicly make store purchases in accordance with their spheres of exchange ideologies [7]

A suggestion not unlike Salisbury's—and one which invites similar objections—is put by Douglas (1967) In her view, one may expect societies where prerogatives of certain goods are reserved for incumbents of high social status, to impose restrictions on these goods' exchange with freely available commodities, lest 'structure(s) of privileges' be undermined (1967: 137–8) With this one can agree. But the hypothesis will not account for the far more extensive restrictions imposed by ideologies of spheres of exchange How does it explain prohibitions on inter-sphere transactions *among individuals of equivalent status*? *Contra* Douglas (1967: 138), there is certainly scope for such exchanges, as among the Tiv 'of old days', when a man might exchange brass rods (formally restricted to the 'prestige' sphere) for food (in the 'subsistence' sphere) in order to meet food requirements for a large feast (P. and L. Bohannan 1968. 235).[8]

The hypotheses of Salisbury and Douglas cannot account for the range of restrictions implied in ideologies of spheres of economic exchange. The Eskimo material presented here suggests instead that the origin and persistence of the spheres of exchange of many economies may well relate to strategies of hoarding. Certain scarce commodities are set aside exclusively for the unpredictable but necessary payment of auspicious goods, services, or social or ceremonial dues. These commodities, together with the goods, services or dues they purchase, then comprise one or more spheres of economic exchange

NOTES

My thanks are due to Robert Paine and the Institute of Social and Economic Research at the Memorial University of Newfoundland for the award of an Izaac Walton Killam fellowship which provided me with twenty months of fieldwork in the Canadian Arctic, between November 1969 and July 1972 The ethnographic present of the Eskimo material is around September 1970.

[1] Thus, among the Mountain Fur (Barth 1967) two principal spheres of exchange are underlain by prohibitions on the sale of beer in the market place, and on the sale of labour for cash The former prohibition would protect local supplies of millet, from which beer—a commodity in high demand on the market (1967: 157)—may be brewed The latter prohibition would dampen the local demand for cash (communal labour is required in house building and in phases of millet cultivation) and hence the need to cut back on subsistence production to increase cash cropping

[2] The D O ignored the advice of fellow Canadians that he should increase the local supply

of cash to encourage Eskimos to learn to handle Canadian money. But he did not fear that Eskimo support for the co-operative would dwindle if he facilitated purchasing from mail order houses (for which post office regulations demand cash). Believing that all Eskimo requirements were satisfied by the local store, he was reluctant to acknowledge a demand for cash for external purchasing And he argued that the store lacked the security to stock large cash reserves

[3] It is worth noting that neither do local Canadians make store purchases with cash. They find it convenient to buy on credit and then periodically write cheques to clear their debts

[4] Gambling with bullets suggests that the market economy flow of commodities can influence the local subsistence economy. And it is indeed the case that the injudicious investment of bullets may seriously curtail an Eskimo's hunting One man I knew lost his whole stock of ·22 bullets at a spinning top session, and the next morning was said to be feeling very lonesome (*patnngutuq*) Luckily, bad weather prevented hunting that day and provoked another session The man staked his remaining half dozen heavy rifle bullets, cleaned out his opponents, and then hunted happily for the rest of the camp Other Eskimos, however, are more cautious and reserve a quantity of bullets exclusively for hunting

[5] The thus engendered distribution of subsistence goods over wide areas can of course be achieved other than by these goods' exchange with 'valuables' Among the Tiv, wide ranging ties of friendship and kinship are employed for the same purpose (Bohannan & Bohannan 1968 · 143)

[6] I am not concerned here with why Tikopian bonito hooks are scarce—a mystery, indeed, to Firth (1939 342), for they can apparently be produced quite easily Presumably, there are sanctions against their 'mass production'

[7] To the extent that the purchasing commodities of spheres of exchange may be regarded as moneys, Codere's comments (1968 565) on plural moneys should be mentioned here Yet, that the employment of several moneys, each for the purchase of a certain small number of goods, implies an economy with several relatively inconvertible spheres (Codere 1968), is an assertion I cannot support—as my criticisms of Salisbury would indicate I do not see how merely their limited 'symbolic powers' would obviate these moneys' informal swapping, a procedure that inconvertibility would disallow

Of course, informal swapping could not occur if a good's purchasing commodity (or money) could be employed *only* on specified purchasing occasions But this sort of restriction is not itself implied in ideologies of spheres of exchange, its presence would thus constitute a basis of spheres of exchange—one quite separate from the basis provided by strategies of hoarding

[8] In Sahlins's view, intra-community prohibitions on the exchange of foodstuffs for 'durables' sufficiently account for subsistence 'circuit(s) of exchange' 'For a wide range of social relations, balanced and direct food-for-goods transactions (conversions) would rend the solidary bonds' (1965: 173). This may be so, but it begs the question why—as among the societies here cited —inter-community food-durable exchanges are also prohibited The scarcity of the durables (valuables) would perhaps provide the more complete answer Indeed, it may be that a reluctance to part with valuables (in exchange for food) could have promoted ideologies for generalised intra-community sharing of food

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FATHERS WERE NOT GENITORS

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The question of whether cultures existed which did not recognise any connexion between copulation and pregnancy is an old source of debate in anthropology Recently Leach has reviewed this issue and concluded that it is highly improbable that such genuine ignorance exists anywhere as a cultural fact A study of beliefs on Bellona Island in the Solomon Islands seems to point to the opposite Before contact with the outside world the Bellonese had apparently not recognised the biological role of a human genitor

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In 1966 Edmund Leach revived the old anthropological discussion of whether cultures existed which were 'ignorant' about the relationship between copulation and pregnancy In contrast to Spiro (1966) he finds it 'highly improbable on common sense grounds that genuine "ignorance" of the basic facts of physiological paternity should anywhere be a cultural fact'. He moreover disapproves of the word ignorance or innocence 'To say that a native is ignorant amounts to saying that he is childish, stupid, superstitious Ignorance is the opposite of logical rationality, it is the quality which distinguishes the savage from the anthropologist' (Leach 1969 92)

In this article I shall attempt to show on what grounds I disagree with Leach in his strong claims concerning the non-existence of peoples who do not care about physiological paternity or who are 'totally ignorant' about the facts of life.

Let me first say a few words about the use of the word 'ignorance' in the present context I agree with Leach when he says that 'ignorance is a relative matter, and obviously we are all ignorant in some degree, particularly about sex' (1969 91) If this is true it must also be true that 'knowledge' about the facts of life are relative We 'know', for example, that all matter consists of atoms and molecules We 'know' that a male sperm cell has to unite with a female ovum to complete fertilisation. Only few of us have had the opportunity to, or cared to, observe these facts of life. Yet we happily believe in them and in the processes in which they are involved as 'true' in a scientific sense Unable to verify the statements we believe in what scientists tell us about our micro-world, and we live our lives according to our beliefs. The unseen in this way comes to constitute part of our knowledge, just as it does in almost all other human cultures

Unlike the situation in our own culture, it is not in the root of all cultures to attempt to explain the origin of everything. It seems unlikely, at least to me, that every single culture in this world has myths of an etiological type telling why birds have no ears, why there is a difference between man and woman, or how the world came into existence. Most cultures, however, have focused upon certain aspects. I shall not deal here with the intriguing problem of why a specific culture has focused upon specific aspects, but rather attempt to show that even though sex is admittedly a universal phenomenon of specific interest in all cultures it does not

necessarily follow from this that all cultures have elaborate beliefs, myths, or other kinds of explanations concerning the origin and persistence of sexual activities, and concerning their function and consequences—or that the focus of such cultures shows ignorance or stupidity if it is not the same as ours.

Obviously the lack of myths concerning sex does not necessarily indicate ignorance—in exactly the same way as the existence of myths concerning a certain phenomenon should not be contrasted to Western scientific truth, because myth and science appear to have quite a lot in common.

Whether a given society has myths concerning sex or not it does not seem to me to be exactly obvious that sex should mean the same or be given the same place in all cultures. It is, for example, not impossible that we as Westerners are particularly obsessed with the biological aspects of paternity whereas we neglect to focus our interests on other sides of sexual activities If we ask the question what coitus is all about, the obvious answer of a Westerner might be that it serves to procreate children A population itself, or perhaps even an anthropologist, might conclude that the 'function' of coitus is to emit signals to the surroundings that the copulating couple still maintains the functions of a nuclear family on which other parts—or even non-members—may rely as a social group, thus leaving the biological part of the explanation of coitus completely aside.

Below I shall present a cultural code of beliefs concerning parenthood which certainly differs from that of Western science To the Polynesians of Bellona Island in the British Solomons its statements are proved through experience through generations, and it is only lately that they have been told by missionaries and British administrators that their beliefs through generations were 'false' beliefs.

I shall focus on the question of their views on the relationship, or rather non-relationship, between copulation and pregnancy.

The data should be evaluated as a kind of historical anthropology in the sense that they consist of statements by present-day Bellonese concerning their beliefs and acts as they were before the introduction of Christianity about 1938, and the subsequent contact with British government administrators and with foreign missionaries after the second world war

The code presented here is based on data obtained from a) structured interviews by means of a question list with five male adults, four of whom were adults at the arrival of Christianity, and one of whom was then a teenager, b) numerous responses by other male adults to parts of the list of questions, c) background data concerning other aspects of the culture obtained while working on other parts of the total cultural code, and d) a very detailed interview made by Ulla Prætorius Israel with an adult female informant in 1968. Where ideological variations of apparent significance for our understanding of the Bellonese beliefs appear, they will be presented here. As the people of Bellona have now almost totally accepted the new code concerning the beliefs in question, the presentation will be given in the anthropological past—in order to keep the reader in the correct temporal position in relation to the subject matter

The Bellonese believed that children came from the abode of the gods which lay somewhere below the Eastern horizon. There was some uncertainty or vagueness as to whether they exactly originated in Nukuahea, the abode of the sky gods,

or in Manukatu'u, the abode of the district gods (Monberg 1966: 63). In Nukuahea was the house of offspring (*hange matingınga*) where, most people believed, unborn children lived, and to which infants returned when they died. Children were brought through the world of darkness (*poo'ungi*) by ancestral spirits and implanted in the womb of a woman. How this latter actually physically took place was a matter of no concern to the Bellonese. 'That is merely how it happened.' Stillborn children or infants who died when very young were sometimes believed to be taken back along the same route. Some Bellonese believed that these children were reborn later by the same woman, and, accordingly, given the same name. This was, however, not a general belief.

A child was termed 'the fruit of a woman (*te hua o te hahıne*)' and likened to the fruits of trees and the eggs of animals

Bellona had 428 inhabitants at the advent of Christianity in 1938. Each of these was member of a specific patrilineal descent group (*manaha*) and a particular clan (*sa'a*) of which there were two: Iho and Kaitu'u. Each clan was divided into a number of patrilineal groups, three in Iho and eighteen in the Kaitu'u clan. Residence was almost invariably patrilocal with a man living on parts of what was previously his father's land

When a man and a woman moved together in marital alliance they thus lived on land belonging to the husband of the family The Bellonese believed such an alliance to produce progeny, *not* by or because of the man's copulation with the woman, but because the patrilineal ancestors and the deities of the patriline were pleased (*mangaohie*) with the alliance A woman became pregnant because the deities and ancestors of her husband had sent her offspring If too much time elapsed before pregnancy occurred the husband might invoke his ancestors or deities by uttering a short prayer asking them to send a child to his wife. Usually pregnancy occurred after this The Bellonese were well aware of the first signs of pregnancy: disruption of menstruation, swelling of breasts, and, commonly, nausea linked with a desire for specific types of food.

When such signs occurred the husband would most commonly prepare a minor ritual at the grave of one of his patrilineal ancestors. Graves were usually at the sides of the entrance to the ritual grounds in front of the homestead of the most important members of the patrilineal descent group Graves of important ancestors were covered by small houses. The father-to-be weeded and swept the grave mound, and sometimes brought clean sand to it together with a small amount of food. He then addressed his ancestors and the ancestral deities imploring them to send a *male* child to his wife in order that the patriline might continue. My Bellonese informants were very vague as to whether there was already a child in the womb of their wife at that time They uniformly stated that this was not an important matter at all. It was the formula which was of importance. In this formula the ancestor and/or deity was asked to bring a male child to the woman. This seems to indicate that at a certain stage in the past the Bellonese may have believed that actual pregnancy did not occur before the ritual had been carried out.

The Bellonese never prayed to have female children, and the birth of a female child was considered a sign that the ancestor had not heard the prayers, or for some other reason failed to help his descendant. This should not be seen as an

indication that the Bellonese did not want female offspring, but rather that 'girls were born anyway'. Informants stated that it was of the greatest importance for a couple to have male offspring in order that the patriline might continue, but that it was also of some importance to have daughters because they, through their subsequent marriages, created affinal ties with other patrilineal descent groups. Because of the preference for male offspring men would commonly choose to carry out their little rites at graves of ancestors who were renowned for having answered prayers for male children.

Women never prayed to supernaturals to have children. I shall deal with the problems concerning children born out of wedlock, abortion, and infanticide below.

During pregnancy the woman was careful in her daily habits: she did not indulge in heavy work, in the gardens or elsewhere, she ate a hot meal in the morning, a thing only done by pregnant women, and she preferably ate specific food items such as molluscs, resin of the *ngeemungi* tree (Santiria apiculata), fully developed but unripe cooked papayas, spinach, wild yams, and bananas. She would avoid a considerable number of food items believed dangerous to her 'fruit', especially twin fruits of bananas and certain trees in order to avoid having twins The Bellonese explained the occurrence of twins as either the result of breaking the taboo on eating twin fruits, or, if it could not be explained in this way, as a punishment from the ancestors or deities Twins, whether of the same or different sexes, were almost always both killed by burial under stones immediately after their birth.

It will be obvious to ask where copulation came into this picture. The Bellonese answer to this is: nowhere. According to all sources copulation was considered 'a pleasant thing, joke, or game'. To the direct question of why humans copulated all informants said that it was because people liked it, it tickled, it was the habit of men and women. They moreover added voluntarily that it was not until the advent of British government officials and missionaries that they heard about a relationship between copulation and pregnancy. Our female informant said that even then it took a very long time before people actually 'understood' this relationship. Personally I doubt whether the older men of the island really believe in it, even today. Several informants said: 'We were stupid then [before the arrival of Christianity].' Needless to say only they themselves have the right to make such a statement. Outsiders such as ourselves would prefer to say that they seemed to have a different philosophy behind the occurrence of children from that which we have, and that they focused their interests differently from us.

Admittedly the present author remained for a long time suspicious concerning the alleged Bellonese ignorance of biological paternity. Was it really likely that we had here, in front of us, a patrilineal society which did not recognise the biological importance of a human genitor? All test questions put forward seem to indicate that this was really the case. Only once, in a group-interview with a number of males, one middle-aged informant said that 'we suspected that there might be some connexion, because the penis went into the place where children are, but it was not an important matter at all, and we did not think about it.' Most of the other men present during the interview disagreed with this and said that they had never thought of any connexion between sleeping together (*momoe 'anga*)

and pregnancy (*hai tama 'anga*) before white man told them about this. 'How should we have known?', they added.

When asked what was the significance of semen, most informants showed ignorance. Some said it was a kind of urine; one informant stated that it had the purpose of making the vagina slippery and pleasant, but most men did not know what it was

Examples of other questions meant as a test of the veracity of the alleged ignorance of biological fatherhood were the following:

'What did women do if they did not wish to have children?'

All informants uniformly said that husbands were asked to pray to the ancestors imploring them to stop sending more children, or the woman massaged her abdomen or put hot stones on it during pregnancy, thus provoking abortion. It was added, however, that these procedures were only carried through if it was known from previous experience that birth would be dangerous to the woman's life, and never as a means of controlling the number of children of a married couple. Informants added that in former days it was almost impossible to have too many children, as abortions, stillbirths, and infant mortality rates were high Our census for 1938 seems to confirm this. If a family felt that they had too many children, adoption would be the most common means of controlling the size of individual nuclear families (Monberg 1970. 130). Recent research has shown that Bellona was ecologically capable of feeding a much larger population than the one existing at that time (Christiansen in preparation).

Another question was· 'What did you think was the reason that the child looked like his mother or father?'

Most informants said that resemblance was not recognised at all, but some stated that a child's resemblance to its mother's husband was due to affiliation with his patriline If it looked like its mother it was due to the fact that mother and child were of the same blood Interestingly enough all informants stated that children and mothers were 'of the same blood' (*te toto e tasi*) whereas this was not believed to be the case with fathers and their children. When asked why a child sometimes looked very much like a person other than his father, all informants answered that 'people did not speculate about this (*he'e maanatu kinai*).'

Another question concerned the problem of who was considered the father of a child if it was known that the mother had had sexual relations with others than the husband The answer was uniformly the husband When asked how people could know this, the equally uniform answer was because he was the woman's husband.

If marital alliance was so important for the procreation of children because the husband of the woman was the one who had to pray for children, it was obvious to ask how children born by unmarried women were created at all. The Bellonese answer to this was that such children, especially if they were males, were signs of gratitude towards living descendants such as the unwed mother and her kin, as they were 'given' without reciprocal offerings or prayers. An unwed mother was thus not stigmatised for producing children out of wedlock (the contrary is the case today under the rule of Christianity), but the child's birth status rights were somewhat uncertain because it did not belong to any patrilineal descent group on the island. Children born out of wedlock were most commonly adopted and thereby came to belong to the descent group of their adopting father (Monberg 1970: 123).

If a husband died while his wife was pregnant or if he had divorced her, the child would normally be considered as belonging to the patrilineal descent group of the deceased or divorced husband. The reason for this affiliation was said by the Bellonese to be that it was the dead or divorced father who had originally prayed to the supernaturals for a child. If a pregnant woman, however, remarried after the death of her husband or after he had divorced her, the patrilineal affiliation might sometimes be a source of dispute among the patrilines whose members were involved as husbands of the woman. Normally, however, the members of the patriline of the person who was married to the woman at the time she became pregnant, would be granted the rights to the child.

In spite of the uniform and persistent statements by my informants that they were actually ignorant about the relationship between copulation and pregnancy before the advent of Christianity and before more regular visits by British government officials began around the second world war, some readers may still feel in doubt about the veracity of the Bellonese claims.

If we consider some other parts of the Bellonese cultural code readers may have an opportunity to reconsider their opinions.

Before the time of contact between outsiders and the Bellonese there were no mammals of considerable size on the island other than humans, flying foxes, and rats Contrary to a society of herdsmen, humans had no real opportunity to observe the relationship between copulation and pregnancy in nature.

Sexual relations on Bellona were—apart from very strict incest rules—extremely free. (The alleged ignorance of the 'function' of copulation in conjunction with strict incest rules points to the latter as being part of a biological rather than a cultural code) Unwed adult couples mated rather freely, and there was allegedly considerable freedom in the extramarital sexual activities of married males and females, although such extramarital relations were publicly deemed as bad, and most commonly created anger and jealousy.

Bellonese myths, stories, songs, and rituals contain, unlike the case in most other Polynesian cultures, no reference at all to sexual activities. (An exception is a body of epigrammatic songs, *kananga*, which function as taunts or are sung to attract prospective lovers These songs are discussed elsewhere (Monberg in press)) Even in fertility rites where ancestors and deities were implored to send good crops, many fish, and children in return for food offerings sexual activities were not mentioned at all. Similarly Bellona dancers never imitated the movements of copulation, or anything else connected with sex

Naturally, people of Bellona talked of sex, but never in public and not in connexion with procreation. This is at least what they themselves claim Sex was frequently mentioned in connexion with swearing. Common curses were 'Eat your mother's genitals', 'Go and sleep with your sister', or even 'red penis'. In humbling oneself in front of one's brother-in-law or of a prestigious person from another lineage one would often talk of oneself as 'your anus', thus displaying a glimpse of Bellonese abhorrence of anything vaguely connected with faeces or urine. A discussion of Bellonese relations to sexual activities, to defecation, and micturation deserves a paper in itself and will not be discussed in detail here.

To the Bellonese sexual activity was thus a social game with no direct relation to the supernatural world, whereas procreation of children was a more serious

affair, an act of social importance involving the activities of ancestors and deities and also concerning the problems of the allocation of the children within the social world of the Bellonese.

It could easily be claimed that the data presented above only concern the surface structure of the Bellonese cultural code, and that deep in their hearts the Bellonese knew perfectly well what copulation was all about, and that their claims of ignorance were merely parts of an ideological pattern tending to screen reality and to emphasise the methaphysical topography of the relationship between gods and men.

But what kind of data would be necessary to make such a claim valid? On what basis other than assumptions, guesswork, or, perhaps, wishful thinking could such a claim be made? There seems to be no empirical support for the claim that 'ignorance' of any connexion between copulation and pregnancy is merely a superficial ideology on Bellona.

The surprising thing about this 'ignorance' is not its existence, but the fact that it can surprise us, and that some anthropologists may be willing to submit themselves to all kinds of intellectual acrobatics to explain it away.

NOTE

This article is based on fieldwork between 1958 and 1974, supported by several grants from the Danish Research Council

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PATTERNS OF EARLY FORMATIVE SUBSISTENCE IN SOUTHERN MESOAMERICA, 1500–1100 B.C.

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During the Early Formative Barra and Ocós phases, the occupants of Pacific coastal Guatemala and Chiapas possessed a technology and ceramic style which differed in many respects from those of the adjacent interior. The stylistic integrity of the coastal zone began to break down at the end of the Early Formative period when contact with the interior led to the adoption of new styles, and to changes in the technology. An examination of subsistence artefacts, agricultural potential, and food remains suggests that changes in coastal life at the beginning of the Middle Formative extended to economic pursuits as well. Evidence indicates that the colonising communities of the Early Formative brought, or soon acquired, a subsistence pattern on the coast which emphasised tuber cultivation. Increasing contact with the interior highlands, possibly reinforced in part by exchange of agricultural products and lithic material, resulted in the integration of both new ceramic styles and extensive maize cultivation into the coastal pattern by the beginning of the Middle Formative period.

I

The Early Formative Ocós Phase in the Pacific coastal Soconusco region of Guatemala and Chiapas is among the most complex lowland adaptations of the Early Formative period in Mesoamerica with respect to both the network of extra-areal contacts and the internal structural complexity characteristic of Ocós communities. It is the purpose of this article to detail the specific parameters of that complexity as they respond to a compound question: To what extent, if any, were the lowland Early Formative peoples engaged in agrarian pursuits and what was (were) the cultigen(s) whose production accompanied the apparent *ex nihilo* emergence of these sedentary communities during the Early Formative? Lacking direct evidence in the form of plant remains, answers to these questions will require the most careful examination of such 'indirect' evidence as is available, with explicit attention to the particular strengths and weaknesses of each inference. An examination of these numerous subsidiary problems will lead us to conclude quite tentatively that, while their contacts with the rest of southern Mesoamerica (especially with highland Guatemala, Honduras, and the entire Tehuantepec region) were extensive, the Early Formative peoples of the Soconusco relied upon tubers and other non-maize cultigens to a degree unique in Mesoamerican prehistory. I should admit from the start that, although the evidence at present available lends strong support to these conclusions and nowhere contradicts them, neither do the data inject them with certainty or even high probability. Still, I think it can be shown that this absence of definitive conclusions does not detract value from the inquiry, but only suggests certain specific objectives for further investigation. The several excavations currently under way in southwestern Mesoamerica, as well as further work in Colombia and Ecuador, should provide confirming evidence for my hypotheses.

II

History of archaeological research in the Soconusco

Elaborate Late Formative and Classic Period developments in the highlands and Maya lowlands have long drawn students to those areas in overwhelming numbers The southern Pacific lowlands, whose initial contribution has as yet been only minimally recognised by most, and whose later role as a cultural backwater of sorts has offered little appeal to prehistorians, have until quite recently been the object of resounding neglect. Of the few investigators who have attended to examinations of the Early Formative in this area, only two have met with noticeable success.

M. D. Coe (1961) established the first Early Formative chronology for the Soconusco, calibrating it with the ceramic sequence from Chiapa de Corzo (Dixon 1959; Lowe & Agrinier 1960), and discussing its affinities with areas to the south (Coe 1960). Coe's initial analysis, based upon excavations at La Victoria near the Rio Naranjo in southwestern Guatemala (see fig 1), was expanded with later work

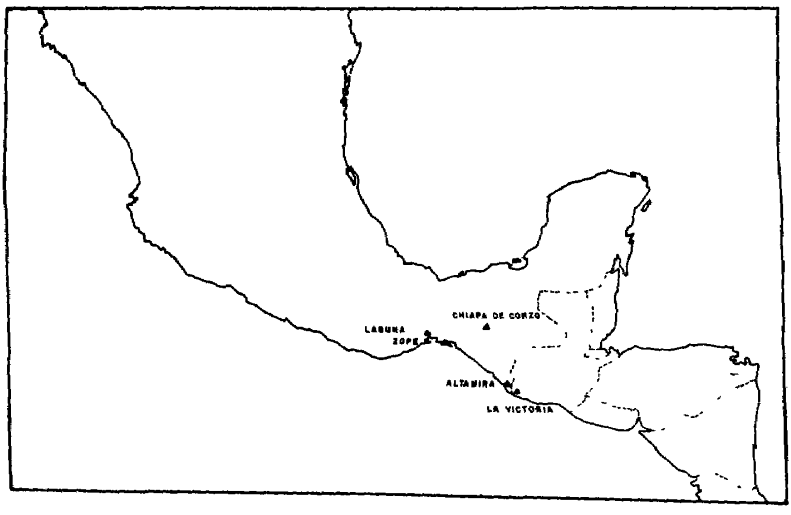


FIGURE 1

at the nearby site of Salinas la Blanca in collaboration with K. V. Flannery (Coe & Flannery 1967), albeit without the discovery of a second Ocós component. The latter investigation also resulted in the only study available which focuses specifically upon microenvironmental zonation in coastal Guatemala.

The second author to whom this inquiry is greatly indebted is Gareth Lowe who, with Dee Green (Green & Lowe 1967), located the only other known Ocós component, at Altamira, in Pacific coastal Chiapas some 150 kilometres northwest of La Victoria Lowe's work earns added importance from the fact that, at Altamira, Ocós levels are conformably underlain by a very similar component of the newly defined Barra Phase, which Green & Lowe (1967. 56) argue convincingly is the

immediate progenitor at Ocós. These authors have thus added strength to the suggestion that, if the development of the Barra-Ocós style is not wholly indigenous to the Soconusco, it is quite probably the earliest ceramic horizon there.

Little other work has been completed along the Soconusco coast. However, recent excavations by Robert and Judith Zeitlin at the coastal site of Laguna Zope in Oaxaca have yielded a continuous sequence from the earliest Formative into the Classic period. Laguna Zope promises to be one of the most important Early Formative sites in Mesoamerica; completed analysis of the material is anticipated with enthusiasm The Zeitlins have generously made certain lithic materials available for preliminary study by Miss Hallie Whitlock, whose analysis contributes substantially to hypotheses presented here. Additionally, a number of sites in the Tehuantepec region have yielded data which are of interest Dixon's (1959) and Lowe and Agrinier's (1960, Lowe 1962) work at Chiapa de Corzo has been mentioned; other excavations by the New World Archaeological Foundation, especially those at Izapa (Ekholm 1969), are essential to the hypotheses presented herein The Central Depression has also yielded materials at the Santa Marta Rock Shelter (MacNeish & Peterson 1962) which will be significant for this analysis. Other investigations in the Tehuantepec region, most of them concentrated farther east, will be of some value here but will be mentioned later Finally, this discussion, especially in its concern with the reconstruction of Early Formative environments, is indebted to McBryde's (1947) admirable attempt to synthesise the cultural and physical geography of southwestern Guatemala and to his implicit respect for the then nascent concept of microenvironmental analysis.

III

Early formative styles and typological affinities

Even though this investigation is only secondarily concerned with ceramic analyses, typological considerations have earned a natural priority in archaeological synthesis and deserve attention here. Although much of what can presently be said about extra-areal contacts and influence in the Early Formative Soconusco has already been discussed by M D. Coe (1960, 1961: 120–134) and by Green & Lowe (1967: 60–75), some of these data are of special significance to this analysis. Recently reported excavations in central Chiapas have provided radiocarbon dates which confirm most of these authors' chronological evaluations of Early Formative phases in the Soconusco.

The chronological chart presented in fig. 2 is substantially the same as that presented by Susanna Ekholm (1969: 19), with the inclusion of the Cotorra and Dili phases as they occur at Padre Piedra Because, as the chart suggests, the story of the Early Formative in southern Mesoamerica appears to rest upon an analysis of the Barra, Ocós, and Cotorra phases, it is desirable to evaluate the similarities and contrasts which they present in terms of ceramic design. In particular, it will be useful to compare the latter two phases, which are at least in part contemporaneous.

Although I first reached this conclusion on the basis of other arguments (which will be presented later), I am persuaded that ceramic variations taken alone are sufficient to demonstrate that the full Ocós phase is a distinctively lowland phenomenon whose main features are but poorly manifested north and east of the coastal

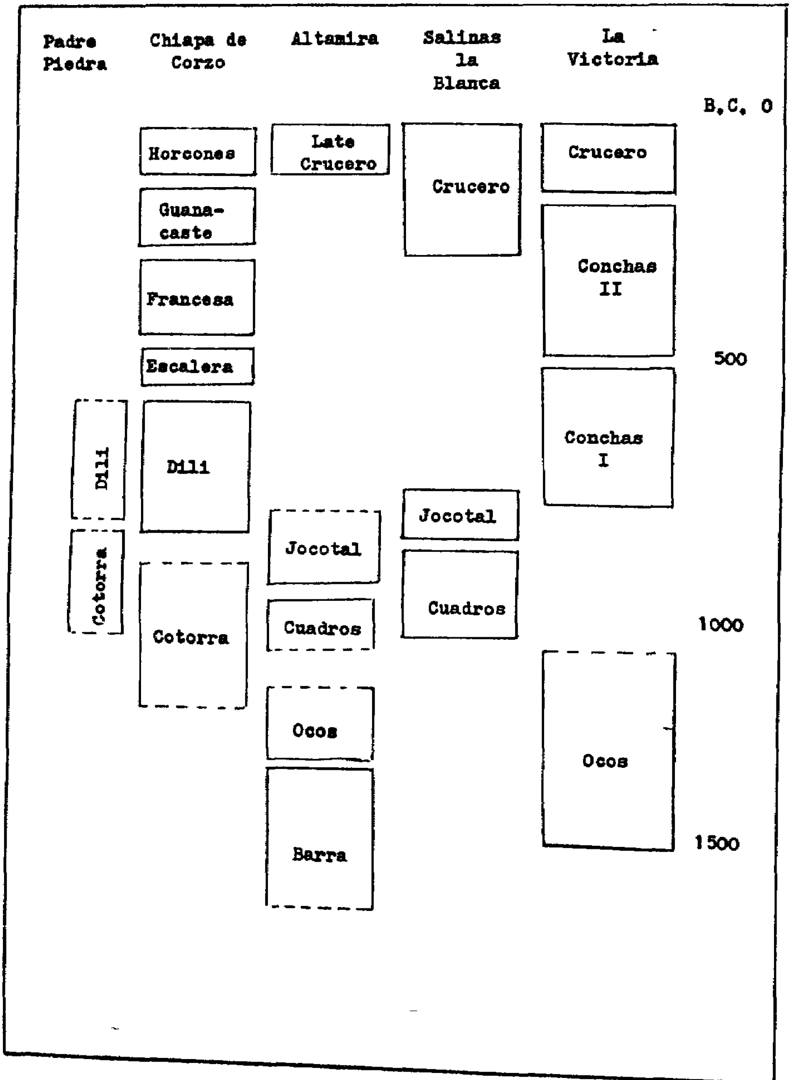


FIGURE 2. Ceramic chronology of southwestern Mesoamerica.

zone. Even at Chiapa de Corzo, on the basis of whose ceramic sequence the Ocós phase was first chronologically established by Coe (1961: 123–4), the Early Formative contrasts between Cotorra and Ocós are perhaps greater than the similarities. At the minimum, it can be said that the difference between the Ocós and Cotorra phases are as great as or greater than those between Cotorra and Cuadros. When the La Victoria report was published there were good grounds for considering the Ocós and Cotorra phases to be largely coeval (Coe 1961 · 123; Lowe 1960: 5) In the interim, however, work at Altamira, Padre Piedra, and Salinas la Blanca, along with further radiocarbon dates from Chiapa de Corzo, have yielded the revised chronology presented in fig. 2, which shows less overlap between Chiapa I and Ocós (Green & Lowe 1967, Coe & Flannery 1967; 67–69; Ekholm 1969: 19–22). In light of this evidence, Coe has recognised that

> there is a strong similarity between the ceramic assemblages of Cuadros and that of Chiapa I (Cotorra or 'Pit 50') at Chiapa de Corzo—far more convincing than similarities between Ocós and Chiapa I (Coe & Flannery 1967 68).

The available ceramic evidence indicates that the most complete or elaborate Ocós manifestations were restricted to the *Costa*; while sites farther inland, along the isthmus, enjoyed fairly extensive contact with coastal communities, the latter were closer to or in closer contact with, the central geographical focus of the Ocós horizon. It may, then, have been Cuadros times before a largely homogeneous material culture (and especially a common pool of decorative techniques) was widely shared across southern Mesoamerica Even then, despite the mutual similarities among early Middle Formative ceramics from Salinas la Blanca (Coe & Flannery 1967. 67–9), Altamira, Padre Piedra (Green & Lowe 1967: 66–7), and Chiapa de Corzo (Dixon 1959), there is evidence of a cultural division between the isthmus and the Pacific Coast. The Cuadros and Jocotal complexes at Padre Piedra, for example

> have a closer superficial resemblance to those of Chiapa de Corzo than to their coastal counterparts. This is not surprising, viewing the mountain range separating the interior and coastal regions (Green & Lowe 1967 67)

The greater homogeneity between inland and coastal sites during the Middle Formative may well be due, as Susanna Ekholm suggests (1969: 95–6), to an Olmec expansion across the Tehuantepec region from the east, and to the consequent possibility that the Cuadros phase is intrusive throughout this area. David Joralemon (personal communication, 1974) has called my attention to several rock carvings in Olmec style, distributed throughout the southern Isthmus area. These carvings have been discussed by Ekholm-Miller (1973), who tentatively suggested Cuadros (early Middle Formative) associations for several. Joralemon notes that corn symbolism is present on most of these carvings, often in the form of maize cobs held in the arms of human figures. These Olmec 'corn-bearers' are of special interest for the hypothesis that influence from the north (including an emphasis upon maize cultivation) was first strongly felt in the Pacific coastal area during Cuadros times. Regrettably, my inquiry must end at the early boundaries of the Middle Formative, and the question of Olmec expansion could only be pursued here at the expense of further discussion of earlier periods.

At Santa Marta Cave, in western Chiapas, MacNeish has recorded an early

ceramic complex which shares aspects of the Ocós and Chiapa I styles. A single radiocarbon date of 1320 B.C. ± 200, from charcoal associated with Cotorra-like materials, supports the claim that these remains are coeval with later Ocós and early Cotorra (MacNeish & Peterson 1962: 38).

On the piedmont, to the northwest of Altamira, an Ocós component has been posited at Izapa by Susanna Ekholm (1969. 25–6) Because, however, Mound 30a at Izapa was constructed (and reconstructed) with fill from adjacent areas, there is no hope of establishing the original stratigraphic positions of this occupation on the basis of available data. The 'occupation', that is, has been hypothetically reconstructed from the occurrence of Ocós ceramic phase markers in the Mound 30a fill. Yet I am uncertain whether the group of sherds which is taken to represent the Ocós component at Isapa is especially similar to those from Altamira and La Victoria. The absence at Izapa of any hollow clay figurine tradition assignable to the Early or Middle Formative is also striking in light of the manifestations of such a tradition at La Victoria and Altamira Examination of subsistence activities at these and other sites (see below) corroborates the suggestions that peoples living on the piedmont and in central Chiapas were engaged in significantly different activities from those dwelling on the coast

Knowledge of Early Formative contacts and cultural affinities between coastal Soconusco and regions to the south remains substantially as it was outlined by Coe (1960) several years ago The total corpus of this evidence, together with arguments such as those marshalled by Coe (1960), Evans & Meggars (1957), Strong (1951), del Solar (1966), and others ensures the truth of the onetime heterodoxy that aboriginal Middle Americans enjoyed extensive maritime contact with much of the New World Lathrap (1973) has recently provided evidence which reinforces this hypothesis, and has suggested that tuber cultivation and trade of manioc flour were important elements in this maintenance of long distance trade networks in both South and Middle America While little new data bearing directly upon the Ocós phase have become available, my understanding of Ocós as a phenomenon with a lowland hearth leads me to suspect that the full story of Ocós (and perhaps Barra) contacts with South America lies unexcavated along the Pacific coasts of Nicaragua and Panama

The total of the ceramic evidence reviewed here, though clearly incomplete, shows Ocós influence throughout Chiapas during the Early Formative, although it seems to appear only in diluted form at the inland sites Usually, of course, it is misleading to speak of cultural influence in such a unilateral fashion Probably all the Soconusco sites participated in a weak sphere of mutual influence. It is possible that the lowland site of La Victoria, situated as it was on a sheltered estuary near the mouth of a river, also served as a terminal for canoes bearing trade goods or other products from areas to the south. However, we risk falling here into the realm of purest conjecture, and should proceed to a further examination of contrasts between inland and coastal sites.

IV

Early Formative subsistence in southern Mesoamerica. general

In Barra and Ocós levels at Altamira, despite the presence of good preservation conditions (Green & Lowe 1967: 58), faunal remains were noticeably scarce.

Indeed, in the Barra levels of Mound 19, no bone was found (Green & Lowe 1967: 31). Shell, curiously enough, is also completely absent (p. 58), although the site is now less than 6 km from the ocean and even closer to brackish-water lagoons. With respect to actual biotic remains, then, Altamira shares with La Victoria a conspicuous scarcity of animal bone (Coe 1961: 141–2); yet it is possible that the occupants of the latter site relied heavily upon estuarine fauna, especially shellfish, throughout the Early Formative occupation (Coe 1961: 114). Floral remains in the form of fossilised seed are absent in Ocós levels, although it should be noted that fifty impressions of maize cobs (presumably of the primitive Nal-Tel variety) were obtained from Early Formative Cuadros floors at the adjacent site of Salinas la Blanca (Coe & Flannery 1967: 71) However, inasmuch as the nature of the transition from Ocós to Cuadros remains unknown for the present, it would be incorrect to project the Cuadros subsistence system back into Ocós times. As Lowe has noted,

> pottery comparisons definitely do show without any doubt an early ceramic-using culture [Ocós] oriented to South America giving way to another (Cuadros-Jocotal) unequivocally related to the north . (Green & Lowe 1967: 59)

There is in fact, as I will argue later, little evidence favouring the hypothesis that maize was the central, or even an important, cultigen during the Barra and Ocós phases It will only be noted here that, while grinding stones such as mortars or metates are rare in Ocós levels at both Altamira and La Victoria, these utensils are *wholly* absent in Barra levels at Altamira.

The case is different at the inland sites At Chiapa de Corzo, definite manos and metates have been obtained from Cotorra levels in pit 50, while other grinding implements such as pestles or cassava pounders are absent (Dixon 1959: 19 and fig. 53). Also absent at Chiapa de Corzo are the small unworked obsidian flakes so prominent at Altamira and La Victoria.

At nearby Santa Marta Rock Shelter, faunal remains of deer, peccary, tapir, armadillo, squirrel, and iguana have been recovered from the Cotorra level (Level 5, or occupation 6) in association with obsidian blades and Brushed Tecomate pottery (MacNeish & Peterson 1962: 11 and 17) Most significantly, though, this same level yielded large amounts of corn pollen, indicating that, at a period coeval with the middle of the Ocós phase, maize farming had emerged as a central subsistence activity in central Chiapas. It is difficult to imagine that the early Cotorra inhabitants of Chiapa de Corzo were not also predominantly maize farmers, inasmuch as their environment was essentially the same as, and indeed was quite adjacent to, that of Santa Marta. While MacNeish and Peterson do not discuss the presence or absence of grinding tools in Level 5, the manos and metates from corresponding levels at Chiapa de Corzo have recently been described by Lee (1969: 114–19). These metates all seem to be of the closed-ended limited-trough type, and find no stylistic counterparts in Early Formative levels at either Altamira or La Victoria. A metate from the Cuadros occupation at Salinas la Blanca is rather more similar to those from Chiapa de Corzo (Coe & Flannery 1967: 63 and pl. 21).

Faunal and artefactual evidence, then, as well as fossil pollen samples, appear to confirm the hypothesis suggested by ceramic variations. Although there is as yet no ceramic material from central Chiapas which is temporally comparable to that

from the lowland Barra phase, Barra levels at Altamira clearly lack those features which reveal the nature of subsistence in the earliest known Early Formative complexes at Chiapa de Corzo and Santa Marta. The Barra component has yielded neither manos, metates, maize pollen, maize cob impressions, nor extensive faunal remains. The same is true at Altamira for the Ocós occupation, with the possible exception of a very few mano fragments; at La Victoria, the earliest levels have yielded several small metate fragments, but again, faunal remains are scarce and evidence for maize cultivation is scarcer. Shellfish, however, are plentiful and certainly accounted for a sizeable portion of the Early Formative diet at La Victoria. Thus, by the fourteenth century B C. (if MacNeish and Peterson's dates are accurate), interior Chiapas had adopted a pattern of subsistence similar to that of the rest of highland Mexico, based (primarily, or at least in large part) upon maize cultivation supplemented by extensive seasonal hunting. Yet, curiously, the coeval peoples of the Soconusco coast, whatever their mode of subsistence, appear to have lacked such an orientation although they were producing the most spectacular material goods in southern Mesoamerica Little can be known with certainty about the particular adaptations which supported this development However, the application of certain ethnographic analogs, together with a more extensive analysis of certain artefacts from the coastal sites, provides a set of reasonable and explicit hypotheses, which may be tested by further excavation.

V

Patterns of early formative subsistence in coastal Soconusco: artefactual evidence

Of all the cultural remains at the coastal sites of La Victoria and Altamira, probably the most valuable for the reconstruction of Early Formative subsistence patterns are the stone tools While stone artefacts are limited in numbers through all levels at both sites, a range of products are represented, including manos, metates, mortars, pestles, grinders, pounders, hammerstones, celts, axes, jade beads, pumice abraders, obsidian blades and scrapers, and a type of small obsidian flake. The grinding tools have been briefly discussed already, and will be mentioned again later, it is felt that little information (concerning subsistence techniques) can be derived from discussion of the few axes, celts, or beads. It can be shown, however, that the pounding tools and small obsidian flakes listed above contain useful information for this study. Provenience data used in the following discussion of the flakes from Altamira are given by Green & Lowe (1967 91 and 128); for the provenience of flakes from La Victoria, see Coe (1961: 35–46 and fig. 5).

The La Victoria flakes (classified as 'unworked obsidian flakes') were not described in the final report, although the few artefacts from Formative levels analysed as obsidian scrapers and spokeshaves (see Coe 1961: fig. 51–j) were said to be 'black obsidian of poor quality, often banded' (1961: 103). Professor Coe informs me that the specimens were virtually identical in form to those from comparable levels at Altamira, and has permitted me to examine a sample of fourteen of the Altamira specimens presently in the care of the Anthropology Department at Yale.

According to Green and Lowe (1967 128), the Altamira flakes are grey with black banding, and most show no signs of use. In the sample which I examined, all



PLATE I Grating edge of Test Flake E



PLATE 2 Grating edge of Altamira Flake OB 4309

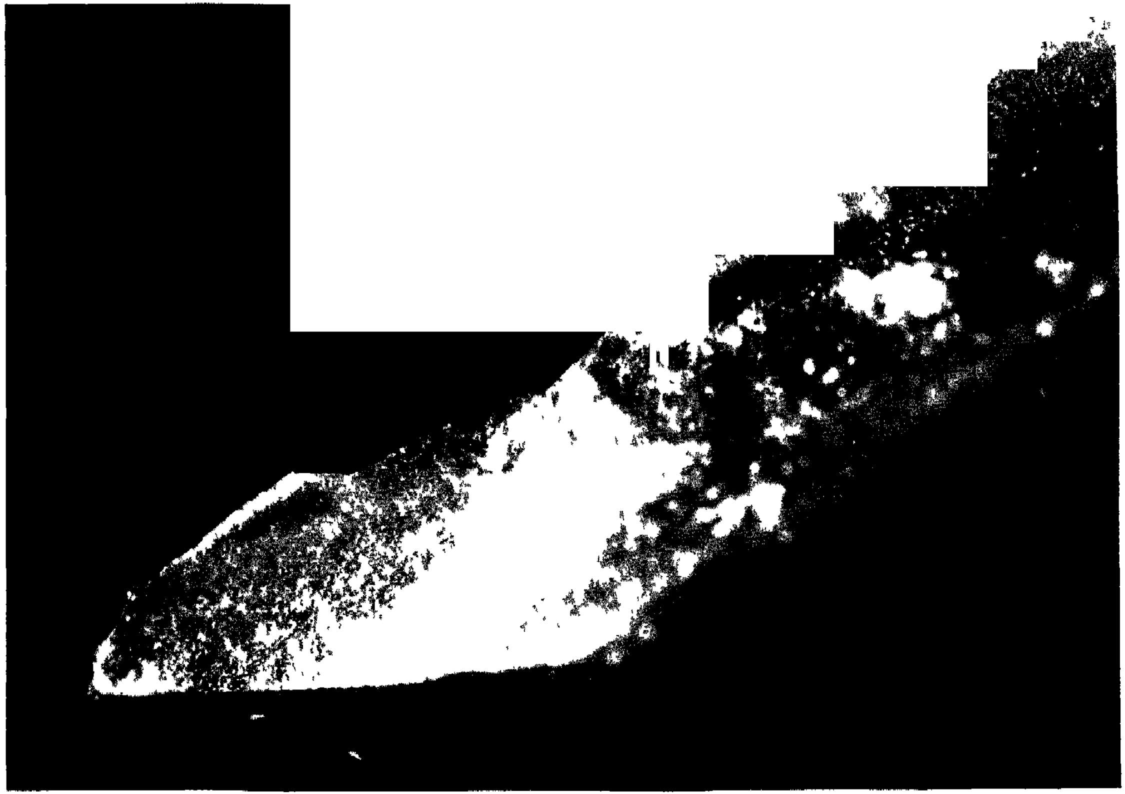


Plate 3 Unmodified edge of unused Flake N



PLATE 4 Edge of control flake R, used in scraping bone

flakes were of dark grey translucent or black obsidian with black or grey banding present in most instances; a few of the specimens were marked by one or two inclusions of white quartz or plagioclase. All flakes measured less than 3 cm in maximum extension, with a mean width of just over 1 cm. Of the fourteen flakes, however, only four showed apparent signs of use under a magnifying glass; three may have been intentionally retouched on part of one side. Scrapings of minute encrustations from another specimen (OB4313) proved to be fine medium-brown clay.

Of greater interest are the areal and stratigraphical distributions of the flakes at the two sites. Green and Lowe (1967) have suggested that obsidian flaking at Altamira was predominately a Barra phase pursuit whose importance declined in later periods. The simple decrease in numbers of flakes from earlier to later levels is, of course, no measurement of this decline, inasmuch as neither the length of occupation nor the number of inhabitants for each level can be stated with sufficient accuracy. However, by assuming that the number of ceramic remains in a given level broadly reflects population size in relation to other levels, and given the added premiss that the obsidian flakes were in everyday household use, it is possible to derive relative measures of the importance of the flakes (and, thus, of the activity in which they were involved) through time. In table 1 such a measurement is

TABLE 1 Variation in use of obsidian flakes at Altamira and La Victoria.

Phase	Altamira, Trench 19-H	La Victoria, Unit E-4
Barra	No of flakes 265 No of sherds: 132 % flakes/sherds 200·75%	—
Ocós	No of flakes 216 No of sherds 315 % flakes/sherds 68 57%	No of flakes: 92 No of sherds. 622 % flakes/sherds: 14 79%
Cuadros	—	—
Jocotal	No of flakes 218 No of sherds· 516 % flakes/sherds: 42 24%	—
Conchas I	—	No. of flakes: 52 No. of sherds· 2276 % flakes/sherds 2 28%
Conchas II	—	No of flakes 95 No of sherds: 10327 % flakes/sherds: 0 92%

presented, with the number of flakes expressed as a percentage of the number of potsherds in Ocós and Conchas levels in one pit at La Victoria, and in Barra, Ocós, and Jocotal levels in Trench 19-H at Altamira (these were the only units for which sufficient data were published to permit such a comparison). These figures reveal a constant decline in the per capita manufacture (or use) of the flakes through time at

both sites A concomitant decline in the activities with which the flakes were associated also seems to be indicated.

It was hoped that associations of the flakes with other artefact types would suggest the kind of role the flakes fulfilled, if any, in the Early Formative subsistence system. At La Victoria, while the flakes did occur in greater numbers towards the centre of Mound E (in Ocós levels), so did other artefacts. It appears, that is, that the flakes were randomly associated with other artefacts of comparable date, from pit to pit. Given that the site could not have been excavated with this particular problem in mind, however, there is very little basis for any assertion about the horizontal distribution of the flakes with respect to other remains. At Altamira, where a number of mounds were tested, Lowe concluded that

> There is no doubt that obsidian flaking was a Barra phase industry and that the Mound 19 surface was the centre of this activity then and later (Green & Lowe 1967. 128).

Yet whether the flakes were evenly distributed across Mound 19 at any level cannot be told, inasmuch as only one trench was excavated there.

The data are little more enlightening with respect to artefactual associations of the flakes through time. Lowe has claimed that

> At both sides [Altamira and La Victoria] abundant obsidian flakes fall off in quantity rather sharply after the Ocós phase, and true metates make their appearance in the area at this time, approximately 1100 B C (1967 59).

While this comment is very tempting for my analysis, I suspect that in just this one case Lowe has asked a little more of his data than they can yield. It is true that the Early Formative milling stones at both sites are quite crude; as Coe (1961 · 115) has noted, they can 'hardly be called metates.' Yet the numbers of metates in all levels at La Victoria are so few that statements as to their relative abundances must be considered tentative Moreover, when the levels are grouped in terms of phases, the decline in obsidian flakes appears rather more constant than abrupt; and the importance of milling stones, expressed as a percentage of the number of potsherds, fluctuates from level to level, rather than increasing steadily as Lowe implies.

Regrettably, then, there is little direct internal evidence which suggests a plausible interpretation of the obsidian flakes However, if we consider the possible kinds of activities in which they may have been utilised, it is possible at least to limit the range of interpretation. First, it is virtually inconceivable that the flakes served any religious function, for they are in no way stylised. I suppose that it is possible that they were set into clubs or 'swords' which were similar in function if not in design to the later *macqauitl* of the Mexicans, although I know of no occurrence of such a weapon outside central Mexico. It is infinitely more probable that the flakes were used in the preparation of food As was noted earlier, hunting was singularly unimportant on the Soconusco coast in the Early Formative, and marine fauna seem to have occupied only a minor position in subsistence at Altamira. It is therefore unlikely that the flakes were used in the preparation of meat products. That the flakes were not debitage is also clear, given the total absence of any other obsidian artefacts in Barra and Ocós levels (Coe 1961: 115 and 38–46, Green & Lowe 1967. 57 and 128).

Resulting from this speculative process of elimination is the indication that the flakes were used in the preparation of vegetable foods. Messrs. Coe (personal communication) and Lowe (Green & Lowe 1967: 59) have suggested manioc graters as the most likely candidates. While neither manioc graters nor manioc are widely distributed in the Mesoamerican highlands, both occur with some frequency along the Caribbean and Pacific coasts south of central Mexico. Although manioc (*Manihot utilissima*) is not the staple crop anywhere in Mesoamerica today, it is widely grown throughout the humid lowlands. Farther south, manioc production expands, and achieves dominance throughout the tropical forests of South America, as well as in the Caribbean. The antiquity of manioc cultivation, which will be established in a later section of this article, is sufficiently great to accommodate the interpretation proposed above; it is our task to assess the probability of this hypothesis.

VI

Following suggestions by Professors M D. Coe and D. W Lathrap, an experiment was designed to test the hypothesis that the sample flakes from Altamira (and, inferentially, those from La Victoria) were remains from manioc graters. Although I was initially sceptical that detectable wear patterns could be impressed upon obsidian by manioc grating, it was clear that, if patterns distinctive to that activity could be established, our hypothesis might emerge from the realm of conjecture.

As was noted earlier, significant macroscopic patterns of wear were not detectable on the fourteen sample flakes from Altamira. Microscopic examination was therefore necessary, so that museum specimens, which could not be disassembled from their grater boards, were of little aid A small block of medium grey translucent obsidian, of unknown provenience, was obtained through the courtesy of Dr Richard Armstrong of the Department of Geology at Yale. From this core a set of seventy-five flakes were produced which duplicated in size and morphology the range of variation of the sample flakes from Altamira. Two pieces of medium-weight balsa wood, each measuring 38 × 13 cm, were laminated together to produce a grater board 2 cm thick Using a piece of quartzite as a 'hammerstone', sixty of the newly produced flakes were driven into the board to depths of about half their maximum extensions. Spacing between the flakes averaged 0·5 cm.

It was found that, because of the size and irregular shapes of the flakes, manioc could be effectively grated only by unidirectional (rather than 'back-and-forth') motion across the grater surface In accord with the practice followed in most contemporary communities, the tubers were peeled before being grated Following these procedures, 0·75 pounds of manioc could be grated every twenty minutes (or 2·25 pounds per hour). Tubers were grated for a total period of four hours.

Ten of these flakes which most closely resembled those from Altamira were selected for analysis (the amount of time involved in the microscopic study of the flakes did not permit examination of a larger sample). Ten of the fifteen unused flakes were selected as control specimens and a final, single flake was used to scrape raw meat from a steak bone. The test flakes were initially examined with a binocular microscope under magnification of 15 × , no pattern of edge wear or damage

was detectable When magnification was increased to 30×, however, a distinctive pattern was isolated on nine of the ten test flakes. Moreover, in every case in which it occurred, the pattern was found *only* on one side of one edge of a flake, and invariably these were the sides and edges which had absorbed the direct force of grating. I was further gratified to determine that this, or any, pattern of edge wear occurred on *none* of the control flakes which had not been used in grating. It must be admitted that the comparison and identification of wear patterns in a case such as this is somewhat subjective In any case, an examination of the Altamira sample showed, in my judgement, that the wear pattern established for the test flakes was present on eleven of the fourteen Early Formative specimens (i.e., on 78·6 per cent. of the Altamira flakes). Moreover, in all but one instance, the pattern was present on only one side of one edge of each flake.

The pattern is difficult to describe, but may be characterised as a broken series of small 'pockets' which mark the removal of minute chips along the very edge of a flake Frequently the grating surface approaching the grating edge has been worn to a smooth, flat plane. Several of the flakes were photographed under magnification of 30× with the expert aid of David Holbrook, photographer for the Department of Biology at Yale. The results are pictured in plates 1–4. Plate 1 exemplifies the edge-wear pattern detected in the text flakes Plate 2 pictures a flake from the Altamira sample which clearly displays both the edge-wear pattern and the 'inclined plane' effect mentioned earlier (see also fig. 2). Plate 3 displays the smooth, unmodified edge which characterised all the control flakes, while plate 4 reveals the edge damage caused by bone scraping. The deep, irregular pattern on this last flake is quite dissimilar from those exhibited in plates 1 and 2.

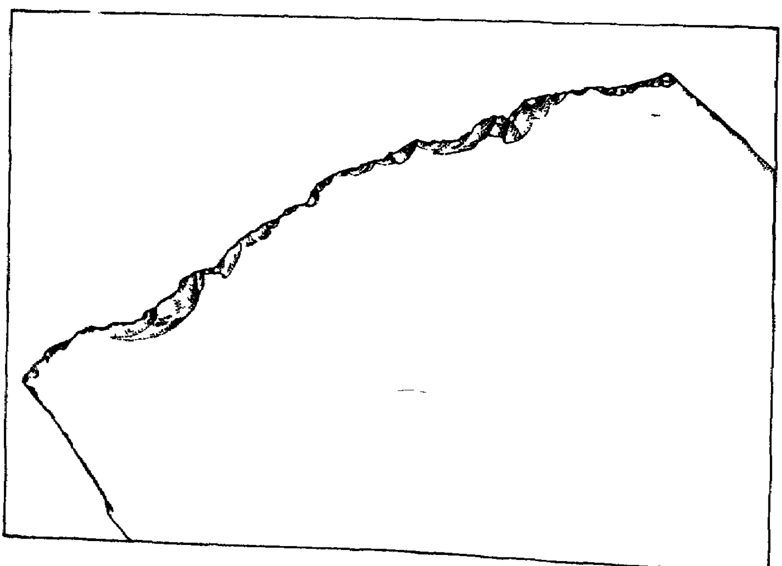


FIGURE 3. Grating edge of Altamira flake OB 4309.

Recently, with permission of excavators Robert and Judith Zeitlin, Hallie Whitlock (Ms.) has studied a larger sample of eighty-six obsidian flakes from Ocós, Cuadros, and Jocotal phase levels at the site of Laguna Zope in coastal Oaxaca. The dimensions of flakes in the Laguna Zope sample are (maximum dimensions 1·0–2·7 cm, maximum thickness 0·2–0·6 cm) comparable with those of the Altamira flakes. Under magnification of 15 ×, the 'grating pattern' as described above, was clearly distinguishable on 38 per cent. of the Jocotal specimens, on 47 per cent. of those from Cuadros levels, and 50 per cent. of the Ocós flakes.

The evidence cited above argues convincingly that the flakes from Altamira and Laguna Zope (and presumably those from La Victoria) were used in the grating of some soft vegetable with a density and texture comparable with that of manioc. Our hypothesis may benefit from a further survey of manioc production and preparation in contemporary Middle American communities.

VII

Use of obsidian in manioc graters, at least since contact, seems to have been extremely rare. I have been unable to locate even one occurrence of this material in graters which can be documented ethnographically. As Lowe (Green & Lowe 1967: 128), has noted, 'the brittle nature of obsidian makes it less suitable for such a utilisation,' and flint or jasper are the most popular materials for manioc graters, at least in the Caribbean and South America. Yet, where cryptocrystalline quartzes are unavailable, the range of innovation in flake production is extensive. The Waiwai of northern Brazil produce a flake similar in form, though not in size or composition, to those considered here. Clifford Evans had kindly provided me with a small sample of the Waiwai flakes; an analysis of the flakes, performed by Dr Evans's staff at the National Museum, showed them not to be obsidian (C. Evans, personal communication). Yde (1965: 34) has described the Waiwai material as

> greyish-black, finely crystalline, and hard rock which breaks along irregular cracks with no predominant direction. The fracture is slightly uneven, in some places splintered. Some surfaces are curved, more rarely they are plane. No parallel structure is apparent.

These flakes appear to me to be basaltic, and are only about half the size of those from Altamira and La Victoria. Only a few hundred kilometres to the south of the Waiwai, however, the use of stone is absent in manioc preparation. The Trumai of central Brazil, for example, peel the tubers with pelycypod shells and reduce them to pulp on graters constructed of bone fragments set into a soft wood (Murphy & Quain 1966: 30). To my knowledge, the northernmost occurrence of the manioc grater (outside the Greater Antilles) is in Belize, where tiny white quartz chips are embedded in planks of mahogany measuring about 35 × 80 cm (Taylor 1951: 63 and pl. 13). In most of the Maya lowlands, however, and in southern Guatemala, the manioc grater is conspicuously absent in the present day, even though manioc frequently occurs there as a secondary crop. In the northern lowlands tubers are widely grown, but, as Villa Rojas (1945: 55) notes,

> Cassava, squash, sweet potatoes, and yams are not given any special preparation but are simply boiled or, more frequently, roasted in the earth oven.

Only 'sweet' varieties of manioc, or those requiring no special processing for the removal of prussic acid, are grown in eastern Yucatan (Villa Rojas 1945). 'Bitter' manioc also appears to be absent as a cultigen in much of Guatemala. According to McBryde (1947: 36 and 139), manioc occurs in limited quantities throughout the highlands of western Guatemala at altitudes below 1600 m. Highland manioc, however, is boiled, in the manner popular in Yucatan, rather than grated. The case is similar in the Chorti highlands of eastern Guatemala, where again only 'sweet' varieties are cultivated, and the root is cut into cubes and boiled (Wisdom 1940: 94). The Chorti also produce a starch from the roots, which is used primarily to stiffen clothes. Yet, interestingly enough, roots which are grated for this purpose are ground on *metates* (Wisdom 1940: 56); manioc graters are not used. Thus, to Evans and Meggars' (1960 341–3) note that manioc griddles need not accompany the use of the root, we may add that manioc graters are also not natural concomitants of cassava cultivation It is of further note that metates are not necessarily indicators for the presence of maize cultivation, even during times when maize is known to have been domesticated (admittedly, the Chorti do grow maize as well as manioc). True manioc graters themselves are quite rare wherever manioc occurs as a secondary crop, throughout Central America, and in parts of northwestern South America. It is this same area in which 'sweet' manioc is dominant, as it probably was even at the time of European contact. Oviedo (1851–55. 270) suggests that only 'sweet' varieties were utilised in Guatemala, and Reichel-Dolmatoff (1961. 85) claims that even the lowland tribes of Colombia ignored poisonous varieties of the plant. Both widespread use of 'bitter' manioc and the ubiquitous presence of manioc graters are phenomena peculiar to the Caribbean and Amazonian culture areas

This brief survey has added little support to our evaluation of the flakes from Altamira and La Victoria, but the hypothesis is certainly not negated. It has been shown that manioc cultivation persists throughout most of southern Mesoamerica, though the root is usually boiled rather than grated Our interpretation is also favoured by McBryde's statement that the plant is most widely grown in lowland areas, and that

> Starch balls, about 1½ cm in diameter, are prepared from it in the Lowlands, and these appear in large sackloads in Highland markets, brought by itinerant merchants (McBryde 1947: 139)

Preparation of such products would, it seems, require the use of graters.

The wide range of variation in the construction of manioc graters also favours the hypothesis, especially if trade between the Soconusco and the Guatemala highlands was as extensive during the Early Formative as it is in the present day (since the highlands are a central source of obsidian). Coe (1961: 128) has pointed to ceramic affinities between the highlands and Soconusco during the Conchas II subphase which suggests that the two areas were in extensive contact with each other, and that contact may have been initiated by the ancient highland desire for the cacao which grew abundantly in the Soconusco (McBryde 1947: 33). As will be shown below, there are no major agricultural products which the highlanders could exchange for goods from the *Costa* (since the *Costa* produces all but one of the major food cultigens of the highlands). It does not seem unlikely, therefore, that the highlands traded their valuable minerals, especially obsidian, to the lowlands in

return for agricultural and marine products, and that the ready availability of obsidian as a trade good promoted its use in food preparation at Altamira and La Victoria in preference to other stone materials.

The 'manos' from Early Formative levels at the coastal sites deserve brief mention here. Of the three stones from Ocós levels at La Victoria which were classified as manos, Coe (1961: 122) writes that 'The ends ... show use as pestles or pounders'. During Conchas times, milling stones again appear to have doubled as pounders (Coe 1961: 106 and fig. 61). Specimens from Altamira are comparable in form (Green & Lowe 1967: 29 and 128), although wear patterns are not discussed in the final report. Lee (1969: 115) claims that manos from the Cotorra phase at Chiapa de Corzo are of the same type as those found by Coe (1961: fig. 41), but it is clear that the Chiapa de Corzo specimens (Lee 1969: fig 70) are rather square-ended, while those from La Victoria and Altamira are more ovoid. Noticeably, however, Lee (1969: 114) says that several of his specimens exhibit 'roughly pitted surfaces'; it seems likely, then, that they too were used as pounders or grinders. At Santa Marta Cave, MacNeish and Peterson (1962: 28 and pl 5) obtained from Cotorra levels two 'pebble hammers' with evidence of pecking on one end, while manos from the same levels showed no such signs of use although they did exhibit a certain sheen, presumably from use in grinding. That all these tools were utilised in the preparation of vegetable foods is beyond question, and when I learned that the Waiwai employ virtually identical implements as cassava pounders (Yde 1965: 35–6) I was quick to embrace a similar interpretation of at least some of the Early Formative Soconusco manos, and to view this as confirming evidence for the flake hypothesis. I have since reconsidered this position and concluded that the data are far too sketchy, and the range of possible uses of the manos too great, to offer even tentative support to such an interpretation. Fine microscopic analysis of the manos may reveal more information about their functions; lacking such analysis, we can say no more than that they were utilised in the preparation of vegetable foods and that, given the areal distribution of similar end-wear patterns, it is possible that their primary use was in the grinding of maize (which was probably the major cultigen in the Central Depression of Chiapas). If the manos do indicate the processing of maize, then their occurrence at the lowland sites also argues for the cultivation of maize in coastal Soconusco during the Ocós phase. It may be significant, however, that the Barra levels at Altamira, which contained the greatest number of obsidian flakes at the site, yielded *no* grinding tools at all (Green & Lowe 1967: 57).

It may seem at this point that the contribution of this rather dry artefactual analysis towards the establishment of my central hypotheses has not been great. Yet, given only the most indirect sorts of evidence, it was clear from the start that, if the inquiry were to proceed, it would be necessary to examine each bit of evidence as completely as possible for whatever it might contribute. It seems to me that a number of patterns are emerging here which together will comprise a probabilistic picture of Early Formative subsistence in the Soconusco. The completion of that picture, however, requires attention to a further body of evidence with which I have not yet dealt.

VIII

Early Formative subsistence in coastal Soconusco: palaeoecological evidence

Climatic data gathered at Ayutla (some 17 km to the north of La Victoria) and from Champérico (about 38 km southeast of La Victoria) show that temperatures are higher, and precipitation lower, along the coast than on the *Boca Costa*. In a twenty year period, the mean annual rainfall at Champerico (altitude 3 m) was 272 mm less than at Ayutla (altitude 40 m) (Simmons *et al.* n.d.: 186 and 212). Further north, in the mountains, the increase is startling the station at Belen, Malacatan, reported 2·6 times the annual precipitation of Ayutla (p. 212). Monthly rainfall averages for these stations indicate that, to the fortune of the coastal farmers, about 85 per cent. of the year's precipitation falls during the months June to October inclusive. At Champerico, rainfall from November to March is quite low, averaging only about 7 mm per month. Yet Coe (1961 13) reports that farmers on the coast frequently obtain three maize harvests annually, one of which is in March. The climate and soils of the *Costa* have been adequately described by Simmons *et al.* (n.d.) and summarised by Coe (1961: 10–14) and Coe & Flannery (1967: 9–11) and do not require repetition here. The primary environmental change since the Early Formative seems to have been the seaward shift of coastal micro-environments reported by Coe (1961. 111) With respect to maize cultivation, it is sufficient to note here that, despite the relative paucity of rainfall along the coast, the land behind the estuaries will support at least two maize harvests annually.

The actual productivity, as well as the emphasis upon maize, seems to vary from community to community. There is some reason to believe that, despite the importance of maize and the good yields reported by Coe (1961: 13–14) for the land around La Victoria, production of even the same varieties would have been lower during the Early Formative. Remembering that the site is presently almost 3 km farther from the brackish estuary than it was during the Ocós phase, it is interesting to note McBryde's comment that

> An informant at Tahuesco, a Pacific shore settlement, said that probably less than half of the inhabitants there planted maize, and, of those, some went 'higher up' for planting Much of the outer plain is flooded during the rainy season Some of this lack of interest in agriculture is, however, due to the almost universal participation of the populace in salt-making and fishing activities which occupy much of their time (McBryde 1947: 22).

Because the coast itself was probably more than 2 km closer to La Victoria in about 1500 B C, it is likely that maize cultivation in the immediate vicinity (i.e., within a kilometre or so) of the village was correspondingly lower than today. This is especially so when the lower productivity of early maize is considered.

Minor cultigens unquestionably included cotton (Coe 1961: 115) and may well have included avocado, direct remains of which have been found in Cuadros levels at Salinas la Blanca (Coe & Flannery 1967: 72).

At La Victoria, extensive exploitation of estuarine molluscs is evident during the latter part of the Ocós occupation (Coe 1961: 114–15 and 144). Shell occurs only sparsely, however, in earlier Ocós levels, although it is difficult to imagine that marine fauna were not equally important during the earlier period. At Altamira, there is no evidence of the use ~~of~~ shellfish or other aquatic fauna which might serve as indicators of an estuarine-oriented economy (Green & Lowe 1967 58) As

mentioned earlier, the virtual absence of other faunal remains at both sites suggests that hunting occupied an insignificant role in the diet.

It is the Barra component at Altamira which presents the greatest challenge to the reconstruction of subsistence patterns, for that component is marked largely by the absence of any reliable indicators. Terrestrial fauna, shellfish, and floral remains are not represented. Neither were there found metates, manos, comales, mortars, pestles, or any other grinding implements. Indeed, the only evidence which was found occurred in the form of the small obsidian flakes discussed earlier, and these occur abundantly in Barra levels. If our interpretation of the flakes is correct, then it is quite possible that the earliest Formative communities in lowland Soconusco relied heavily upon cassava production to the exclusion of hunting, fishing, or maize cultivation, and that other subsistence activities were added through the years.

Coe (1961: 13) has recorded the cultivation of four varieties of 'sweet' manioc near La Victoria, and coastal Soconusco appears to present ideal growth conditions for the plant, at least in the tropical forest areas *Manihot* is well-adapted (and perhaps best-adapted) to areas below 2000 m altitude where rainfall varies considerably during the year (Jones 1959: 16). The value of manioc has been underscored in an interesting way by Burkill (1951: 448).

> [manioc] cannot be absolved from the ruin of more good land than any other plant because it is the first thing that the indigent man plants when he clears forest for cultivation, and it so rapidly exhausts the soil that the circle of felling moves the faster . . in Malaya . . it is the first thing that a family of one of the wild tribes commits to the hazards of their inadequate plantations and is their bread to the extent to which they can come by it

It is unlikely that Burkill's characterisation of manioc as a destroyer of soils would hold up under examination of its long tenure as the staple crop of much of South America. That the plant is found growing on poor soils is probably due to the fact that it 'can produce an economic crop on soils so depleted that they have become unsuitable for other cultivation' (Jones 1959: 16) However, Burkill did correctly emphasise the value of manioc as a source of food energy. In central Brazil, one hectare of cassava usually produces over 15 tons of roots per harvest, with a total yield of 14·2 million calories, while a field of comparable size planted in native maize yields only about 4·4 million calories (Hill 1952: 364). Moreover, although it is widely known that the roots of the plant are rather low in protein content, the leaves contain more protein, and are occasionally eaten as greens (Rogers & Appan 1970: 72).

If the Barra phase represents the initial period of contact between the Soconusco coast and areas to the south, and if Carl Sauer (1952) was correct in his prediction that the earliest centre of cassava cultivation will be found in northwestern South America, it is not unlikely that Barra peoples had borrowed more than ceramic designs from the Ecuadorians. Although Coe and Flannery (1967: 104) have suggested that maize had been traded down to the Gulf coastal lowlands by 2000 B.C., its presence or importance on the Pacific coast at that time remains unestablished. Even at Santa Marta Cave, the first definite evidence of maize cultivation appeared in levels dated to the fourteenth century B.C. It is therefore a distinct possibility that maize cultivation had not yet reached the southern Pacific coast by Barra times; the absence of grinding utensils in Barra levels at Altamira seems to

corroborate this view. It is also possible, as Coe (1961: 126) implies, that manioc was cultivated in central Honduras as early as 2000 B.C., so that long-range diffusion of the plant need not be posited to account for its presence in the coastal lowlands during the Barra phase.

IX

Conclusions

While large portions of the evidence which a definitive consideration would require are still missing, we have been fortunate to find that the hypotheses suggested by the variety of available data are mutually corroboratory. An examination of extra-areal ceramic affinities found that Early Formative coastal communities enjoyed intermittent contact with central Chiapas as well as with north-western South America Yet reasonably clear distinctions could be drawn between coeval remains from the coast and those from inland sites Stylistic variations in the construction of stone tools were also greatest between the coast and the interior during the Barra and Ocós phases, while those of the Cuadros and Jocotal phases conformed more closely to the specimens from Santa Marta and Chiapa de Corzo. Ceramics and grinding tools therefore indicated that the coast had maintained a cultural autonomy from the interior during Barra and Ocós times, but that it merged with, or succumbed to the influence of, southern Mexico during the late Early Formative.

Although manioc graters are most commonly associated with the cultivation of 'bitter' varieties of the plant, while 'sweet' manioc is much preferred throughout Middle America, the obsidian flakes from La Victoria and Altamira still seem most amenable to analysis as grater flakes Highly poisonous cassava does grow, and is occasionally cultivated, in Guatemala, and the popularity of these varieties in South America may account in part for their presence in the Soconusco during the phases considered here The possible manioc griddles from Yarumela I levels might confirm this hypothesis, since griddles also seem to be associates of 'bitter' manioc. Manioc graters are not, of course, tied by necessity to 'bitter' varieties, since the graters only provide a convenient means of reducing the root to pulp

NOTE

I should like to express my gratitude to Dr Michael D Coe for his helpful advice and encouragement, and for reading a draft of the paper Special thanks are also due to Mr Robert Zeitlin and Mrs Judith Zeitlin for making their artefactual materials available for study, and to Miss Hallie Whitlock whose patient and detailed analysis of wear patterns on obsidian from Laguna Zope has contributed substantially to the data base of this article I should also like to thank and recognise the skills of Mr Carlos Aroestegui, Mr David Holbrook, and Miss Susan Klein for their production of drawings and photographs contained herein.

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MISINFORMATION IN VERBAL ACCOUNTS: SOME FUNDAMENTAL CONSIDERATIONS

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Ethnographers often rely heavily on the verbal reports offered by the people they study. A detailed consideration of the problems inherent in such reliance is presented. The 'truth' about the phenomena under investigation is said to be 'filtered' in passing through the informant, and an attempt is made to specify the nature of the filters involved. Four kinds of information are considered: 1) Information about external, objective events, 2) information about 'social facts' which have their bases in interpersonal understandings, such as the rules of a game; 3) information about an informant's own internal states, 4) paradoxical information. Certain relevant philosophical issues are also discussed. It is suggested that by increasing their awareness and understanding of the sources of misinformation in verbal accounts, ethnographers will be better able to assess the accuracy of their informants' accounts. Since the giving and receiving of verbal accounts are everyday activities, an appreciation of the processes described will also lead to a more thorough comprehension of the nature of social interaction.

I

Information and misinformation

A revered precept of pedagogical anthropology, less honoured perhaps in practice than in the pulpit, is that the rules for behaviour enunciated by informants should not be accepted uncritically as descriptions of the way natives actually behave. For example, anthropologists have discovered, occasionally to their intense chagrin, that 'prescriptive' marriage rules sometimes have more to do with the way people talk about marriage than with the way they marry. But stated rules are certain to be only a small subset of the total set of verbal data collected and the cautious attitude recommended in regard to the subset might appropriately be adopted when dealing with any member of the set. In general, the relation between an informant's statements and the information which the statements purport to convey should be taken as problematic. The problem is far from trivial, since a great deal of what most ethnographers 'know' about their communities, and present as fact, is based on informants' accounts rather than on first-hand knowledge. In a previous paper (Bilmes in press), I attempted to demonstrate that misinformation and ambiguity are pervasive features of Northern Thai village verbal interaction. In this article, I will discuss, in a more general way, the nature and sources of such misinformation.

The discussion depends on two assumptions. First, I assume that there is a distinction between accurate information and misinformation, that there is an ultimate reality to which statements can be referred in order to ascertain whether or not they are true. This assumption makes it possible to *talk about*, and make *probabilistic assessments* of, the truth or untruth of assertions even when we have no

Man (N.S.) 10, 60–71.

direct access to the reality on which they are based. We thus take into account what Blumer (1969) calls the 'obdurate character of the empirical world.' Although the world exists for us only in our images of it, sometimes the images prove to be inadequate, and we are forced to revise our views. This leads us to postulate an underlying reality which does not bend to our images of it, and my discussion is based on this postulate. Without it, the term 'misinformation' would be unintelligible. The assumption is being used to help set the terms of this discussion and is not to be taken as an assertion about the ultimate nature of things.

In an unpublished paper, Garfinkel (n.d.) compares the 'correspondence' view of reality, which is essentially the one on which I have based my first assumption, with the phenomenological 'congruence' theory.

> The leading premise of the 'congruence' theory of reality is that the perceived object of the 'outer world' is the concrete object, and that the two terms, 'perceived object' and 'concrete object' are synonymous and interchangeable. Rather than there being a world of concrete objects which a theory cuts this way and that, the view holds that the cake is constituted in the very act of cutting No cutting, no cake, there being no reality out there that is approximated since the world in this view is just as it appears' (p 5).

These two views of reality can be at least partly reconciled by adding to the congruence theory the idea of a 'precedent reality', which may be said to exist in the mind of an ideal observer. An example: at one point during the course of my fieldwork, two Government health officials came to the research village to give cholera inoculations. They located themselves at the village temple, and the villagers went there for their shots I was present from the moment they arrived until they left, and I counted all the villagers that came for inoculations. Thirty-six per cent. of all the persons in the village came. I later asked a number of respondents how many of the villagers went. When I asked respondent X, he said about 70 per cent.

Now, it is my presumption that if I had his knowledge of the situation *in addition to* my own, I still would have held to the view that only 36 per cent. went. On the other hand, if he had had my knowledge of the situation, i e , if he had been there and counted as I did, he would have 'known' that fewer than half the villagers had gone, and this knowledge would have 'taken precedence' over any rumours he might have heard or any previously-held expectations. This is to say that I had access to a 'precedent reality', that *anyone* who had been where I was on that day, and who could and did count, as I did; the number of persons coming for shots, would have found my knowledge and not X's to be 'factual'. A precedent reality invests the experiencer with 'final' knowledge, i.e , a precedent reality is recognised as having precedence over all other possible 'individual realities'. A true precedent reality cannot be contravened by any experience or testing procedure; if it is, it is immediately redefined as not-precedent-and-never-having-been-precedent. Precedent reality, in other words, consists in an agreement among all concerned as to what sorts of evidence and observational procedures give one direct and incontrovertible knowledge of the 'truth'.

This approach may not, for those who hold to the congruence view, provide an acceptable compromise, since it seems to assume 'the possibility of a community between the actor and the observer,' and it is this assumption, Garfinkel claims, which is at the heart of the correspondence theory of reality. In reply, I would point

to the fact that if I had told X that I had been at the village temple from the time that the health officials arrived until the time that they left, and that I had counted the number of persons that were inoculated, he would have accepted my information as authoritative. If he did not accept my information, he would explain this in terms of some fault in my message—e g , it was a lie, or in me—e.g., I counted carelessly. But he would acknowledge that *if* I was not deliberately lying, and *if* he understood what I was trying to say, and *if* my memory was accurate, and *if* my procedures were correctly executed, then my statements would have been accepted as authoritative. That is, for certain questions at least, we both recognise and agree on the situations and procedures which provide the observer with a final warrant to make pronouncements about 'reality'. 'We must accept that there are no adequate grounds for establishing criteria of truth except the grounds that are employed to grant or concede it . .' (McHugh 1970. 329). Truth, then, is an accurate statement about the precedent reality which exists in the mind of an ideal observer—the features of the observer, his observational technique, and the nature of the observed event having been agreed on by all concerned.

I invoke 'the possibility of a community between the actor and the observer' on empirical grounds, because it seems to exist, without denying the interest of the question of how this community came to be I can learn to behave appropriately in any culture because, at some level, the world is given to us all in the same way, or at least in analogous ways, so that one way can be mapped onto another, feature for feature If the world is 'just as it appears,' then it is necessary to point out that, under certain conditions, and given certain procedures, it will appear the same to everyone (This, in fact, seems to be a rather weak statement of a position which might be taken by a phenomenologist. For example, here is Merleau-Ponty's opinion· '.. the perception of the world ... is the doing of pre-personal forms of consciousness, whose communication raises no problem . I am in no way distinguishable from an "other" consciousness, since we are immediately in touch with the world and since the world is, by definition, unique, being the system in which all truths cohere' (1967. 362)) I can see as many kinds of snow as the Eskimo if I follow his procedures for looking at it. An external standard for what is real seems to be necessary for, or at least universal in, human social intercourse. While accepting the point of view that this agreed-upon 'precedent reality' may not be 'real' in any ultimate or exclusive sense, I am nevertheless, for simplicity's sake, going to call it 'reality'

Garfinkel takes cognisance, in one way or another, of most of the points which I have made in the last few paragraphs. In fact, having pulled the rug out from under us, he later seems to slip it back into place 'Granted that there is knowledge that shows the peculiar ... property of being invariant with reference to the social status of the knower .. what is there propertywise in the structure of the attitudes that are constitutive of such an order of experience under which the experienced world shows such intriguing features' (p. 18)? He seems here to be acknowledging the achievability of a community of knowledge between actor and observer. Rather than doubting its (possible) existence, he adds (or, rather, says that Schutz adds) an admittedly important question—*how* is it possible? However, he sees this kind of knowledge as 'historically recent', a product of logico-empirical methodology, whereas I have suggested that it is fundamental and universal.

The first assumption was ontological—there are such things as facts. The second assumption (or perhaps it is merely an observation) is epistemological—although facts (whether they are conceived of as 'real', or as descriptions of precedent reality) exist, we have no final way of identifying them. We must be satisfied with probabilistic assessments and statements that are true 'for all practical purposes'. As our purposes change, however, we may find it necessary to re-evaluate our 'facts'. If every acceptable measure shows the matter to be as we think it is, if our image does not run foul of the obdurate character of reality, we take the thing as it is in our image—provisionally—to be a fact. The more we test it, the more we believe in it. But since new tests can be invented we must deal in probabilities rather than certainties. Our 'facts' remain tentative, our 'knowledge' approximate; our certainties are, in Husserl's phrase, 'good until further notice' (Factual judgements are necessary in the identification of accurate information. Misinformation, on the other hand, is more obtrusive. Although only a factual judgement can locate it exactly, a purely logical approach may be sufficient to reveal its presence in the neighbourhood. If we accept as a postulate that a proposition cannot be simultaneously true and not true, then, if we are presented with two contradictory statements, we can deduce that at least one of them is false That is, we assume that there is a single reality, even if we do not know what it is, that things are either 'one way or another'.)

II

Sources of misinformation in verbal data

I am assuming, then, that statements (normally) can be said to be either true or false, and that sometimes the truth value of a statement can be ascertained 'for all practical purposes', but never finally Let us now consider a case in which a researcher questions an informant about an event which the informant witnessed. We are dealing here with a question of external reality—what physical events occurred 'out there?' We want to draw from the informant's statements 'true' information about the event-as-it-occurred (as a videotape of the event might reveal it). Leaving aside the important matter of how we select and present our questions, we may ask, how have the events which the informant witnessed been 'filtered' in the process of coming through him to us? There is a sizeable literature relevant to this subject, and I will not attempt to review it here. Instead, I will simply try to indicate, in schematic form, some important sources of distortion. (Most of these points are familiar to social scientists and can be found in standard textbooks in the disciplines concerned, so references will be cited only where deemed appropriate.)

A. *Perceptual distortions and selective perception*

1. The informant cannot produce a complete description, since such a description would be endless. (A completely accurate report would be impossible, since there are inherent limits on the perceptual discriminations which humans can make. Also, it is not clear to me that a complete report would be possible even in principle; given that everything must be described in some way, is there any limit

to the ways in which an event can be described? When is the description complete?) He must, therefore, group the raw stimuli and select among them.

2. This grouping and selection begins with the act of perceiving. Gestalt psychology has demonstrated the highly structured nature of our perceptions. The structure may not only cause us to miss 'irrelevant' stimuli and to 'see' parts of the structure which are actually missing; in some cases it may bring about perceptual displacement, so that the stimulus is perceived, but not in the place where it actually occurred (e.g Fodor & Bever 1965).

3. The value of stimulus objects and their familiarity and meaning to the observer have been shown to bias perception.

4 There seem to be systematic differences in the sensitivity of differing personalities to certain kinds of stimuli, as well as differences in sensitivity due to the nature of the ongoing act in which the observer is engaged and due to the observer's ongoing interpretation of the event.

5. 'Norms of perception' arising in groups have been shown to affect the perceptions of individual members

6. Cultural differences have been shown to exist in the perception of optical illusions

7. Language seems to affect perception; the many names that Eskimos apply to snow, Arabs to camels, and Thais to rice, suggest that their perceptions (i.e., that which they *notice*) of these objects are more highly differentiated than those of, say, an American would be. These differences are not to be explained by differences in the inherent perceptual capabilities of persons from different groups Rather, it appears that, in learning a language, we learn to attend regularly to certain features of the objects referred to, and these features differ from one language to another. Linguistic relativists have suggested, in addition, that the grammatical structure and metaphorical usages of one's language have a pervasive effect on how one perceives the world, but the scientific basis for this assertion remains tenuous.

8. We may add to this list such 'attitude defence mechanisms' as the seeking of consonant information, and selective avoidance of dissonant information.

Given that the observer has in fact turned his attention to an event, it is not entirely clear as to whether selection and distortion occur in the perception of the event or in the memory of and production of responses to the event.

> Two steps of codification have occurred between external events and the individual's reaction to these events, and the observer has access only to the 'product' (in the mathematical sense) of the two steps when superimposed. From this product it is impossible to arrive at any knowledge of either stage as a separate process. . . The subject may have 'perceived' the events wrongly, or he may have translated correct perceptions into wrong actions. but which of these errors has occurred the external observer cannot tell. The question is unanswerable and therefore unreal' (Bateson 1966 422).

Bateson may have spoken too hastily on this matter. Rothblat & Pribram (1972) have analysed electrical brain activity in monkeys in an attempt to answer this question. They conclude (1972: 434) that 'both the response and the input filter theories are supported; during learning, selection occurs subsequent to analysis of stimulus attributes . . but then becomes encoded (by overlearning) in the input mechanism where it *can* act as a filter'. The implication, if we may generalise from

monkeys to men, is that our previous experience affects not merely the way we remember and respond, but the way we perceive.

B. *Retrospective distortion and selectivity*

1. Having perceived the event, the informant must interpret it (this is not to say that the interpretation of the event cannot be started before the event is completed) and draw on his memory of it in order to produce an account. Miller (1956) has cited evidence indicating that although persons can make fairly fine distinctions of stimuli along some given attribute dimension (e.g., pitch), there is a limit of approximately seven on the number of stimuli that he can discriminate simultaneously along one dimension. This holds no matter how easy we make it to distinguish each stimulus from the others. The problem appears to lie not in the subject's physiological capacities for discrimination of stimuli, but in his intellectual capacity—more specifically, in his ability to remember. Given that people cannot remember everything they have perceived, they are forced to select among and categorise their perceptions.

2. As the informant perceives the event, he interprets it, and it seems reasonable (if not tautological) to expect that those features of the event which are most significant to him are those which he is most likely to remember. The raising of an eyebrow may be recalled, the scratching of a nose forgotten. This is one point at which Freudian ego defence mechanism may enter to block or distort the interpretation and memory of threatening events. Interpretation of the event may also be affected by the need for attitude consistency, causing changes not only in the value put on objects, but also in the relations perceived to hold among them (e g. Abelson & Rosenberg 1958).

3. Language appears to be a crucial factor in memory It has been shown that memory, even of 'purely visual' stimuli, is largely linguistic (see, e.g , Brown & Lenneberg 1954; Lantz & Stefflre 1964. Recently, however, some of the research on colour recall has been re-examined by Heider (1972). Her findings bring into question Brown and Lenneberg's ideas about the relation of codability and memory of colours, but Lantz and Stefflre's theory about the relation of communicability and memory seems to remain intact, and Heider herself asserts that linguistic codes clearly influence memory)

4. D'Andrade's research (1974) suggests that what a person remembers of an event is systematically different from his on-the-spot coding of the event, and that the distortion is such as to bring his memory of the event into accord with his semantic structures.

5. Miller (1956) shows that a numerical array can be coded into 'chunks' to facilitate memory. Decoding will reproduce the original array. Ordinary language seems to be used in a similar way to code everyday experience. In the process of chunking, however, much of the original event is lost, abstracted out. It would seem, therefore, that the code that is used is likely to affect what is finally remembered—only those features which are coded can be decoded.

C. *Understanding and answering questions*

1. Given that the informant has certain memories of the event, he must place some construction on the interviewer's question in order to know what kind of an

answer would be relevant. Even if the question is exhaustive—'Tell me everything that happened'—the informant will use his understanding of the total context of the question to ascribe intentions and make and act on judgements of relevance.

2. Having interpreted the question, the informant's answer will be influenced by problems of verbalisation, by what he is capable of saying and what he thinks the interviewer is capable of understanding. His assumptions about how the interviewer can be communicated with, about what must be made explicit and how, will affect his presentation.

D. *Situated motives*

1. The respondent may want to please the interviewer or to give the 'right' answer. The 'demand' characteristics of an interview, which can be extremely subtle and pervasive are used to achieve this end (see Rosenthal 1966). The classic example of interviewer effects is the case of Clever Hans, the horse who read non-verbal demand cues so well that, until his 'technique' was discovered, he was thought to be capable of amazing feats of intelligence, including a comprehensive knowledge of written German (Pfungst 1911).

2 The informant is likely to want to say normatively appropriate things in normatively appropriate ways, and to present a certain image of himself. In fact, looked at from a certain perspective, the function of many social norms may be to keep people from telling each other the truth.

3. The informant may say a good deal less than he knows, or thinks he knows, or he may deliberately lie, simply because he is not motivated to give a full description He may be bored with the interview or anticipate that a certain answer to a question will provoke a barrage of further questions.

E. *Understanding the answer*

1. The informant's answer is filtered by the interviewer's own selective perception, interpretive procedures, and memory or recording system. (Garfinkel (1967) makes the point that one can 'see the system' in the data only by assuming the position of a competent member to the arrangements that one seeks to give an account of.)

If the researcher wants to understand the pertinence of the account to the event being talked about, he has to pass backward through all these 'filters'. And if the account is based on hearsay rather than on actual observation, the interpretation becomes even more problematic.

III

Other kinds of information

In the preceding discussion, I have been considering the problems in the obtaining of information about an actual event, external to both the researcher and the informant There are, however, other kinds of information relating to other, more elusive, 'realities'. One such type of information has to do with what we might call 'intersubjective reality'. Suppose that a group of persons get together and invent a new game, which they agree to play on the following day. After the meeting (but before the game is played), I ask several of them whether, according

to the rules, a goal is to be counted as one or two points. Some say one, some two. Assuming that there is no person or document with final authority to decide the matter, can a rule be said to exist which specifies the point value of a goal? Can it be said that a rule exists simply because everyone at the meeting seemed to agree to it? People can, after all, change their minds or claim to have been misunderstood. Must everyone agree, or only some specific member? If, when the game is played, a goal is counted as two points, can we say that that was the rule all along? These seem to be questions of definition. How am I and how are my respondents defining the word 'rule'. The problem of definition notwithstanding, I can still say something about my informants' answers if I assume that they all understood my question in approximately the same way (even if not in quite the way that I meant it), and that the 'value' of a goal is not taken by them to be a purely personal matter. The fact that they have given me contradictory answers indicates that some of the answers are either incorrect or incomplete (e.g., the first goal may be worth one point and the second two). Of, if they define a rule as consisting in the general agreement of all concerned parties, then, in so far as that agreement does not yet exist, the only accurate answer would be, 'I don't know', or 'We don't have a rule for that yet'. Anyone who has heard children (or adults) argue about 'what the rules *are*' for a game which they have just invented is in a position to realise that laymen, as well as sociologists, have a tendency to reify rules, treating them as things, separate from the persons who make them and abide by them.

Equally problematic is information about 'intrapersonal reality'. To begin with, we have no direct access to the subjective states of other persons But there is the further problem that the person himself may not be an ideal observer of his own emotions. (By 'emotion' I mean any motivating subjective state) Experimental evidence on this point has been accumulating since Schachter and Singer (1962) published their now classic study. Zimbardo *et al.* (in press) have raised serious questions about the validity of the Schachter and Singer experiment, and so I will, for purposes of illustration, describe a less ambitious but neater experiment, reported by Valins (1966) Male college students were shown slides of female nudes. The students were given the impression that their heart rates were being monitored, and they were allowed to 'overhear' their heart rates as they looked at the slides When certain slides were shown, the (fake) heartbeats that the subjects heard were changed so as to make it appear that the heart rate had speeded up or slowed down These slides were generally rated more attractive by the subjects than those on which the heart rate had remained steady. This implies that the subjects used the heart rates as clues, from which they made inferences about their own feelings. It seems, then, that our access to our own subjective states is not as direct as we sometimes think. Occasionally, we may 'decide' our emotion rather than feel it. (Having made the decision, though, we may come to feel the emotion.)

Clearly, experiments of this sort raise some very complex issues. In order to have an emotion, do you have to know that you are having it? Can you be wrong and think that you have a certain feeling when actually you have a different one? Or, when you think you have an emotion, does that fact alone make it so? In the Valins' experiment, the preferences that the students thought they had were 'real', at least for practical purposes, since, when the students were given a chance to choose slides to take home with them, they tended to choose the ones that were

accompanied by a change in heart rate. If one of these students were our informant, he could perhaps tell us accurately how he felt about the various slides, but he could not tell us how he would have felt had the heart rate clues not been present. This, it would seem, is not an important loss, since we are asking for an accurate description of 'reality' and not of some hypothetical situation.

Unfortunately, the problem goes deeper than that. Bem (1972: 2) argues that:

> *Individuals come to 'know' their own attitudes, emotions, and other internal states partially by inferring them from observations of their own overt behavior and/or the circumstances in which this behavior occurs. Thus, to the extent that internal cues are weak, ambiguous, or uninterpretable, the individual is functionally in the same position as an outside observer, an observer who must necessarily rely upon those same external cues to infer the individual's inner states*

Bem uses this hypothesis to reinterpret the well-known experiment by Festinger and Carlsmith (1959). Students were asked to do very dull tasks, and then were paid by the experimenters to tell other students that the tasks were enjoyable and interesting Some of the students were paid $1, others $20 Later the subjects indicated how much they had enjoyed the tasks. The subjects who were paid less for telling others that the tasks were enjoyable subsequently seemed to have a more favourable attitude towards those tasks than did those who were paid more. According to Bem's 'self-perception' theory, the subjects consider their own behaviour in telling others that the tasks were fun. The subjects who were paid $20 have an adequate explanation for their behaviour in the financial reward they received for it. The ones who were paid only $1, on the other hand, must look elsewhere for an explanation of their own behaviour, and they tend to conclude that their attitude towards the tasks was somewhat consistent with their behaviour in telling others that the tasks were enjoyable.

It is most important to notice here the implication that the $1 subjects (or the $20 subjects, or both) were wrong about their original attitudes, that is, about how much they had actually enjoyed the tasks. They did the tasks under the same conditions, without knowing that they would be 'hired' as 'assistants' afterwards, and they were selected for high or low payment randomly, so there is no reason why one group should have enjoyed the tasks more than the other. If Bem's theory is correct (and it must be said that the evidence is not yet conclusive), then persons, even persons who are trying to tell the truth, are not reliable as reporters of their own past emotions. (The word 'past' is superfluous, since all reports must refer to that which is in the past) Either we can have emotions without being aware of them, or we are at times only fleetingly aware of them, and so cannot remember or report them. But if the person himself is not an adequate observer of his own emotions, and if no one else can have direct experience of that person's emotions, are we justified in positing any place for his emotions in the realm of reality? Do his emotions exist? Emotions (or, if you prefer, values, or attitudes) may be asserted to exist on at least two grounds First, they seem to be there on the evidence of intuition. Second, they are a residual and seemingly necessary category for explaining why people do things. Broadly conceived, they are, by definition, the reason why people do things.

Since there seems to be (at least, as far as we know at present) no ideal observer or infallible procedure for observing emotion, we are left without a precedent reality. (It is possible, though, that techniques can be worked out for the reliable

observation of our own emotions, or the indirect observation of others'. Of course, we already have certain techniques of this sort, but they are not completely reliable or specific even in principle.) We simply assume that emotions exist, and that specific emotions have specific consequences, and this assumption is both plausible and useful, as proved by the fact that people seem to make such an assumption and act on it universally. Moreover, if shorn of the possibility of a precedent reality, and forced to choose between the congruence and correspondence views of reality, I would tentatively opt for the latter. In other words, I think that emotions (and other subjective states) have an objective existence, that a person's emotion may be real, in the sense that it influences his behaviour, even though he is not aware of it. For example, when I say a sentence I do not think that I am ordinarily aware of the grammatical structure of the sentence, and yet, in some sense, I must have the structure 'in' me, else how could I produce grammatical sentences? In the same way I may experience emotions without being aware of them, without their 'appearing to me', and they may affect my behaviour. I am simply making the choice of calling that which explains the otherwise unexplainable, and is not incongruent with that which I already know, 'real'. (Perhaps the difference from a congruence view is mainly one of vocabulary. If the congruence theorist chooses to say that what I have called 'real' is merely one kind of reality—say 'scientific reality'—this is acceptable to me. I would merely add that I am using 'scientific reality' as a standard with which to judge the truth or falsity of assertions Instead of 'false', read 'false with reference to scientific reality'.)

Another kind of 'information' is 'paradoxical information'. Consider the sentence, 'This statement is false'. It seems to me that this is not a meaningless sentence. We can understand that the sentence asserts: 1) 'Statement X is false', and 2) 'Statement X is identical with 1) ' Although it is not meaningless, the sentence cannot be said to be true or false given any knowledge whatsoever. There is no imaginable reality to which it can refer, it evades reality entirely. Bateson and his associates (1956) have found a certain kind of paradoxical communication, which they call (part of) a 'double bind', to be critical in the etiology of schizophrenia. One of the characteristics of a double bind is the presence of a message 'which is so structured that (a) it asserts something, (b) it asserts something about its own assertion and (c) these two assertions are mutually exclusive' (Watzlawick *et al.* 1967. 212) From a common-sense point of view, it is not at all surprising that such messages should play a part in bringing on schizophrenia. Schizophrenics are commonly characterised as persons who have lost touch with reality. Such a condition might be seen as a logical consequence of constantly receiving messages which do not refer to any imaginable reality. (Although the messages may be injunctions, they can be translated into statements such as 'I want you to. ...') A consideration of such paradoxes, where a sentence makes a statement about itself, leads Tarski (1956: 165) to the conclusion that '*the very possibility of a consistent use of the expression "true sentence" which is in harmony with the laws of logic and the spirit of everyday language seems to be very questionable, and consequently the same doubt attaches to the possibility of constructing a correct definition of this expression*'. My intuitive solution has been to insist that the sentence be related to some 'reality' external to itself.

I suspect that misinformation is present in verbal accounts to a much greater degree than we ordinarily think. It is true that much of the evidence which I have

cited in this article was produced in laboratory experiments using ambiguous stimuli. People do not ordinarily have to deal with tachistoscopes and autokinetic effects in their daily lives. More generally, while I have tried to show that the *possibilities* for distortion of 'reality' are diverse, I have not directly demonstrated that they are important enough to produce a significant degree of inaccuracy in everyday communication. For evidence on this point, in a circumscribed but 'natural' setting, the reader is referred to Bilmes (in press), to studies of validity in sociological surveys (e.g., Parry & Crossley 1950), and to discussions of fieldwork problems scattered throughout the anthropological literature (e.g., Chagnon 1968: chapter 1). Finally, I have not dealt here with the question of how researchers and natives can and do handle the problems presented by the fact that verbal accounts are frequently inaccurate. This subject must be postponed for a future article.

NOTE

I would like to thank Michelle Rosaldo and Charles Frake for their comments on earlier versions of this article.

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THE SACRED MUSHROOM IN SCANDINAVIA

REID W. KAPLAN

All known examples of a remarkable Bronze Age Scandinavian religious motif are brought together for direct comparison, including those which have a dubious claim to membership in the set These latter are dismissed on various grounds, leaving those which have a probability approaching certainty of being related. Attention is drawn to two recently discovered Swedish petroglyphs These both link the motif, heretofore found exclusively on bronze razors, to the main body of the religious symbols of the period and provide additional evidence of the unsatisfactory nature of prior identifications Occam's razor is utilised to propose that the motif is what it appears to be—a mushroom An hypothesis, based on known activities of neighbouring cultures and recent ethnobotanical findings, is advanced to explain the appearance of a mushroom in a ritual context A Swedish folk practice which may be an attenuated form of such a mushroom ritual is described

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I

In Sweden, Norway, and Denmark there are rock carvings that are the product of a rich religious tradition They are found largely in agricultural areas and usually close to the sea. These petroglyphs are the product of the Bronze Age culture of the region although they have Neolithic antecedents (Stenberger 1962: 99). Contemporaneous with the formation of these engravings, many of the same motifs appear on bronze objects often found as grave goods, particularly flat, wide bladed razors. Analysis of the motifs suggests strongly that they were symbols of a religion which had as its main theme worship of the sun and that they were engraved as part of the ritual of this worship (Gelling & Davidson: 1969).

The motifs range from abstract symbols to representational figures, the latter somewhat schematised in rock but often highly embellished in the more plastic bronze medium. Amongst these motifs the ship-figure is particularly noteworthy. It seems to occupy a central position in the ritual as the vehicle by which the sun, after being drawn westward by horses across the sky by day, returns to the east. The ship often serves as a setting for other symbols, indicating the religious nature of the subject in much the same way that a cartouche indicates the royal nature of the Egyptian hieroglyphs within it. Thus we find snakes, fish, men brandishing axes, horses, birds, wheel crosses, and many other figures within the ship setting. Each of these has been placed more or less securely in the mythic framework that has survived the intervening millennia.

One of the least securely placed is a curious motif occurring on seven bronze razors found in Denmark (fig. 1), on two rock carvings in Åby, Bohuslän, Sweden (fig. 3) and on a hanging vessel found in Öland, Sweden, now in the Kalmar Museum (fig. 4) The razors have been dated to Periods IV and V of the Bronze Age (Althin 1945: 221; Sprockhoff 1954: 87), somewhere between 1100 B.C. and 700 B.C. The rock carvings (Fredsjö 1967) are in the 'rich' style of Marstrander (Marstrander 1963: 457) and are, therefore, probably contemporaneous with the razors.

The rock carvings at Åby are situated near one another on a sloping rock face.

Man (N.S.) 10 72-9.

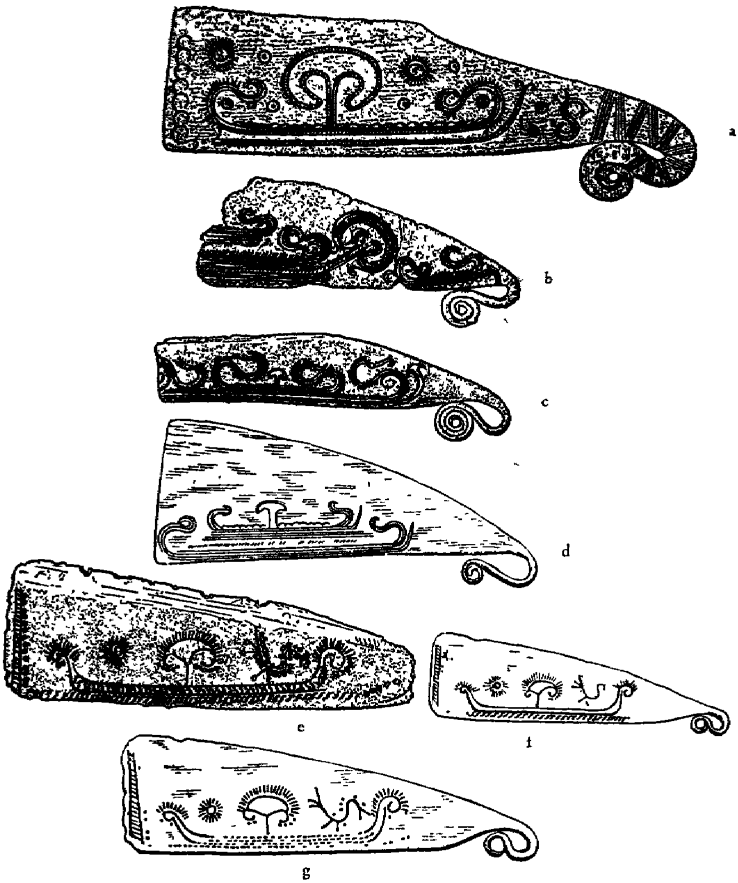


FIGURE 1. All known examples of the mushroom motif on bronze razors: *a*, Honum, Skanderborg; *b*, provenance unknown, *c*, Abkaer Mark, Hadersleben, *d*, provenance unknown, *e*, Vandling, Hadersleben, *f*, Hadersleben, *g*, Nustrupfeld, Nordschleswig.

They are accompanied by other ship figures, a wheel cross, a complex spiral, and several male figures. Both of the ships contain, in addition to 'crew strokes', a principal human shape with arms upraised in a worshipful manner, two objects on poles, and the object discussed in this article. The objects on poles are being held by members of the crew and have been identified as spears (Gelling & Davidson 1969: 135). It is to be noted, however, that the shafts continue through the top of the putative spear point, as well as appearing through its body, that there are two distinct shapes only one of which is pointed, and that both carvings show a size difference between the examples. They really appear like loops, perhaps of rope, suspended from the tops of shafts. The shape with which we are primarily concerned is being held in one case by a well delineated human figure. In the other,

it may be held by a crew stroke or may be free standing—it is difficult to tell which.

These carvings allow us to do two things. We can now firmly place this motif in the main body of cult images used by the Scandinavian rock carvers. Heretofore we were dependent solely on the rather dubious example of the 'standing double spiral' (fig. 2*d*) to forge the link to the image found on bronze razors. More importantly perhaps, we may use this additional evidence to aid in the identification of the object.

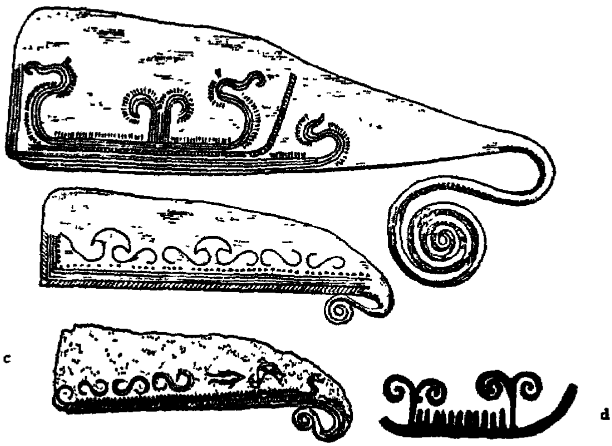


FIGURE 2 Similar Motifs· *a*, Denmark, *b*, Grostorp, Skåne, *c*, Skåne, *d*, Bohuslan.

Part of the problem of identification is that the sample is small and there has been a tendency to include within the class anything that appears remotely similar in shape. Theories must then be stretched to include erroneous evidence. As new evidence accumulates, we begin to enjoy the luxury of discarding data that do not fit or, at least, placing them in another category. The first to be excluded is the razor from Gröstorp, Skåne (fig. 2*b*), originally included by Althin but ignored by Sprockhoff and others. First, it does not appear within a ship setting unless the ship is schematised beyond recognition. Second, there are repetitions of the figure. In all other cases, only one shape is shown. Third, none of the motifs in fig. 1 has complex curves; all are supported on distinctly straight stems. Fourth, not all the s-shapes are joined; the two on the right are isolated. Lastly, it is similar to the twin horse motif (Sprockhoff 1954: 77 sqq.) shown in fig 2*c* with the addition of an arc connecting the two s-shapes. Only a religious and artistic pun, conjoining multiple symbols, could be advanced as reason for its inclusion.

Excluded also must be the form which Sprockhoff (1954: 87) calls a 'standing double spiral.' This appears on the rock carving first collected by Baltzer (1881) (fig. 2*d*) as well as a bronze razor first published by Madsen (1872), and clearly lacks the concave-downward, surmounting arc of all the others. Obviously it would have been simple to add to the easily worked bronze, had it been thought

necessary. The new rock-carvings (fig. 3) show, even in the difficult medium of rock, that it was necessary. Whether this other symbol depicts a 'twin-tree' or a schematic 'twin-horse' motif (Sprockhoff 1954: 84; Gelling & Davidson 1969: 135) is moot. It clearly does not belong to the class under consideration.

a

b

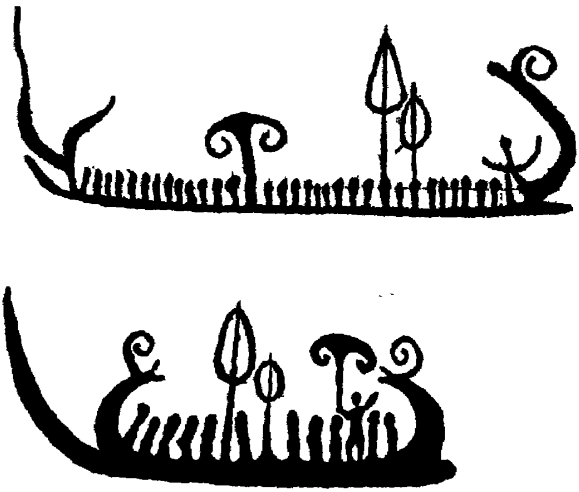


FIGURE 3. The mushroom motif in rock-carvings: *a*, *b*, Åby, Bohuslan.

Having made this exclusion, it still remains to discuss the other identifications of this shape, none of which has been convincingly made.

It was first described as a sail, or rather a bush used as a sail (Schuchhardt 1918), although there is no evidence that the Bronze Age Scandinavian sea-farers used anything more elaborate than paddles to propel their boats. Recently a boat has been built of the design appearing in the petroglyphs and found to be relatively seaworthy (Johnstone 1972). Part of the experiment showed that a bush *could* function as a sail, although, obviously, that is no proof that it, in fact, did. The Åby carving, showing a man holding the object (fig. 3*b*), removes this explanation for good and all from contention. As Althin notes (1945: 217), this interpretation never gained much acceptance. No sail could possibly function without proper support and there is no way to explain a reverential attitude towards a sail. In general, those attempts to understand motifs such as this solely from a utilitarian view miss the essential point of the obvious symbolic character of the depictions.

Many have thought the motif to be symbolic of a tree, perhaps the world-tree of northern myth which held up the heavens (Hoernes 1917: 35; Wilke 1919: 158–162; Almgren 1934: 12; Gelling & Davidson 1969: 132). This was rejected by Althin (1945: 218) on the grounds that trees were portrayed in an entirely different manner. The new carvings make clear that we are not dealing with extreme stylisations of boughs, which interpretation was possible as long as the 'standing

double spiral' was included in our evidence. Also, it is hard to understand why the world-tree, with all its associations of stability and massiveness, would be held in the air in one hand, even in effigy.

The blade of a cult-hatchet has been offered as an identification of the shape (Schwantes 1939: 20). One can see that the blade would have an unusual orientation, totally unexpected from what is known of the rock carvings. Where a known axe is depicted, it is always shown with a handle. Never is a blade shown by itself, let alone pointing skywards In addition, the new carvings show the blade-like area to be much too thin, even for a non-useable cult object.

II

If it is none of these things and if we have excluded examples which point towards a stylised horse, then what is it? On the simple principle that something is what it looks like, it must be identified as the effigy of a mushroom. Both Sprockhoff and Gelling have called it 'mushroom-shaped,' but have sought elsewhere for an explanation (Sprockhoff 1954: 83, 86; Gelling & Davidson 1969. 135). This situation brings to mind the mushroom stones of Mesoamerica. Carl Sapper was the first, in 1898, to call them 'mushroom-shaped' figurines. From his day, as the Wassons told the story in 1957, 'all archaeologists knew these artifacts as "mushroom stones," but not one has ever come to grips with the possibility that they represent what they look like. Indeed it is customary to set off the designation in inverted commas and the learned writers often add a safety clause expressly saying that of course the designation is only one of convenience' (Wasson & Wasson 1957). This may be because, until recently, little notice has been taken in the archaeological literature of the role played by hallucinogenic mushrooms in the religion of cultures the world over Many Indian tribes of Mexico (Wasson 1961; Schultes 1969) and various groups of central and northern Eurasia (Wasson 1971; Eliade 1972. 220 sqq) are shown to have used hallucinogenic mushrooms in religious ritual. In addition, the same mushroom used by the Paleosiberians has been, convincingly for many, identified as the Soma of the Ṛg-Veda (Wasson 1971). Since there are these precedents for the important ritual position of mushrooms, may we not postulate a use of the divine mushroom in Scandinavia as well?

In the case of two of the mushrooms (figs. 1*a* and 1*b*) engraved on razors and the examples on the hanging vessel (Fig. 4), there are details which indicate structures on the surface of the cap. They are qualitatively different from those rays emanating from the mushrooms of figs 1*e*, *f*, and *g*, which seem to indicate brightness. In all cases they are placed on the cap and appear to depict cells or spots. They are not general ornamentation—they stop at the top of the stalk or stipe of fig. 1*a* and are only on the top surface of fig. 1*b* and fig. 4. Could they represent the white patches that appear on the brilliant red surface of the cap of *Amanita muscaria*? All the examples suggest a rounded, almost spherical, cross-section with an incurved pileus. One of the rock carvings suggests a prominent umbo. These characteristics are consistent with *A muscaria* in all but its latest mature stage. Three of the depictions (figs. 1*a*, 1*b* and 4) clearly show a hollow stem. Two of these (figs 1*b* and 4) have markings which suggest that the hollows contain something. It is the case that the stipes of *A. muscaria* are indeed hollow and

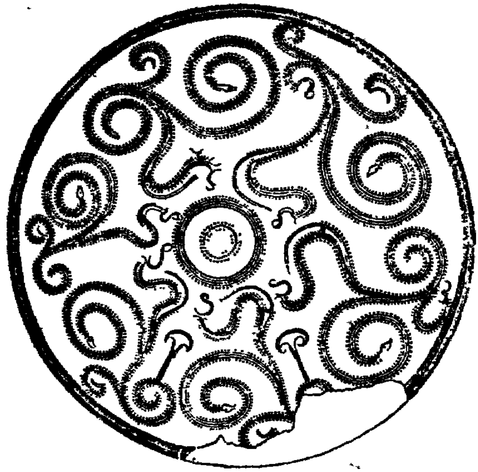


FIGURE 4 The mushroom motif with zoomorphs. Bronze hanging vessel, Öland, Sweden.

that they are often, especially towards the base, loosely packed with fibres. One representation (fig 1*b*) shows curving, dependent structures coming off the pileus at its lower edges This is precisely where the remnants of the universal veil of *A. muscaria* are often seen. *A. muscaria* abound in Scandinavia and we possess reliable testimony that the Inari reindeer Lapps once made use of these hallucinogens in their shamanic performances. It is certain that *A. muscaria* was at least as common in Stone and Bronze Age Scandinavia as it is today. It grows in mycorrhizal relationship with *all* species of the genus *Betula* and most species of *Pinus*. It has been found in association with *Abies*, *Larix* and *Picea* (Ramsbottom 1953: 204). Exceptionally, it has been found with oak and chestnut. The two primary hosts, *Betula* and *Pinus* have wide distribution in Europe and Asia (Wasson 1971: 154 sqq.) and *A muscaria* has been found everywhere they grow. It is, therefore, axiomatic that a favourable habitat for the hosts is favourable for the mushroom as well. Pollen analyses in northwestern Europe, and particularly Sweden, show that from the end of the last glaciation *Betula* and *Pinus* have been remarkably abundant. From the time the ice sheet retreated until Periods IV and V of the Bronze Age, *Betula* constituted between 90 per cent. and 13 per cent. of the forest population. *Pinus* shows the same range. About 1000 B.C. *Betula* averages 33 per cent. over all of Sweden and *Pinus* 41 per cent. Furthermore, *Betula* and *Pinus* populations have a fascinating inverse relationship. When climatic conditions are such that birches decrease in number, pines increase and vice-versa. This means that the total of the two, in any one region of Sweden, has remained at a constant and high level through the entire postglacial period, regardless of climate. In most areas they have amounted to 70–90 per cent. of all trees in the forest for nine millennia (Von Post 1946: 196, 197, 200, 201, 211). Neither the world wide thermal maximum which culminated *c.* 3000 B.C., nor the climatic 'deterioration' which followed, had any real effect on the combined numbers of birch and pine. *A.*

muscaria has thus been assured an amazingly constant and plentiful supply of hosts of which it inevitably would have taken advantage.

Finally, one must not be put off by the relative size of man and mushroom in fig. 3*b*. There are numerous examples of petroglyphs of easily recognised objects which have been exaggerated for emphasis or have been portrayed much larger than life size.

Although it is difficult to trace many of the motifs down to historic times, there is one intriguing, if oblique, reference in northern folk-custom to mushrooms. It is a Swedish custom, practised until fairly recently, which is part of the festival of the Midsummer Fires On St John's Eve, after the kindling of what was known as Baldr's Balefire, the 'spectators cast into the flames a kind of toad-stool (Bäran)[1] in order to counteract the powers of the trolls and other evil spirits' (Frazer 1966: 172)

Thus, the evidence in bronze and rock suggests that a component of the sun-centred religion of the Bronze Age north was a mushroom cult, utilising the psychotropic properties of the fungus for ritual purposes These practices, still maintained by other northern groups into this century, died out, as did so much in Scandinavia, by the early Iron Age.

NOTES

The author is indebted to Mrs Gillian Bottomley for graciously allowing him to present her drawings of the Åby carvings, to Mr R. Gordon Wasson and Professor Michael Coe for advice and encouragement, and to Mr Peter Gelling for appreciated help

[1] Frazer was accurate but not complete in his transcription of this account He neglected to include a subordinate clause after the word *Baran* 'so named after certain emissaries of Satan' (Lloyd 1870 265) Without this clause, Frazer's entry makes it appear as if *Baran* might be the specific, if local, name of the mushroom used Obviously the word refers to the mushroom as the representation of a spirit, for which it is a surrogate, stand-in or embodiment

There are two words related to the verb *bara* (English *bear*, O.N *baera*, O.S , O G and O.E. *beran*, Dutch *baren*) which are used in this sense. *Bararen* (the bearer) was a ball of yarn into which a man would drop some blood from a finger on which he had made a slight cut This signified that he had given his soul to the devil, in return for which, the 'bearer' would bring prosperity to his house Another related word, *bjara*, is the term for a kind of troll, formed of various material and brought to life through certain manipulations by its owner The troll worked for its owner by bringing to him milk and butter from other people's cows (Wassén 1974)

It seems that we are dealing with a related usage in this instance, destroying the evil spirit by destroying the mushroom in the flames It is still interesting to contemplate how a mushroom could become in the peasant mind a personification of an evil spirit

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BYANSI KINSHIP TERMINOLOGY: A STUDY IN SYMMETRY

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The non-prescriptive kinship terminologies typical of the Indo-European languages of north India are often contrasted with the symmetrical prescriptive ones of the Dravidian-speaking south, but terminologies of the latter type also occur in the Tibeto-Burman languages of the northern (and eastern) fringes of the subcontinent One such terminology was reported in 1911 from the Himalayan valley of Byans. The present analysis examines how Byansi departs from the pattern of an ideally logical and simple symmetrical prescriptive terminology Where possible, the departures are viewed diachronically, in the light both of the growing influence of the Indo-European regional language and of comparisons with genetically related languages. The complexities in ego's level of the terminology are interpreted as corresponding to the point reached in a process whereby the criterion of relative sex has been losing ground to the less symmetrical criterion of absolute sex.

In north India marriage with any category of relatives is in general discouraged and the kinship terminologies of the Indic (i e. Indo-European) languages spoken there show none of the features that usually accompany cross-cousin marriage. The contrast with south India and with the hill regions east of Bangla Desh is well known. In both these non-Indic-speaking areas marriage with cross-cousins is widespread and the fact is reflected in the 'prescriptive' terminologies, whether these are straightforwardly symmetrical as in the Dravidian languages, or whether they include asymmetrical features as often in the Burmic ones One aim of this article is to draw attention to the fact that terminologies similar to those of south India occur also in the northern fringes of the Indian world, the other is to present a detailed analysis of one instance

I

Byans is a Himalayan valley lying in Almora (U.P.), in the corner where the western border of Nepal meets Tibet At the time of the Linguistic Survey of India its seven villages contained some fifteen hundred inhabitants (Konow 1909: 518; Pant 1935: 242). The local language, Byansi or Byangsi, was classified as one of the Western Subgroup of the Complex Pronominalised Himalayan languages, or according to a more recent taxonomy (Shafer 1955) as a member of the Almora branch of West Himalayish. It is closely related to Chaudansi (or Tsaudangsi), the language of the next valley to the south, so closely indeed that the two are 'almost one and the same dialect' (Konow 1909: 428). The remaining languages in Konow's subgroup are spoken by other mountain communities living to the north-west of Byans on and about what he calls the 'ethnographic watershed between Tibetan and Aryan'. The group as a whole is classified as Tibeto-Burman,

Man (N.S) 10, 80–94.

or more precisely (Shafer), as one section of the Bodic division of the Sino-Tibetan family. Both Byansi and Chaudansi have been much influenced by Pahari, the Indic language spoken over large tracts of the hills immediately to the south, and it is likely that they will eventually be replaced by it. They were still spoken in 1952–3, but since then there have been great changes, including much emigration (Srivastava 1966: 178, 211). At the same time there were some two thousand speakers of the language reported to be resident in Nepal (Nepal National Census, quoted by Clark 1969: 250), and it is possible that further information relevant to our analysis could more readily be obtained in the extreme north-west of that country than in Almora.

II

The kinship terminologies of the two valleys, together with two others from the same state, were recorded by Lall (1911) in a paper that has seldom been mentioned in the subsequent literature. I shall concentrate on Byãnsi as it is given in greater detail and is the more interesting, but refer occasionally to Chaudansi for comparison. Lall, who was a lawyer and member of the Indian Civil Service, seems to have been a reasonably careful ethnographer However, he is sometimes inconsistent in the spelling of native terms and Byansi *pooni* and *puni* can hardly represent different words, any more than Chaudansi *bhooli*=yZ[1] and *bhuli*=WyBW (cf. Pahari *bhuli*=yZ, PGDy, WyBW, given in Berreman (1963: 412)). His use of diacritic signs over vowels is also erratic as well as being unexplained, and they will be omitted here After a discussion he summarises the terminologies in a series of columns with English equivalents on the left, but the data have been rearranged in table 1 in a form more convenient for analysis. The specification MBW, omitted from his table, has been filled in from the text (p. 193) Opposite a few specifications he leaves a row of dots, implying either that his data were unsatisfactory or that no kinship term was used in address (on which his attention was apparently concentrated). To supplement Lall we have only a few individual words mentioned in passing by other writers (see note 4 below), and the following list of one-word equivalents from Konow (1909: 539 sqq): *ba* and *na* as in Lall, *pi*='brother', *rangsya*='sister', *minang siri*='woman', *siri*='son', *chame*='daughter'.

The Byansi terminology is in fact a clear example of the type known (among other labels) as symmetrical prescriptive; as such, its structure can be conveniently displayed with the aid of a matrix diagram (fig 1). No term equates relatives of different generations so each level may be considered separately. The principles of classification involved are particularly clearly seen in the parents' level, especially in the section containing the parents' opposite-sex siblings. The terminology is prescriptive in that affines and cognates are not distinguished, and it is symmetrical in that there is no distinction between patrilateral and matrilateral relatives, or between wife's parents and husband's. The only irregularity here is that *pooni* duplicates the EM specifications of *chini*. Various explanations might be suggested for the overlap, but in the absence of more precise data there is little to be gained by speculation. Both words contain the element *ni*, whose etymological relatives are found very widely in Tibeto-Burman terms for FZ (cf. Benedict 1941: 317).

TABLE 1. Byansi kinship terms, summarized from Lall (1911).

titi	FF, MF
lala	FM, MM
ba	F
na	M
babu	FeB
kaku	FyB, MZH
puna	FeBW
chichi	FyBW
pochi	MeZ
shenchi	MyZ
chini	FZ, MBW, WM, HM
pooni	WM, *puni* HM
thangmi	MB, FZH, WF, HF
yha	eB, FBSe, MZSe
nunu	yB, FBSy, MZSy
pota	eZ ms, FBDe ms, MZDe ms, WeBW
tata	eZ ws, FBDe ws, MZDe ws, WeBW, HeBW, SWM
ringssha	yZ, FBDy, MZDy, WyBW, HyBW
poa	FZS ws, MBS ws, HB, HMSe, eZH ms, yZH ws
chhemain	FZD ms, MBD ms, WZ, BWe ms
tete	FZS ms, FZD ws, MBS ms, MBD ws, WB, HZ, HMDe, yZH ms, BW ws, SWF
chharpeo	WZH
mase	HMSy
rithi, yorashri	H
rithi shia, minangshri	W
sri	S, BS ms, ZS ws, WZS
chamain	D, ZD ws, WZD
bhanj	ZS ms, ZD ms, WBS, WBD
pij	BS ws, HZS
pima	HZD
namsia	SW ms and ws
mayeh	DH ms and ws
khwe	SS, DS

The other section of the parental level appears more complex, and certainly contains more terms

ba	F	*na*	M		
babu	FeB	*puna*	FeBW	*pochi*	MeZ
kaku	FyB, MZH	*chichi*	FyBW	*shenchi*	MyZ

However in Chaudansi Lall gives as variants of *pochi* and *shenchi*, but with the same specifications, *poo-chichi* and *shen-chichi* respectively. This suggests that in both valleys the unmarked term *chichi* formerly covered MZ as well as FyBW; in other words that there used to be only six words in the group and *chichi* simply meant 'wife of *kaku*'. Furthermore it is typical of Tibeto-Burman that parent's same-sex sibling terms should be compounded of the word for the parent plus an element meaning larger/elder or smaller/younger (*ibid.*: 316). Thus the term for *babu*'s wife, *puna*, no doubt derives from *na*=M and the same prefix as in *poo-chichi*[2]. *Babu* is not a compound but an honorific borrowed from Pahari, cf. Nepali *bābu* 'father, term of respect for older men' (Turner 1931; for details of its usage in north-east U.P. see Dumont 1962). Thus on lexical grounds *babu* and *puna* could well be innovations, and it is reasonable to suppose that before their introduction the categories on the father's side were not split by relative age, any more than those

		male	female	female	male
first ascending level		*ba* *babu* *kaku*	*na, pochi* *puna, shenchi* *chuchi*	*chim* *pooni*	*thangmi*
ego's level	ms	*chharpeo*	*chhemain* *rithishia*	*pota*	*tete*
	ms or ws	*yha* *munu*		*ringsha*	
	ws		*tete*	*tata*	*poa, mase* *rithi*
first descending level		*sri*	*pima* *bhanj* *namsia*	*chamain*	*pij* *bhanj* *mayeh*

FIGURE 1, to show the structure of the Byansi terminology, all Lall's terms from the middle three levels are included. The double vertical line separates two sections consisting of the terms for categories of relatives who may not intermarry in a symmetrical prescriptive alliance system, while the single vertical lines separate boxes containing terms for those who have, could have, will, or may do so. In this paper the word 'section' refers to this division of terms, *not* to any division of society which may in some way correspond to it

on the mother's side would have been with the hypothetical six terms In other words there probably used to be four terms in which the only elaboration of the theoretically minimal symmetrical prescriptive terminology was the presence of individualising terms for parents. Benedict's list of basic Tibetan terms contains just such four terms.[3]

Ideally, ignoring specifications resulting from marriages within the generation, the first descending level of a symmetrical prescriptive terminology is made up of terms for (i) ego's and ego's same-sex siblings' children, together with spouse's same-sex siblings' children, and (ii) opposite-sex siblings' children and spouse's opposite-sex siblings' children, each set being subdivided according to the children's sex. (These 'sets' do not of course coincide with the 'sections' of fig. 1, where brothers and sisters, who may not intermarry, necessarily belong to opposite sections.) In Byansi, *sri* and *chamain* cover the first set, except insofar as Lall omits BD ms (no doubt *chamain*), and while giving WZC, ignores HBC, the female ego's equivalent. As it stands, the other set, with its three terms, is less straightforward:

bhanj	ZS ms, ZD ms,	WBS, WBD
pij	BS ws	HZS
pima		HZD

The interloper is *bhanj*, which comes from Pahari *bhanju* (or *bhanja*) = ZS, *bhanji* = ZD (Berreman 1963). In the process of borrowing it has lost its gender distinction, for no obvious structural reason. Before its introduction it is reasonable to suppose that the whole field was covered by the non-Indic *pij* and *pima* (whose relationship

to Konow's *pi*= B raises questions which we shall pass over). Similar classificatory structures are easily found among Bodic-speaking peoples in the Himalayas, e.g. among the Tamang of east Nepal (Fürer-Haimendorf 1955–6: 170), where *kon*= BS ws, ZS ms, and *konme*= BD ws, ZD ms, i.e. respectively opposite-sex sibling's son and daughter It may be noted that in Byansi it is only a male ego who uses the imported term consistently. As for a female ego, nothing is said about BD ws, but BS ws is given elsewhere (Lall's text p 194) as *bhanj*. To an Indic-speaker this might seem inappropriate because of the apparently transparent etymological relationship of *bhanj* to the family of cognates which includes Hindi *bahin*= Z. But to someone thinking in two-section terms, if a man's opposite-sex sibling's child is a *bhanj*, a woman's should be the same; so probably the overlap of *bhanj* and *pij* was a phase in the expansion of the former at the expense of the latter. In Newari (spoken around Kathmandu), another Tibeto-Burman language much influenced by Indic, the process seems to have reached its logical conclusion in the related term *bhincha*, which means opposite-sex sibling's child for an ego of either sex (Nepali 1965: 264–5).

The only remaining terms in this level are the children's spouses. Byansi has the two terms *mayeh* and *namsia*[4], Chaudansi only *namsia*, for *bhanj* there covers DH as well as ZC ms and WBC. Logically the distinction of children's spouses from opposite-sex sibling's children is an unnecessary elaboration of a symmetrical prescriptive terminology, comparable with that of spouse from potential spouse, or spouse's parent from potential spouse's parent, in general with that of actual affine from potential affine.

III

It is ego's level that presents the main analytical challenge in Byansi. In theory it could all be so simple If for brevity we ignore affinal terms and specifications (which do not affect the principles to be discussed), then we could imagine a middle level for fig. 1 made up straightforwardly as follows:

1A	2A	3A	4A

1A = term(s) for male siblings and male parallel cousins
2A = „ „ female cross-cousins
3A = „ „ female siblings and female parallel cousins
4A = „ „ male cross-cousins

It is helpful to think of ego as a special case among his or her same-sex siblings; thus male ego would locate himself in the same box as 1A, and female ego would locate herself in the same box as 3A The A is to signify that the classification depends on the *absolute* sex of the referent.

A second theoretical possibility, just as straightforward, is that the terms should classify consistently by *relative* sex, as in the following arrangement:

1R	2R	3R	4R

1R = term(s) for same-sex siblings, and parallel cousins of same sex as ego
2R = „ „ cross-cousins of opposite sex to ego
3R = „ „ opposite-~~sex~~-siblings, and parallel cousins of opposite sex to ego
4R = „ „ cross-cousins of same sex as ego

Here, whatever the sex of ego, he or she may be thought of as belonging in the same box as 1R. Certain features of the arrangement are just as before. Relatives referred to by terms falling on one side of the double vertical line still do not intermarry with relatives whose terms fall on the other side. Within a section marriage still occurs only between relatives whose terms fall in different boxes. However, as the wavy lines emphasise, the relationship between such adjacent boxes has changed: which box now contains the terms referring to relatives of a particular sex now depends on the sex of ego.

Both these theoretical possibilities for classifying relatives in a symmetrical prescriptive system are constructed by the cross-cutting of two dichotomous criteria. The distinction between parallel and cross is the same in both cases and the difference lies in the second criterion In the first case this bears on the sex of only one party to the relationship, namely the referent. In the second case it concerns the relative sex of both parties. There is evidently yet a third possibility, that it should bear only on the sex of the speaker. It is unlikely for several reasons that a terminology employing this third method would actually occur, but the theoretical possibility deserves mention. It too would be a classification of the relationships according to the absolute sex of one party, and from the fact that its focus comprehends both the absolute sex poles, the classification by relative sex derives a certain quality of centrality or balance. The formal elegance of symmetrical prescriptive terminologies has often been commented on, but it would, I think, be considerably enhanced by consistent use of classification by relative sex in ego's level.[5]

Be this as it may, much of the apparent complexity of the Byansi middle level can be resolved by viewing it as a mixture of the first two theoretical possibilities just discussed Still limiting discussion to cognatic specifications, we may begin with the cross-cousin terms.

tete FZS ms, MBS ms, FZD ws, MBD ws

This corresponds precisely to the theoretical 4R, in the sense that it covers the whole field of 4R and nothing outside it. In the same sense,

poa FZS ws, MBS ws
chhemain FZD ms, MBD ms
} taken together, correspond to 2R.

However there are no terms or combinations of terms corresponding to 1R or 3R; instead, this portion of the field is structured on the basis of absolute sex.

yha eB, FBSe, MZSe
nunu yB, FBSy, MZSy
} correspond to 1A.

pota eZ ms, FBDe ms, MZDe ms
tata eZ ws, FBDe ws, MZDe ws
ringsha yZ, FBDy, MZDy
} correspond to 3A.

It may be noted in passing that just as in the parent's generation the splitting of categories by age does not apply to their opposite-sex siblings, so in ego's it does not apply to cross-cousins. In other words the splitting is limited to boxes that include members of ego's nuclear family. The converse distribution would be surprising.

The difficulty lies in the relationship of *pota* and *tata*. The morphological analogy with the pair *pochi* and *chichi* suggests that *pota* was formed from *tata* with the aid

of the same prefix. This does not however necessarily imply that *tata* formerly covered the semantic field of both terms, as is done in Chaudansi by *ata* = eZ, FBDe, MZDe, WeBW, HeBW, SWM. It could well be that the etymological origin of *pota* was *tata*, but that the area it has appropriated in the structure formerly belonged to some other term with a quite different specification, for instance to one meaning 'elder opposite-sex sibling'. To justify this speculation we may again stand back from the Byansi and look at the matter from a more abstract viewpoint.

IV

In regard to their relationship to sex of ego, there are two kinds of kinship term. (i) Most terms are neutral and their specifications either make no reference to sex of ego or else do so symmetrically, e.g. terms for spouse's mother or opposite-sex sibling's son. In the second example the notation so far used necessitates writing the sex of speaker in the specifications, but this is solely the result of the choice of symbols, the specifications come in pairs, one for male ego, one for female, and by introducing the symbols os and ss we could eliminate the use of ms and ws. (ii) Other terms are asymmetrical with regard to sex of ego and no device or paraphrase can avoid mentioning it. The second type may be referred to as 'marked for speaker's sex'. Now we have seen that in Byansi at ego's level two contrasting principles of classification are used, one for cross-cousins, one for other relatives. If applied consistently, neither principle generates terms that are marked for speaker's sex (cf the definitions above of 1A–4R) But if in the course of its history the terminology were to change from one principle to the other, either there would be an abrupt discontinuity, or a period of gross confusion, or else the system would necessarily pass through a phase containing terms marked for speaker's sex.

The point is illustrated in fig 2, where, as an example, the Byansi cross-cousin terms have been inserted, exactly the same principles would apply to a transformation involving siblings or parallel cousins. Suppose the original classification had been consistently by relative sex Column 2R would then have been covered by a

		relative sex	
		opposite to ego (2R)	same as ego (4R)
absolute sex of referent	male (4A)	*poa*	*tete*
	female (2A)	*chheman*	

FIGURE 2, to illustrate the possible paths of transformation between a classification of cross-cousins based on relative sex (columns) and one based on absolute sex (rows). A term confined to one quarter of the semantic field is necessarily marked for speaker's sex. If the field were dichotomised according to absolute sex of speaker (rather than of referent), the two terms would appear in the figure neither in columns nor in rows but in diagonally opposite quarters.

single term, as column 4R actually is. How could this classification change so as to become based consistently on absolute sex? If the columns simply swivelled in one or other direction so that the same two words covered rows instead of columns, then during the period of transition, the terms covering two of the four quarters, in other words half the total field, would be ambiguous. A much smoother change would result if at least one new term were introduced. It could either take over one half-column or, less plausibly, occupy a whole row straight-away. Several sequences are imaginable in which terms annex adjacent quarters, new terms replace old without change of meaning, or a fourth term is introduced, but sooner or later the way is clear for one or more mergers to lead back to a two-term system, but one that is now aligned in rows The important point is that whenever a term is confined within a single quarter it is necessarily marked for speaker's sex.

In being based on classification by relative sex, Byansi *tete* is a rarity in Tibeto-Burman terminologies, at any rate in those reported from the middle third of the Himalayan chain, but it is not unique. Benedict (1941: 314) defines Tibetan *nu* straightforwardly as younger sibling of same sex as ego, and the Thulung Rai of east Nepal (writer's unpublished field work) have a term *ri* meaning 'opposite-sex sibling', and another, *wanceo,* meaning 'GEG of same sex as ego'. Given that both classificatory principles are employed in the same cultural area in related languages, it is natural to look for transformations between them and to try and interpret terms marked for speaker's sex as transitional stages in the process This is not to claim that such terms could only arise in this way, but there can be no doubt that the postulated transformation does take place in some instances As it happens, the two sibling terms we have just quoted as based on relative sex are etymologically related to the Byansi terms *nunu* and *ringsha* which classify by absolute sex [6] In the second case even the predicted transitional form is attested in Gurung *ri* or *rĩ*=Z 'employé surtout par un homme' (Pignède 1966: 282–3) and in Benedict's *sring*= Z ms. There is further cause to suspect a transformation in the Byansi case because of the contrast with Chaudansi, which makes no use anywhere of relative sex classification With two major exceptions, all the specifications of the three Byansi cross-cousin terms (including their affinal ones) are parcelled out by absolute sex, *chhe* for male referents, *chhemain* for female ones The major exceptions are FZD and MBD which Lall states he had forgotten, though he is sure they were the same, the minor ones are due to the splitting by age of some of the specifications, but his reporting here is not entirely clear and in any case no terms are involved other than *chhe* and *chhemain.* We noted above that Chaudansi has no eZ terms marked for speaker's sex, and the same is true of its remaining sibling terms.

Assuming that a transformation is indeed occurring in Byansi, the next question is in which direction. Though it is not a logically necessary one, it is a reasonable and economical assumption that the direction is the same with siblings and parallel cousins as with cross-cousins. Several reasons combine to suggest that the movement is away from the relative sex classification and towards the absolute sex one exemplified by Chaudansi. The most abstract and least cogent reason, perhaps more an evolutionary intuition than an argument, is as follows. We have already suggested that the absolute sex classification is the less logically elegant of the two. One aspect of this is that it focuses on the substance (male/female) of the referent, leaving the relation (same-sex/opposite-sex) to be inferred (for one will normally

know the sex of an ego). This focus on the actual difference between the sexes, rather than on the fact that they are different, seems to be a precondition for the development of lineality, a form of asymmetry which, as we point out later, represents a complication of a symmetrical prescriptive terminology. If the relative sex classification is indeed not only more elegant but also less complicated than the absolute sex one, then it is tempting to predict a general trend from the former to the latter.

Secondly, we have seen that the use of relative sex classification is rare, whereas that of absolute sex is common, in fact ordinary, both in Bodic and Indic. Moreover according to Benedict (1941: 319), sibling terms marked for sex of speaker are 'archaic for the (Tibeto-Burman) group as a whole' This consideration gains force when Byansi is compared with Chaudansi. Theoretically the close similarity of the languages might have resulted from convergence, due to the influence of one on the other, but there is no evidence in favour of this possibility, and it is far more likely that they have diverged from an earlier common language, in other words that they once shared a similar kinship terminology. In that case it is *a priori* more plausible that Chaudansi should have changed so as to follow the common pattern than that Byansi should have introduced for elder sister an 'archaic' distinction that moves it in the direction of a rare pattern. Similarly for cross-cousins, if the three-term Byansi pattern of fig. 2 derived from the presumably two-term Chaudansi one, two half rows must have been invaded by *tete*, whose specifications make it unique in the area, and whose reduplicate morphology contrasts with that of the more obvious or plausible innovations, such as borrowings from Pahari or compounds made up of two distinct morphemes One meets no comparable difficulties in imagining the opposite transformation, in which the column belonging to *tete* would have been absorbed with the aid of a third term formed from *chhe* by means of a feminine suffix *-main* (seen also in *chamain* or *chame*=D, and perhaps in *pima*=HZD). In the absence of *chhe*, Byansi *chhemain* appears curiously out of place; perhaps the most plausible explanation is that *poa* has displaced an earlier *chhe* (cf. note 4)

Finally, the fact that the Chaudansi are the less Mongoloid in appearance (Srivastava 1966: 177), and living to the south in a warmer climate, have doubtless undergone more Pahari acculturation, adds a little further support to the view that it is they who have diverged from the Byansi rather than vice versa. So it is at least a reasonable hypothesis that the central level in Byansi was in an earlier period of the language structured consistently by relative sex.

V

It remains to consider specifications in ego's level resulting from marriages in that or the children's level For the cross-cousin terms the additional specifications are:

tete	WB,	HZ,	HMDe,	BW ws,	yZH ms (but ZH ms on p. 195), SWF
poa	HB,		HMSe,		yZH ws (but ZH ws on p. 194), eZH ms
chhemain	WZ,			BWe ms, (but BW ms on p. 195)	

The spouse's siblings and half-siblings present few problems. There is a special term *mase* for HMSy but none is given for HMDy or for the other spouse's half-sibling possibilities. Probably the relative age criterion would apply here between referent and husband rather than referent and ego, but the point is left uncertain. For sibling's spouses most of the apparent anomalies disappear if one follows Lall's text rather than his table (where eZH ws is left blank and BWy ms given as 'addressed by name'). The immediate reaction to *poa*= eZH ms might be to dismiss it as a slip, but if, as we have suggested, *tete* is doomed to be absorbed by *poa* and *chhemain* into an absolute sex classification, the doubt over eZH ms might be an early symptom, with male ego taking the lead. Being the more travelled sex, they might be expected to be the innovators, as they apparently were with regard to *bhanj*

A few remaining departures from strict logical consistency may be briefly noted. There is a special term *chharpeo*= WZH used reciprocally by the husbands of two sisters, but no equivalent for HBW. It is natural that a male speaker should use *tete* for SWF as in table 1, but a woman speaker should refer to *her* SWF by a term meaning 'brother', whereas *tete* would be appropriate for her SWM. Presumably either Lall here omitted to record the female ego's usage, or else in tracing relationships through her children a wife follows her husband's usage. Thirdly, although SWM ms is appropriately referred to as 'sister', one would expect, if it is to be elder, *pota*= eZ ms and not *tata*= eZ ws. Finally, WeBW, which is given as either *pota* or *tata*, logically belongs only to the former.[7]

The second ascending and descending levels are agreeably straightforward. No term is given for grand-daughter but it would not be surprising if it were the same as that for grandson. In any case *khwe* resembles the grandparent terms in making no distinction between male and female descent. This is but another aspect of the symmetry of the whole terminology, in which lineality has no place. In fig. 1 the terms for brother have been placed in the box below that of father and above that of son, but it would have been no less logical to align vertically mother, sister and daughter on the left of the double vertical line, in which case brother and son would naturally fall to its right. The arrangement chosen may appear more appropriate since, as it happens, Byansi clans are patrilineal, but the fact is irrelevant to an analysis of this sort of terminology. Students of south India and Ceylon have often noted the absence of expression of lineality in the symmetrical prescriptive terminologies of that region (e.g. Dumont 1953 38; Yalman 1969. 620), but it may be worth emphasising the point (cf. fig 3). To distinguish two sorts of grandfather or grandson would be to introduce a wholly new classificatory criterion. Up to this point the terminology would have recognised only a duality of sexes, but now it would have to recognise a difference in kind between them, i.e. a difference in respect of their transmission of relationship between generations.[8]

Byansi provides, in Lall's words, 'a beautiful example of the intimate connection between the language used and the marriage customs of a tribe', cross-cousin marriage is 'common—nay the rule'. Accordingly to Srivastava (1953: 198) it is 'carried to monopolistic limits', and a girl may only marry elsewhere if MBS or FZS refuses or is already married. As one would expect, marriage with parallel cousins is prohibited (Sherring 1905: 105). It is well known that in south India (Dumont 1957) and elsewhere (Needham 1966–67), symmetrical prescriptive

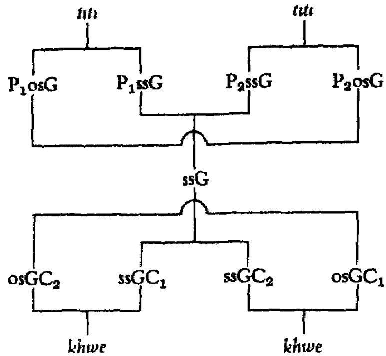


FIGURE 3 Idealised representation of part of a symmetrical prescriptive terminology with Byansi terms inserted in the outermost levels Subscript numbers indicate different sexes within the classes of parents or children, but provided one is consistent within these classes, which sex is assigned to which number (or to ego) is immaterial This form of symmetry would be destroyed at once by distinguishing one sort of *titi* or *khwe* from the other.

terminologies may be used by peoples who consistently practise asymmetrical alliance, but the reported norms give no hint here of any lateral preference. In general villages are exogamous (e.g Mehra & Tiwari 1955: 12), but at least one village, Kuti, consists of two exogamous units (Srivastava 1953: 194). As it is one of the most populous, one wonders whether it was the first Byansi settlement, in which case it would presumably have shown a classical dual organisation such as can be found elsewhere on the fringes of the Bodic-speaking world (notably at Malana in the Panjab Himalaya—see Rosser 1952–3: 70). But not a great deal is known about alliance among the Byansi

VI

The first purpose of this article was to establish the existence of a symmetrical prescriptive terminology in a cultural area where it has previously been generally ignored. Such terminologies are rare enough perhaps, for this to be worth doing for its own sake, but it is interesting to glance at the wider implications. Byansi and Chaudansi are not unique in the area The language of Malana is classified in another branch of West Himalayish, and it too has a symmetrical prescriptive terminology ('normal Dakota type'), though unfortunately few details are reported (Rosser 1956: 196). Moreover all the Tibeto-Burman terminologies reported from Nepal contain symmetrical elements, though none is anything like so consistent as Byansi. It used to be thought (Konow 1909: 179, 427–8) that the Himalayan languages, especially the western ones, showed traces, underlying the obvious Tibeto-Burman features, of a Munda or Austro-Asiatic substratum. In that case one line of thought would be to relate the Himalayan terminologies to those of pre-Aryan India. However the Munda substratum theory is now out of favour, and in any case the more tractable problems lie in relating them to other termi-

nologies within Tibeto-Burman, or even Sino-Tibetan. This goal has provided a general orientation in pursuing the second objective of the article, the analysis.

Kinship terms refer to relations between an ego and a class of relatives, and classificatory principles are deduced from the relations between kinship terms; so the discussion above of the interplay between the classificatory principles of relative and absolute sex can be seen as an attempt to grapple, as concretely as possible, with what Evans-Pritchard has memorably called 'relations between relations between relations'. But the emphasis of the analysis has been less on the properties of the system regarded as a synchronic whole than on its diachronic potentialities. These are of interest in themselves, since ultimately a typology of the changes to which terminologies and their component parts are susceptible is at least as interesting an objective as a typology of their states at particular times. But the main purpose has been to make a more local contribution, towards the vexed problems of the history of Sino-Tibetan kinship. The area is an important one for diachronic kinship studies, not only because of the time span for which written records of a sort are available, but also because of the variety of kinship systems represented, from the prescriptive symmetrical, through mixed types (e.g. that of the Garo), to the prescriptive asymmetry of such anthropologically celebrated peoples as the Purum and Kachin, as well as the non-prescriptive systems of the large-scale societies of recent China and Tibet. Benedict's well-known analysis ends with the ambiguously worded conclusion that 'cross-cousin marriage' was a conspicuous feature of an ancient cultural stratum underlying both the Chinese and Tibeto-Burman cultures. Lévi-Strauss (1969: 375) considers that the statement 'naturally' refers to asymmetrical (matrilateral) cross-cousin marriage, but the matter is by no means so straightforward; Benedict's same final paragraph begins by mentioning the archaic equations 'mother-in-law' = FZ and 'father-in-law' = MB, which are diagnostic of a symmetrical terminology. As I hope to show elsewhere, certain Tibeto-Burman terminologies from Nepal can best be understood as having resulted from the opening out of the symmetry represented by Byansi, rather than from the infolding of an asymmetric system, and if this is true of one fringe of the Sino-Tibetan world, it could be no less true of the whole area. If the details of these processes are to be worked out with confidence, as many terminologies as possible need to be recorded in detail before the tribal languages in which they are expressed are displaced or infiltrated by the larger scale regional and national languages.

NOTES

This article is an expanded version of part of an Oxford University B Litt thesis (1969) entitled 'Some problems in the ethnography of the peoples of Nepal and their neighbours'. I gratefully acknowledge the stimulus and example of Dr R. Needham, and the financial support of the Nuffield Foundation The field research referred to briefly was financed by the Social Science Research Council

[1] Most of the symbols used are conventional and their meaning will be obvious. Possible exceptions are Z 'sister', G 'sibling', E 'spouse', P 'parent' Where necessary sex of ego is specified by ms 'male speaking' and ws 'woman speaking'. We also occasionally use ss 'same-sex' and os 'opposite-sex', which are to be taken as applying in the same manner as e 'elder' and y 'younger'. Thus in PssGC 'parallel cousin', the term that immediately follows is compared with the term that *immediately precedes If nothing precedes, the comparison is with ego,* as in ssGC 'child of ego's same-sex sibling'. If nothing follows, it is the whole preceding expression that is compared with ego, as in GCss 'nephew or niece, whichever is of the same sex as ego'.

[2] This might be interpreted as evidence that *puni* or *pooni* was a later formation than *chini*, the new suffix serving to differentiate actual spouse's mother from potential spouse's mother.

[3] Certain puzzles still remain in this level of the terminology. Assuming *chichi* and *chini* share the same morpheme, its generational distribution appears anomalous in a two-section terminology, and may perhaps be compared with that of Tibeto-Burman **k'u*. In most of the subfamily the latter root appears in terms for MB, but in Tibetan and languages affected by Tibetan it has shifted so as to mean FB, a puzzling phenomenon that has exercised both Benedict (1941: 316–8) and Lévi-Strauss (1969: 371–2). Incidentally, the basic Tibetan root *ne*=FZ is applied loosely in the parental generation, but in saying that one finds *ne* for MZ, the latter authority implies too precise a parallel with **k'u*. Another curious feature of *chichi* is its apparent relationship with Pahari *chacha*=Fyb, MyZH and *chachi*=MyZ, which have many Indic cognates. Similarly the term *kaku* resembles both Pahari *kaka*=FyB and Benedict's basic Tibetan *khu*=FB, which is often found unaspirated, or did *kaku*'s final *-u* arise by some analogy with *ba* and *babu*?

[4] Sherring (1905: 113) writes *mayeh* as *mye*, giving it as 'son-in-law, brother-in-law, uncle-in-law', it is compounded with *chame* 'girl' to form *myechame* 'sisters etc.' of ego, or of her husband if ego is female. At the final death ritual this category of relatives is distinguished from the *puwahiya*, a man's mother's brothers, a word only used in connexion with this ceremony. The information, though approximate, and part of a generalised description of Byans, Chaudans and a third more Hinduised valley, is interesting since it hints at an asymmetry absent from Lall's account. Presumably *mayah* is cognate with Benedict's *mag* 'DH, bridegroom', and its extension in Sherring's data to include members of higher generations is historically secondary. It can be argued that the same process has been occurring with Gurung *moh/mo*=DH, ZH . . , but this would be outside the scope of the present paper. Similarly *puwahiya* is presumably related to Lall's *poa*, and the meaning limited to ritual contexts is likely to be the more conservative. So we seem to have an instance of the usual Omaha equation of wife-givers with a superior generation and wife-takers with an inferior one. (Whether there is a relationship between *-hiya* and Lall's *yha* is unclear.)

For *namsia* cf. Tib. *mNa-ma* 'SW, bride'. The second syllable appears to be a feminine suffix, cf. *rithi shia* versus *rithi*, *ringsha* or Konow's *rangsya*, also *shasha* (Srivastava 1953: 201) or *shyasya* (Sherring 1905: 107) 'bridesmaids'. Surprisingly, both Sherring (1905 and in his more popular book 1906: 109) and Srivastava (1957) give *byolishya* for bridegroom versus *byolo* for bride, but the latter's account, though not acknowledging those of Sherring, contains so many passages of identical or closely similar wording that an error in the earlier source has surely been repeated in the later one. Compare respectively Nepali *byaulo*, *-i* or Kumaoni *byolo*, *-i* (Turner 1931: 463), which have the expected endings for their genders.

[5] Among other consequences all terms in that level would then apply reciprocally, i.e. the same term would apply to a relationship between two individuals whichever of them was taken as ego. Secondly the whole system would be conceptually simpler in that what I have called the two cross-cutting criteria would actually be one criterion (same-sex/opposite-sex) applied twice over: a relative of the catetory 2R is someone of the opposite sex whose parents are opposite-sex siblings to one's own parents (PosGCos). The notion of bilateral cross-cousin can of course be expressed in absolute sex terminology, but only circuitously, by means of an equation (MBC=FZC).

It is interesting as an exercise to imagine a terminology which consistently employed the relative sex principle between generations as well as within ego's generation. There are certainly senses in which male ego's father is like female ego's mother, e.g. in the transmission of roles, and conversely M S··F D in the Freudian theory of complexes. But the absolute sex principle, giving F *ms*=F *ws* and so on, seems everywhere to prevail in ordinary languages, at least for parents. For grandparents, Murdock (1970: 167, 183) mentions a rare pattern reported from the Kenya Highland Bantu, who distinguish PPss from PPos.

[6] It might appear unduly speculative for one lacking formal linguistic training to make assertions about etymological relationships between languages that are ill-reported, largely monosyllabic, and mostly to some extent tonal, but in practice the difficulties with kinship terms seem less formidable than might be feared. In this instance Benedict himself confidently relates Ladakhi *nono* 'title of young nobleman' to classical Tib. *nu* and mentions Byansi *ringsha* in relation to *sring* (1941: 320, 335).

[7] In both these last two anomalies the easiest interpretation is that the male speaker is taking over the woman speaker's term. In the idiom of fig. 2, this could be the final phase of a transformation from a paired-column pattern for eG (e.g. opposite-sex, *yha*, same-sex *tata*) to a Chaudansi-like row pattern in which *pota* having fulfilled its function in easing the transition, will have disappeared again. If this speculation is correct, it remains unclear why for elder siblings the opposite-sex term should have ended up with the male referents, while for younger

siblings the swivelling has been in the other direction, so that the male row has ended up with the same-sex sibling root *nu*. It is encouraging that Benedict's list (1941: 320) of cognates from different languages containing the element *ta* confirms that Tibeto-Burman kinship terms do show unexpected shifts between sexes (as well as between generations).

[8] In comparing the Kariera and Dravidian kinship systems, Dumont (1962: 36–7 fn. 16; 1970: 285) apparently regards the former, which does distinguish terminologically between paternal and maternal grandparents, as being more basic, in a logical or perhaps in a general evolutionary sense, than the latter, which does not make the distinction. Terminology is only part of a kinship system, but as we have seen, to introduce lineality into the Byansi (or Dravidian) terminology would be to make it more complicated, not less. In fact, the most striking feature of such terminologies is surely the extreme simplicity of their underlying structure, however we are to formulate it. Thus it seems exaggerated to claim that 'the cognitive symmetry is too strong to suggest anything but *a series of brilliant theoretical attempts in the past* to order sexual and marital relations' by means of the semantic structure (Yalman 1969 625, my emphasis). One route to an intuitive appreciation of their simplicity is to conceive of the elementary relations involved not as filiation (or parenthood), siblingship and marriage (or affinity), but as filiation, same-sex siblingship and opposite-sex siblingship, marriage and its positive rules being regarded as secondary to these elementary relations Opposite-sex siblingship is fundamental, terms denoting those who have this mutual relationship belong on opposite sides of the central double line of fig. 1, and the separateness of the terms is a reflection of the sibling incest prohibition The complementary relation of same-sex siblingship is one of such emphatic closeness that any terminological separation is an inessential elaboration; terms for those having this mutual relationship belong in the same box of fig. 1, and the children of people so related are treated by the terminology just as are the children of a single parent (indeed siblingship, the sharing of parents, can be regarded as a special case of parallel cousinship, the sharing of parental same-sex siblings, in the same sense as relationship to oneself is a special case of relationship to same-sex siblings). If parallel cousins are classificatory siblings, and siblingship precludes marriage, then, in the absense of complications, cross-cousins are merely the nearest marriage partners that are left.

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EXAMINING STRATIFICATION SYSTEMS THROUGH PEASANT MARKETING ARRANGEMENTS: AN APPLICATION OF SOME MODELS FROM ECONOMIC GEOGRAPHY

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This article presents a detailed analysis of market structure in a region with pronounced economic stratification, western Guatemala. Locational models derived from central-place theory are utilised to this end A locational analysis of the Guatemalan data suggests that the type of marketing or central-place system found in a region is closely linked to the type of stratification system. Thus the theoretical issue to be addressed here is how the organisation of an exchange system affects stratification in agrarian ('peasant') societies.

Most anthropologists view economic stratification as an outcome of production relations. For example, Wolf bases his 'class' definition of peasantry on property systems, observing that payment of a fund of rent and lack of control over the means of production are the distinguishing characteristics of the class termed peasantry (1966: 3–10). This kind of production approach cannot account for several cases of clearly stratified, classically agrarian societies where the peasant producer owns the means of production and pays minimal if any form of tax to economic superiors. Examples include the Paez Indians of Colombia (Ortiz 1967) and the Mayan Indians of Guatemala (Colby & van den Berghe 1969). Most would agree that there is clear economic inequality in the two societies cited above, and that some sort of economic inequality is basic to the notion of stratification. So the question becomes: What is the basis for economic inequality in those stratified societies where the means of production are not alienated from the producer? I will argue that if the answer does not lie in production relations, it might lie in exchange relations.

It has been argued that models of exchange are unable to explain economic inequality because the values traded or reciprocated in all exchanges are roughly equal (Cook 1971; Edel 1972) I take issue with this rather traditional notion on the following grounds. In the first place, it is simply too facile to assume that all exchange systems are in equilibrium, wherein the values exchanged are equal. In the second place, anthropological exchange models (presumably exhausted by three types: reciprocity, redistribution, and market exchange) are so primitive and undeveloped that little can be done to link exchange systems with social phenomena. As a first step toward making an exchange analysis of stratification possible, I will attempt to add some exchange types to the anthropological repertoire, showing in particular four varieties of 'market exchange'. Three exchange types are developed from the classical models of central-place theory, and a fourth from empirical descriptions of distorted central-place arrangements. Analysis of the

Man (N.S.) 10, 95–122.

locational features of these market exchange types shows that only one of the four types places parties to exchange in equilibrium so that returns to trade are roughly equal. The next and most important step will be to show how particular types of exchange control in an economy are indicated by the locational principles that apply to empirical exchange systems, and thus how exchange control can be related to stratification.

To demonstrate my thesis, the four exchange models are applied to different sectors and periods in the market economy of western Guatemala, with emphasis on how differences in the spatial organisation of exchange affects stratification. Attention to the different exchange systems as bases for stratification in western Guatemala suggests how economic inequality can be maintained in agrarian societies without developed institutions for class control of the means of production. I submit that this may be generally true: that an examination of exchange relations will tell us more about stratification in agrarian societies than an examination of production relations, that production relations are critical mainly in industrial societies.

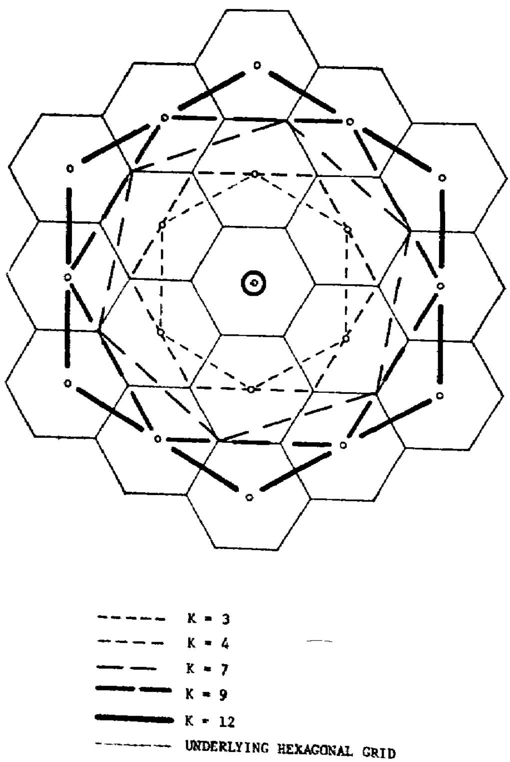
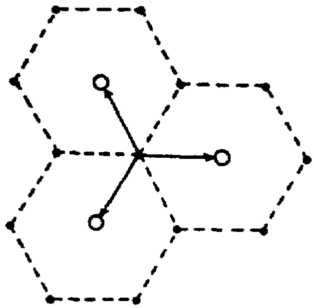
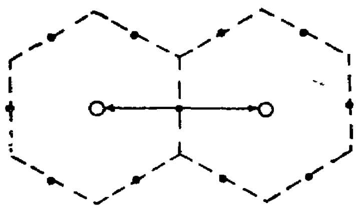


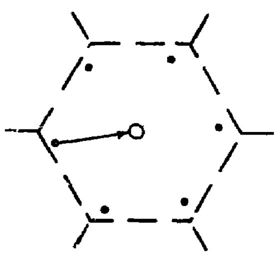
FIGURE 1. Classical Central Place Patterns and Nesting Arrangements.



K = 3



K = 4



K = 7

FIGURE 1. *continued.*

II

Four models of market exchange

Central-place theory, developed early in this century by Christaller (1933), was designed to explain the distribution of and economic relations in systems of market centres. Much modified since Christaller, the theory is still found useful, particularly for relatively simple agrarian systems.[1] Although central-place theory cannot be treated fully here, a brief statement of its principles and assumptions is

necessary. For more comprehensive accounts with reference to the relevant anthropological literature see Berry (1967) and Smith (1974).

Central-place theorists make three very strong assumptions in order to deal with ideal economic landscapes, i.e., for purposes of modelling: (1) an isotropic or featureless landscape with equal transport facility and even distribution of purchasing power in all directions; (2) rationality with respect to distance-cost minimisation on the part of consumers; and (3) perfect competition among numerous and profit-oriented suppliers. These assumptions are rarely met in the real world, but allow one to abstract the basic economic and locational principles of marketing systems, as follows. Characteristic of all central-place systems are at least two levels or a hierarchy of market centres, an even distribution of all centres in each level of the hierarchy, and a regular orientation of smaller centres to one or more large centres so that small centres nest within the hinterlands of progressively larger centres. Around each centre is a hexagonal tributary area or hinterland to which the centre provides services.[2] Suppliers space themselves over the landscape through a competitive process so as to saturate demand up to a point that a profit cannot be made, each type of supplier finding his own trade niche at certain levels of the market hierarchy. Hence suppliers of high-demand goods, located in the smaller centres, are more numerous and more closely spaced than suppliers of low-demand goods, located in the larger centres.

The hierarchical-spatial ordering of centres may be implemented in a number of ways, each with its own distinctive economic consequences. Figure 1 shows just a few of the ways—the three patterns with the highest ratio of large to small centres. Other more expansive patterns can and do occur, but are based on the same principles or a combination of principles that produce those shown, K=3, K=4, K=7.[3] Christaller termed these principles the 'marketing' principle (K=3), the 'transport' principle (K=4), and the 'administrative' principle (K=7). The marketing principle maximises consumer travel efficiency by minimising the number of lower-level centres in a system and locating them between three higher-level centres. This pattern seems to occur mainly when a significant portion of the consuming or producing population is dispersed in rural settlements, and when transport is both costly and primitive; it is the most 'efficient' system for predominantly rural landscapes The traffic or transport principle maximises the position of centres on a limited number of roads by locating lower-level centres between two higher-level centres, i.e., along already established roads. This arrangement seems to be most advantageous when a significant portion of the consuming population is located in central places themselves, and when the goods marketed come from industrial or specialised farm centres rather than from dispersed rural areas. The administrative principle divides territories discretely so that all lower-level centres orient to one and only one higher-level centre. Because no centres are located on hinterland boundaries, higher-level centres do not compete to service the same lower-level centres, although some competition among higher-level centres is necessary for each to maintain its own territory. This principle is admirably suited to carving up administrative districts, since each high-order centre controls its dependent centres and hinterland exclusively.

Each of Christaller's principles implies a different locus of exchange control for an economy and a different evolutionary history. The K=3 system is the most

equitable and competitive in locational terms. Economic power might be exercised more effectively from the larger centres, but the high ratio of large to small centres and the intensity of competition among all centres insures active participation of a large proportion of the population in the economy, little monopoly, and considerable economic mobility. Because location in a K=3 system would not affect position in the exchange economy significantly, one would not expect location to be deterministic of the stratification system. The general configuration of K=3 systems suggests that they would arise with the natural intensification of marketing and market dependence associated with population increase in a predominantly rural region.

The key feature of a K=4 system is transport, so it is to be expected that transport would be an important element of economic control. I would argue that control of transport by oligopolistic groups, certainly urban residents, is likely. There would be competition among urban-based middlemen and merchants, but smaller centres and rural areas would play little role in the regional economy except as consumers. Since the K=4 system is oriented towards urban centres rather than rural areas, the economic elite in such systems would probably always reside in urban centres. Conversely, position in the stratification system would be strongly affected simply by residence inside or outside urban centres, for control of the exchange economy would be best exercised from urban centres. A K=4 system could evolve from an earlier system with the development of urban industries and modern means of transportation; or it could be the result of an imposed economic system, where a politically dominant group based in urban centres attempts to control the already commercialised economy of a rural area [4] (Presumably the more efficient K=4 system would be chosen over the better administrative K=7 system only where the peasants were already involved in a market economy.)

As an economic arrangement, the K=7 system resembles the 'solar' marketing systems described by anthropologists who have worked in Latin America (Nash 1966; McBryde 1945). In these systems there is a low-level hierarchy, usually involving administrative town (elite) centres serviced by several small rural (peasant) marketplaces, but each marketing group is poorly articulated with others in the same region [5] Marketing exists because the elite are tied to the domestic economy for their subsistence; but poor market articulation occurs because the requirement of force overrides market efficiency. In such systems one would expect a marked economic break between three major groups: the rural peasants, the urban economic middlemen and low-level political administrators, and an 'outside' controlling group, often the political-economic elite of the home country in a colonial situation. Here economic position would clearly rest on position in the exchange economy; for by controlling the places, timing, and means of exchange, the dominant group and their retainers—always and usually exclusively located in the major central places—would control the economy more surely than by controlling production resources. Political power or force might be basic to the division between elite and peasant, but economic power in such a system would be most effectively exercised in the exchange economy. A K=7 system almost always suggests imposition of the economic system by rulers—perhaps the imposed arrangement of 'administered' trade, tribute collection, or the like by a colonial power in a region with an undeveloped market economy.

Another type of central-place system, termed 'dendritic' by Johnson (1970), has been described in the literature. Johnson does not specify the chief attributes of dendritic systems from the perspective of central-place theory, but they appear to be the following. All economic networks in a region converge on a single centre rather than on different nodes of a multi-centred, unbounded system. Because of this convergence, there is no competition among equivalent centres for the commerce of smaller centres, and thus dependent centres become progressively smaller with distance from the major or primary centre (see fig. 2). In consequence, the basic attributes of normal central-place systems—even distribution of all centres in each level of the hierarchy and nesting of small centres within the regular hexagonal territories of larger centres—no longer obtain.[6]

Contrary to appearances, the dendritic system is not a poorly articulated system, but rather an extremely well articulated, if non-competitive, system for vertical trade. It is particularly efficient for the kind of vertical trade common to third-world countries—moving primary goods (often plantation crops) out of the country. Monopolistic suppliers of goods at each level of the hierarchy are basic to dendritic systems. Thus it appears that dendritic systems develop when the assumption in central-place theory of perfect competition among numerous suppliers is seriously violated. This would be most likely to occur when a developing market economy is controlled by outside economic interests. One might expect it in the adjunct-export peasant economy described by Nash (1966), or in the backwaters of any modern economic system where the producers are tied to the economy primarily through their participation in export-commodity production.

The dendritic marketing model is similar to a von Thünen production model in what it suggests for stratification. Both models have a single dominant centre, and both describe economic advantage in terms of distance from that centre. The population in the inner zone (near the primary centre) is advantaged in their production-marketing operations, while the population in the outer zones (serviced only by small feeder centres) is seriously disadvantaged (see fig 3). This difference itself affects stratification because it tends to fragment class interests. People, particularly rural peasants, are more likely to see greater differences among themselves than between the peasant class and the removed, exploiting, entrepreneurial class. One expects a sharp, perhaps ethnic, break and rigid class distinctions between rural and urban classes and between economic middlemen and the major merchants, their economic positions related to their spatial positions in the system. As a result of the distance factor, one also expects differences within each class to depend largely on spatial position in the exchange system.

An imposed marketing arrangement is clearly implied by the dendritic pattern. But it would be imposition by an outside, international-market elite, with economic interests in the domestic marketing system rather than the administered-market elite of the K=7 system. The locus of this elite would be outside the system, and the marketing system would be oriented towards them—outside itself and the domestic economy. Economic stratification in the region would also be controlled from outside, and market-exchange forces would be basic to the stratification system.

The following description of the marketing system in western Guatemala should illustrate the connexion between exchange systems and stratification. I will first

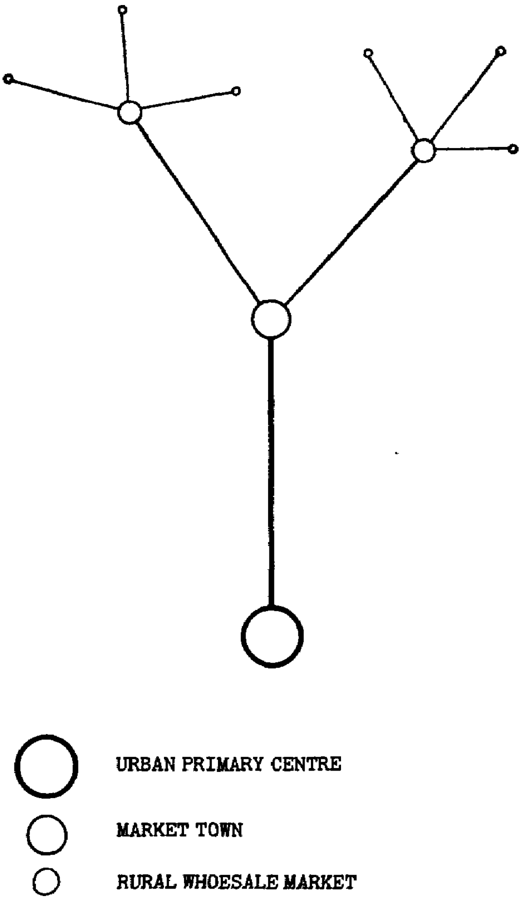


FIGURE 2. Dendritic Market Pattern.

describe the region, geographically, ethnically, and historically. Then I will describe the present organisation of marketing in the region, using the locational models described above to explicate the marketing patterns. (Marketing in Guatemala is complex, involving all four exchange systems; such complex patterning has not been described for any other marketing system to my knowledge, but then detailed regional marketing studies are few; I doubt that the Guatemalan marketing patterns are unique). Finally, I will posit an evolutionary sequence for the economic development of the region (based on the present organisation of marketing), linking marketing changes to historical changes in the stratification system.

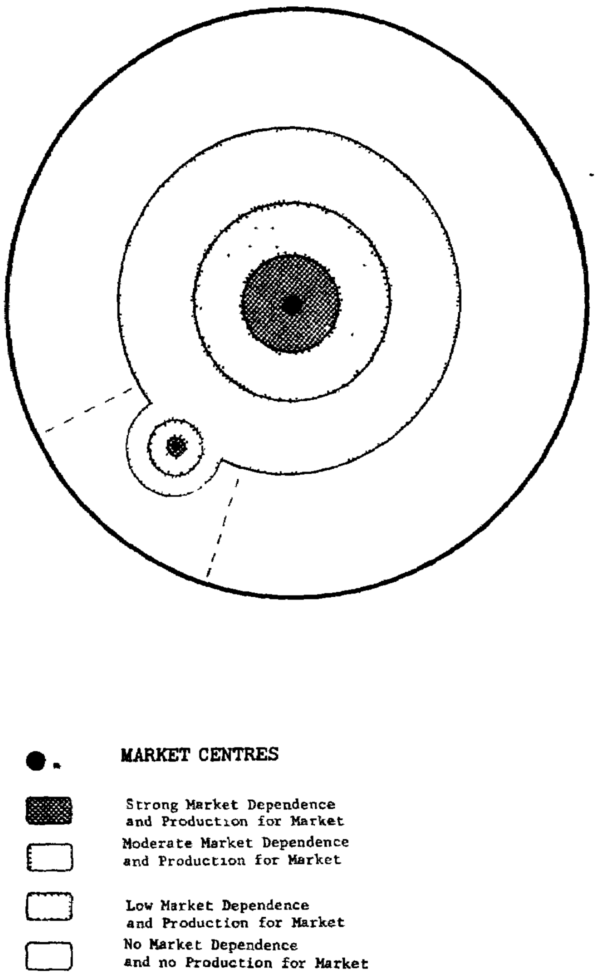


FIGURE 3 Von Thunen Pattern of Market Dependence.

III

The regional setting

The marketing system of western Guatemala is made up of the market centres that fall within the maximal hinterland of the large market town of Quezaltenango. Quezaltenango, the second largest city of Guatemala (urban population 40,000) is located approximately 175 km west of Guatemala City, the national capital and largest city of Guatemala (urban population 500,000). (A comparison of the two urban populations indicates the dominance of the national capital in the country.)

Quezaltenango is centrally located in the west, within a few kilometres of the significant highland-lowland division, a steep escarpment where the altitude drops from 2,000 to 200 m above sea level in less than 24 km. Its hinterland includes the nine western-most departments (administrative divisions) of Guatemala, both highland and lowland. So defined, the region of western Guatemala is about 30,000 square km in area, supports 1·8 million people, and includes some 300 central places. Thus the average population density is about 60 per sq. km; the average population dependent on each centre is about 6,000.[7] The terrain of the region is extremely rugged, ranging from sea level to about 3,500 m above sea level.

Altitude defines the major agricultural areas of the region. Figure 4, following Higbee (1947), shows the distribution of the most significant altitudinal zones and agricultural areas. Lowland zones lie at the periphery of the region, both north and south. Plantation crops, most importantly coffee, sugar, and cotton, are grown in the lowlands, while subsistence crops (corn, beans, wheat) predominate in the highlands. Population is concentrated in the central portion of the region, as are zones of high agricultural production intensity.[8] (Unevenness in these variables can lead to some distortion of central-place patterns, although in this case the causal relationships are not simple, this point will be covered more fully below). Reference to altitude zones, given the intervening variables of population density and soil fertility, can explain the distribution of agricultural regions in western Guatemala But these facts do not explain why population is concentrated in the high altitude zones, where agricultural productivity is limited by both soil fertility and growing season. The answer is historical, but not irrelevant for explaining the present distribution of major market centres. The historical development of central-place patterns determined the subsequent spatial relationship of the two major ethnic groups in Guatemala, as well as the type of marketing relationships that were to ensue.[9]

The historical period that must be considered is the period between the Conquest in 1524 and the present. At the time of the Conquest, the indigenous Mayan populations were concentrated in the highland area, their administrative centres in places that are still today administrative centres—Huehuetenango, Santa Cruz Quiché, Totonicapán, Quezaltenango, Sololá, and a number of lesser places. The chief significance of the pre-conquest situation is that the Spanish conquerors settled where the conquered peoples lived, since to them the major local resource was indigenous labour. Their colonial administrative centres were commonly located on the ruins of the pre-conquest centres. These settlements became the major central places in the region, first supplied through tribute and then through a 'solar' marketing system. The location of the colonial administrative centres essentially determined the subsequent development of the marketing system, as well as the zones of high and low production intensity; post-conquest population distribution was undoubtedly also influenced.

Guatemala gained independence from Spain in 1821, organising its administrative divisions and capitals on the economic base of the prior colonial state. But shortly thereafter (about 1880), the introduction of coffee—a plantation crop with an international market—transformed the region by relocating some people, centres, and administrative boundaries. While population remained concentrated



FIGURE 4 Major Agricultural Regions of Western Guatemala

in the central highlands, leading to intensive cultivation practices there, the lowland plantations were more economically rewarding, due to the favourable market price for their products. They also drew (and continue to draw) a very large seasonal labour force to the lowlands. Consequently, income distribution in the region no longer followed population density closely. (High economic productivity in the lowlands, coupled with dense population in the highlands, led to much greater uniformity of regional purchasing power—the major determinant of uniform central-place patterns.) By the turn of the century, the main features of the economy, polity, and marketing system relevant to the present period were established. Coffee plantations were located in the lowlands, adjacent to the intensively cultivated highlands, which supplied the plantations with labour and subsistence goods. The coffee crop was shipped out of the region through Guate-

mala City to the international market. And Quezaltenango was established as a major central place, its politico-economic role to control the western region and its marketing role to mediate exchanges between the highlands and lowlands.

At present there are two major ethnic groups in western Guatemala, Indians and non-Indians. Most non-Indians claim Spanish ancestry but are usually *mestizos* or simply 'acculturated' Indians;[10] in Guatemala, non-Indians are known as Ladinos and are almost always accorded higher status than Indians, regardless of wealth. The present economy divides the two ethnic groups into four distinct resource-control groups: (1) peasant farmers and craftsmen who are the economically 'active' half of the Indian population, they live in the central highlands and are devoted to producing goods for the domestic (regional) market on their own small plots of land or in their homes; they provide the bulk of handicrafts produced in the region and most marketplace traders; (2) peasant farmers and seasonal labourers who are the economically 'passive' half of the Indian population, they live in the peripheral areas of the highlands and work equally at subsistence farming and seasonal labour on the lowland plantations, (3) permanent agricultural labourers, who are also largely of Indian extraction, but who commonly adopt the dominant Ladino lifestyle; they live in the lowlands and typically are disenfranchised peasants, now a rural proletariat paid for regular work on or related to the plantations; and (4) an elite group that is Ladino, who live in both highlands and lowlands, but unlike the rest of the population, usually in urban centres; they control the plantations, the administrative apparatus, the transport network, and certain other aspects of the distributive economy detailed below

The true elite of the region probably numbers less than 10 per cent. of the population. Another 10 per cent of the population is so defined (as Ladino) and so distributed (in urban centres) as to be potential competitors to the elite; but they are in fact more often their retainers or employees. About 15 to 20 per cent. of the population are permanent agricultural labourers with a Ladino lifestyle but without many Ladino privileges Of the 60 to 65 per cent. of the population defined as Indian peasants, about half are economically active and half are not.

IV

The contemporary marketing system

The regional marketing system today is a very commercialised one. As Tax (1952) observed some time ago, regional specialisation is advanced, everyone is involved in a cash economy, and many people are dependent on the marketing system for most of their basic provisions. In order to show the broad articulation of exchange systems and ethnic groups in the complex, multi-faceted economy of western Guatemala, I will make reference to most of the regional models described earlier. Use of these models requires uniform or consistent measures of central places with respect to their commercial service functions There are, however, two kinds of marketing systems: the system of market towns which have many permanent commercial establishments; and the system of rural market centres which do not have town services. (While no major market town lacks a major marketplace, many major marketplaces are not found in towns.) Because they have

different functions, market towns and rural marketplaces are not strictly comparable. But since the two kinds of centres do articulate with one another in order to service everyone in the region, both aspects of the system will be described and the measurements used will reflect both town and marketplace service functions.[11] The following analysis is based on virtually complete data on all market centres—towns and marketplaces—in western Guatemala[12] (see fig. 5).

The basic structure of the regional exchange system is created by nineteen large Ladino-controlled market towns that are distributed regularly over the landscape.[13] (Members of this group of Ladino market towns will be termed LMTs in the

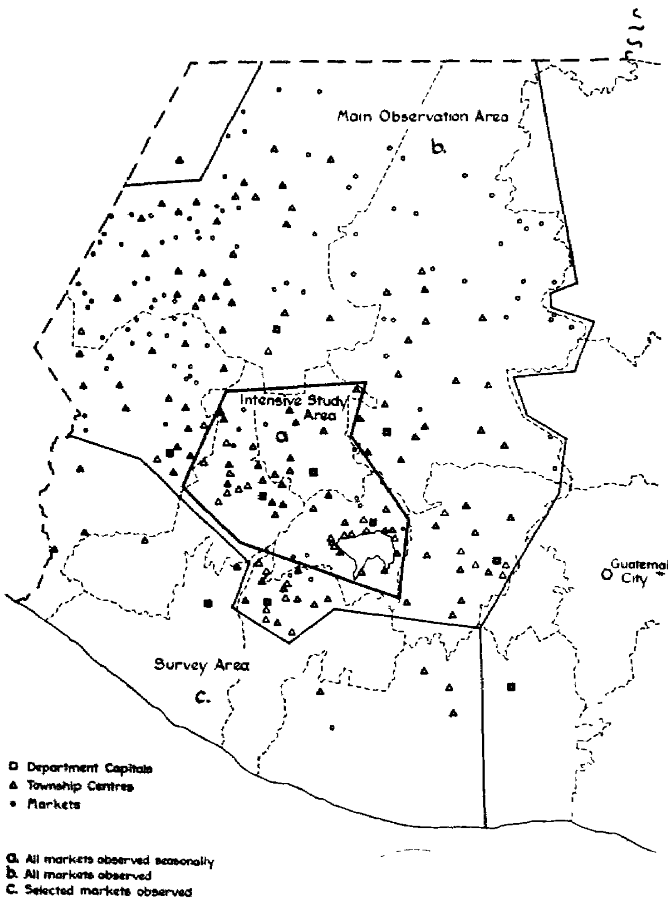


FIGURE 5. Observed Marketplaces and Study Areas of Western Guatemala*.

*All known marketplaces were observed in the highlands (main observation area); only selected marketplaces—probably all major marketplaces—were observed in the lowlands (survey area).

following discussion.) An outline of this system, shown in relation to production zones and major lines of communication, is shown in fig 6. The largest market town, Quezaltenango (centrality index[14] 7880, urban population 40,000), is also the administrative and industrial centre of the region. This central LMT is surrounded by six intermediate LMTs that are equivalent to each other as central places (average centrality index 2640, urban population 10,000).[15] They are linked in trade to each other mainly through the central LMT, Quezaltenango; and as trade centres they are only dependent on and secondary to Quezaltenango, the single higher-level LMT in the region All six intermediate LMTs are large commercial centres, all support large town marketplaces that meet daily, and all but one are also local administrative centres—department capitals like Quezaltenango,

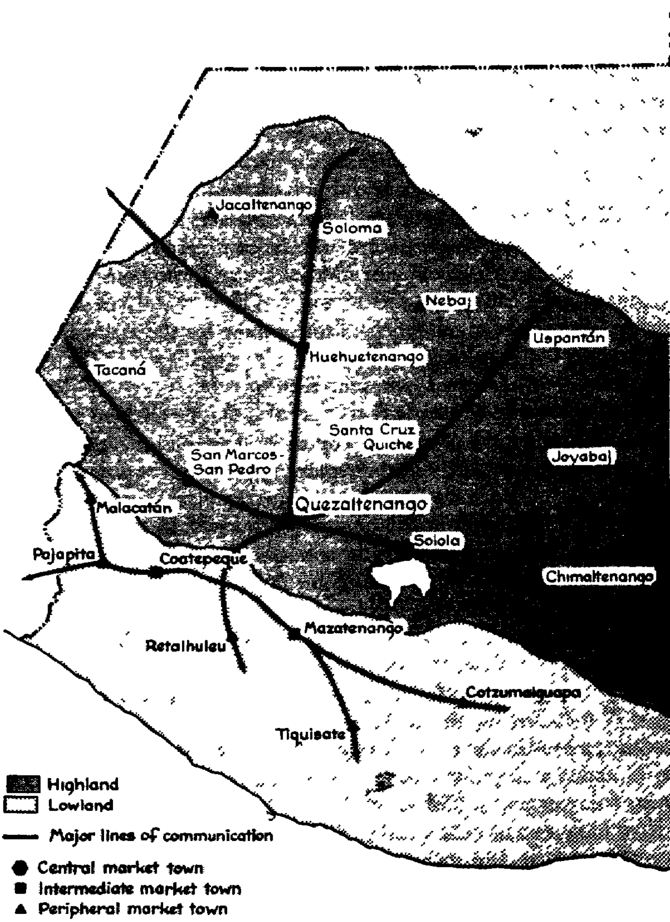


FIGURE 6. Major Ladino Market Towns in Western Guatemala.*

*All centres are shown in true relative locations.

but with fewer administrative functions. Each is about 50 km distant from Quezaltenango, linked to it by a major road. Each intermediate LMT is the major centre for a dependent hinterland that extends from the outer reaches of the regional system to the inner hinterland of the central LMT, Quezaltenango. (Quezaltenango has a local system dependent on it as well as a maximal hinterland—the regional system—that includes all the local systems in western Guatemala.)

The seven LMTs described above (termed the major LMTs) are surrounded by twelve other LMTs that are smaller and that are located near the periphery of the regional marketing system (they are termed minor LMTs). Six of these minor LMTs are located on the main roads that radiate out from the central LMT through the six intermediate LMTs and six are located between the major traffic routes, connected to two intermediate LMTs by secondary roads. All but two of the minor LMTs are smaller than the intermediate LMTs, both as marketplaces and as towns (average centrality index 208, urban population 2,000), and all but two lack administrative functions (The two exceptional cases, Chimaltenango and Retalhuleu, are larger than the other minor LMTs because of relatively new transport and administrative functions; they need not detain us since with respect to domestic marketing they share most characteristics of other minor LMTs.) Most minor LMTs are found in areas of high or average population density, but where production levels are low. They have smaller marketplaces than major LMTs because their dependent populations are poorer and less commercialised, thus utilising less in the way of trade goods. The minor LMT shops and warehouses are provisioned directly by major LMTs, but the minor LMT marketplaces are provisioned primarily by a local hinter and independent of those for the major LMTs. Thus as towns the minor LMTs belong to the maximal intermediate-LMT hinterlands, but as marketplaces they have their own hinterlands.

The common features of all LMTs that are also unique to them are: (1) they are associated with many large permanent commercial establishments; (2) they are the major transport (truck) centres in the region, (3) they have relatively large urban populations, heavily dependent on the town marketplaces for their food requirements; and (4) their urban populations are primarily Ladino, and the Ladinos control the major businesses as well as the administrative apparatus. Since permanent commercial establishments and trucks control the distribution of imports in the region (such things as iron tools, gasoline, kerosene, ready-made clothing), LMTs are critical supply centres for all sectors of the population. And since they have relatively large, wealthy, and market-dependent urban populations, they are also important consumption centres in the regional marketing system. In addition, the major LMTs as Ladino administrative centres control the region politically.

The nineteen market towns described above are the only true 'town' centres in western Guatemala. Each township in the region (some 175) has a cluster of permanent commercial establishments in its political centre, but only the nineteen LMTs have them in sufficient number to claim more than a single township as a dependent hinterland. On the other hand, a number of township centres have periodic marketplaces equivalent in size and diversity to the marketplaces found in the Ladino market towns; i.e., when calculated separately from the town centrality index, they have marketplace centrality indices nearly equal to those for the major LMT marketplaces.[16] These marketplaces are rural bulking centres (termed

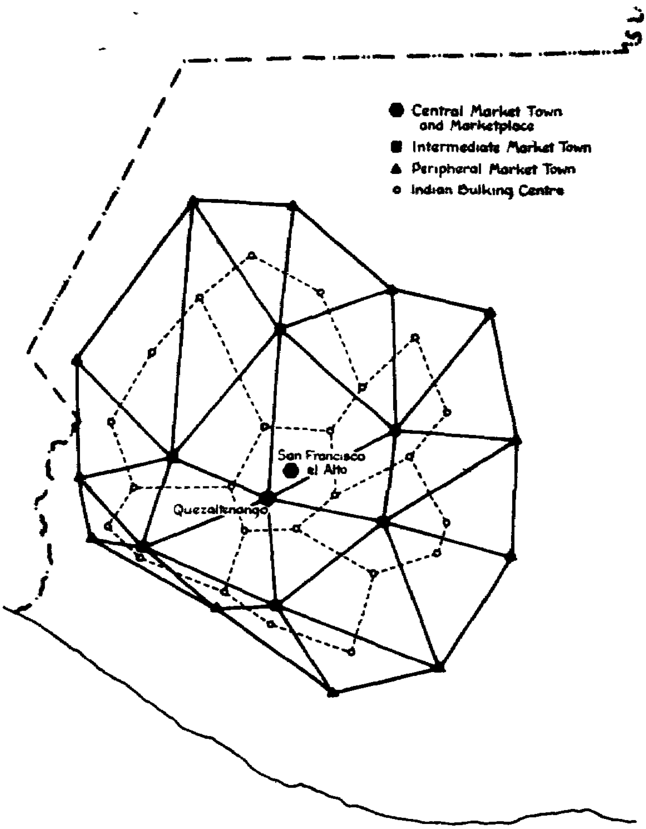


FIGURE 7 All Major Market Centres in Western Guatemala *

*All centres shown in approximately true relative locations. Solid lines connecting LMTs are drawn only to show that RBCs fall between three of them; they do not indicate lines of communication or trade flows Similarly, broken lines connecting RBCs are drawn only to show the hypothetical hinterland boundaries of LMTs, not to indicate trade flows.

RBCs) for goods produced primarily by Indian peasants in rural areas. RBCs contrast with LMTs both in that they have fewer permanent establishments (their town centrality indices average less than fifty) and in that they are oriented toward rural peasant production and consumption rather than urban Ladino provisioning. Most are located in townships of dispersed settlements where few Ladinos reside. The kinds of goods traded and the level of trade (about half retail and half wholesale) are similar in all of the RBCs but one. The exceptional case is San Francisco el Alto, the largest wholesale marketplace in western Guatemala, located only a few kilometres from Quezaltenango, the largest urban centre. The distinctive features of San Francisco el Alto—large size and extreme predominance of wholesaling—stem from its central position vis-à-vis other RBCs. Figure 7 shows the distribution

of RBCs in relation to LMTs. The RBCs are found in the interstices between three of the LMTs; when the RBCs are connected to each other, they form the outer boundaries of the LMT hinterlands.[17] San Francisco el Alto, the central RBC, shares the central position in the system with Quezaltenango, the central LMT; both centres are primary trade centres but handle different kinds of trade.

All the marketplaces in the centres described above (in both LMTs and RBCs) have marketplace centrality indices (calculated independently of the town centrality indices) of more than 350, contrasting with lesser market centres that have fewer goods and services and provision smaller hinterlands. (There are more than 200 of the smaller marketplaces.) Only San Francisco el Alto has a *marketplace* centrality value greater than 2000, contrasting with its *town* centrality value of 49; most other RBCs and LMTs have marketplace centrality values of about 500 Even Quezaltenango has a less important marketplace than San Francisco el Alto, although its marketplace is larger than those in most centres.

The distinctive features of RBCs compared with LMTs lie not in marketplace size and importance, but in lack of associated permanent establishments, and in different market distributive functions. RBCs bulk rural goods for distribution to urban centres, and have relatively minor retail functions; whereas LMTs by dealing in wholesale imported goods (primarily from Guatemala City) for distribution to smaller centres, have important marketplace retail functions, and relatively minor marketplace wholesale-bulking or redistributive functions. In brief, town goods (primarily goods produced outside of the region and imported to it) flow from the LMTs to small town centres, while marketplace goods (primarily goods produced in the rural areas of the region)[18] flow from minor marketplaces to RBCs for bulking, and from there to both other minor markets in the system and the LMTs, which must service large town populations with marketplace goods. LMTs are critical in the movement of town goods, whereas RBCs are critical in the transfer of marketplace goods

Before analysing the spatial patterning of the domestic marketing system, let me clarify some of the features of the pattern Three different levels of LMTs have been distinguished on the basis of town and marketplace central functions and the related size of hinterlands. The place that is central, Quezaltenango, is the highest-level centre; the surrounding ring of centres (intermediate LMTs) are second-level centres, and the outlying ring of centres (minor LMTs) are the third-level centres. Central-place theory predicts under conditions of even distribution of purchasing power that the second-level centres (centres of secondary importance in redistributive functions) will be more distant from the primary centre than third-level centres, in order to service larger hinterlands with the goods that third-level centres lack. In our case, they are closer, spatially distorting the hierarchical pattern of LMTs. The RBCs are in one sense different kinds of entities from LMTs because their main functions are wholesale rather than retail. In another sense, they form a lower level in the marketing system, since they lack some critical retail functions as towns and have other less relevant functions as marketplaces—i.e., they can be considered low-level retail centres but high-level wholesale centres. As wholesale centres, however, the RBCs show no pattern of hierarchical interrelationship except through LMTs. They do have one higher-level centre, San Francisco el Alto, but the relationship of most RBCs to that centre is indirect—at least spatially.

From both their locational pattern and their functions, it is clear that RBCs exist primarily to service the LMTs that surround them.

Another feature of the pattern that should be clarified is that while 'town' goods flow throughout the system, only the nineteen LMTs make up an integrated town system. Marketplaces and smaller centres are supplied either directly by LMTs (the retail seller goes to buy in one of them) or through long-distance traders whose movements do not conform to any central-place pattern. The long-distance trade system has been more fully described elsewhere (Smith 1972*a*; 1972*b*).

V

Central-place patterns and their development

The spatial patterns of the major central places[19] in western Guatemala, although unusual, are quite regular. The most unusual feature is that there are several *different* patterns, best explained by the fact that different regional sectors of the population supply and are supplied with different kinds of goods. There is also a general pattern that encompasses the others. I will first describe the different components of the general pattern to show how different trade systems structure economic relations. Then I will analyse the general pattern to show the socio-political aspects of the system

If we consider the organisation of the LMTs alone (fig. 6), ignoring for the moment the fact that centrality values for the minor LMTs are lower than expected, the pattern conforms to Christaller's 'traffic' arrangement, or K=4. As mentioned earlier, this pattern maximises position on a limited number of roads by locating lower-level centres along roads between two higher-level centres. In our case, twelve of the eighteen LMTs in the Quezaltenango hinterland are located on the six main roads that radiate out from Quezaltenango. These roads are the main arteries for urban trade. Along them flow most regional exports bulked for shipment to Guatemala City in LMTs, and virtually all regional imports received for retail trade by the commercial establishments in LMTs. Since the main commercial functions of LMTs are to move export commodities out of the region and to supply their urban populations with goods imported into the region, the traffic principle is a logical determinant of their spatial pattern. It should be noted in this context that the urban populations of most LMTs are Ladinos with consumption tastes that are different from rural Indian peasants. (Conspicuous consumption of manufactured imports is a fairly good indicator of Ladino status.) Because most of the Ladino elite live in LMTs, the proportion of imported goods sold outside of the nineteen LMTs is quite small. It is therefore not surprising that LMTs have an integrated spatial pattern independent of that for other centres.

When all major centres are considered together (fig. 7), counting RBCs as lower-level retail centres and again ignoring the fact that the centrality values of LMTs drop off at the periphery of the system, we find Christaller's classical 'marketing' arrangement of central places, or K=3. As mentioned before, this pattern has lower-level centres orienting to three higher level centres, efficient when the consuming population is dispersed in rural hamlets or small villages—as are the Indian peasants in western Guatemala. Indian peasants produce most of the marketed

foodstuffs, and these comprise some 80 per cent. of the goods found in marketplaces (in bulk).[20] A good proportion of foodstuffs marketed (I estimate about 50 per cent.) are destined for urban markets; the rest are sold and consumed locally. Since the commodity *flow* is primarily between RBCs and LMTs, it follows that their relationship accords with the rurally oriented 'marketing' principle. In this particular case, the K=3 distribution of RBCs in relation to LMTs shows one regular feature (all RBCs relate to three LMTs), and one irregular feature (only the major LMTs are serviced by six RBCs—the minor LMTs are serviced by only two or three RBCs).[21] Since RBCs are collecting points for rural goods that flow to urban centres, the implication is that demand from at least three LMTs is necessary in order for an RBC to be established. Another possible implication is that the minor LMTs are small because they are serviced by few RBCs; but as I shall point out below, that is putting the cart before the horse

There are several other peculiarities of the K=3 system in western Guatemala. One normally expects the relationship between the lower-level centre (RBC) and the three higher-level centres (LMTs) to which it orients will be mutual—i e., that the lower-level centre will provide the high-level centres with goods produced in the rural hinterland, and that it will receive specialised services from them. In this case, however, only the first part of the expectation is realised because few town goods flow from the three LMTs to the RBC The problem is that while the urban centres require massive food provisions from the rural centres, the rural centres (which provision Indian peasants) do not require many 'town' provisions. Hence, the basic town provisioning system does not extend beyond the LMTs to any significant degree. The possibility still exists for the LMTs to have the higher-level function of provisioning its dependent RBCs with rural goods produced in other parts of the region (for LMTs receive rural goods from many different sources). Yet by and large the LMTs do not provide this service: most of them have large retail marketplaces in which rural goods are sold, but undeveloped resources for the redistribution of rural goods. There is no obvious economic explanation for this A social reason is that Ladinos (who live in LMTs) do not normally engage in marketplace trade, and a politico-economic reason is that LMTs subsist less on their commercial services than on other services Neither of these explanations is adequate, however, without further analysis of LMT functions, to be detailed below

Since the spatial structure of LMTs can be explained at least partially without reference to RBCs, one might expect an independent structure for RBCs—the Ladino and Indian provisioning systems operating independently of each other. Instead we find that the spatial structure of RBCs is not explicable without reference to their relationships to LMTs—RBCs have no hierarchical relationship to each other. In consequence, Indian goods are not collected in one centre for redistribution to the other RBCs. (To some extent, San Francisco el Alto—the single higher-level RBC—does provide this service, but it must do so for the entire region and can only do so efficiently for the immediately adjacent RBCs.[22]) Specifically Indian goods (produced and consumed primarily by Indian peasants) *are* marketed, but the Indian provisioning system is regionally fragmented into many little marketing subsystems. One could compare the RBC system to the inefficient 'network' system frequently described by anthropologists who observe peasant marketing. But the RBC system is only one aspect of the regional system,

for there is a general organisation of RBCs that involves LMTs. The implication is that those forces responsible for LMT distribution, to which RBCs orient, are the prime movers in the organisation of the region.

Another spatial pattern of significance in the region is the 'solar' or K=7 pattern, expressed in the dominance of the major LMTs over the region as a whole and in the poor articulation of the major LMTs with one another in domestic trade. Why this pattern exists is suggested by some of the special functions of the major LMTs (Quezaltenango and the six intermediate LMTs). These centres are not only the major urban centres in the region, but they are also the major political-administrative centres (department capitals)[23] Thus K=7 is a logical pattern for them, since it divides regions into discrete areas for administrative purposes. Quezaltenango is not officially recognised as a higher or regional centre today, but it houses many higher regional administrative functions (such as a special court of law) and does generally act as a primary regional centre.[24]

Compared with some other traditional agrarian marketing systems, western Guatemala is unusual because of the nearly exact correspondence of the important administrative centres with the important commercial centres. Skinner has suggested that the administrative principle is incompatible with the commercial principle, because the former requires discrete, hierarchical divisions, while the latter requires competing, overlapping divisions (Skinner 1964–65:31). This point cannot be disputed. The implication for western Guatemala is that the major LMTs are not strictly in competition But if they are not in competition, and if they are not located in position to service the area effectively as commercial centres, why do they retain commercial importance in the region. One can point to the fact that the Ladino elite controls: (1) the regional bureaucratic administration, (2) national politics, (3) the legal apparatus, (4) the military, (5) major urban industries, (6) plantations, (7) import–export commerce, and (8) the transport system (most vehicles as well as power over where to build roads). And one can add that they are few in number, closely related through kinship and friendship ties, and xenophobic as regards the special status of Ladinos There is, in fact, little doubt that Ladinos can and do conspire to control the region in some respects.

For general theoretical implications, however, it is more important to eschew a conspiratorial theory and to point out the following The simple presence of concentrated numbers of political-military-administrative bureaucrats in a very few urban centres (the regional administrative centres), who have relatively *very* high purchasing power in the region, will draw commercial activity to them. Particularly when such a group is surrounded by a mass of poor, dispersed, and semi-self-sufficient peasants, who have very low purchasing power, there is little contest in the matter. One might say that under these conditions, peasants would be happy to orient the marketing system towards this purchasing power, even if it would ultimately redound to their poverty. In western Guatemala, political administrators were largely responsible for commercialising the peasantry, by forcing a 'surplus' labour effort that provided subsistence for the administration, so economic dominance of the administrative centres was virtually assured. Equally important, the bureaucrats in the major LMTs were relatively unconcerned with 'efficient' marketing arrangements since local marketing revenue was not their economic power base. Even today rural market exchange, largely carried out by

Indians, affects the economic position of the urban elite only indirectly. Urban exchanges are handled between LMTs in the efficient K=4 system, so lack of articulation in rural trade among the major LMT hinterlands is of little moment to the urban elite.

As to how elements of three different marketing arrangements (K=3, K=4, K=7) arose in western Guatemala, the historical evidence is unclear. But present arrangements suggest the following scenario. The Spanish conquest completely disrupted previous commercial organisation in western Guatemala. The conquerors, unconcerned with local commerce, established an efficient administrative system (K=7) in order to police the region and to extract tribute for the Spanish empire. When this small colony was set economically adrift by independence from Spain, the poor articulation of the administrative solar systems became a serious handicap to both peasant producers and urban non-producers. Economic and ethnic division between these two groups led them to work out separate solutions to the now untenable K=7 marketing system. The urban Ladinos, always involved in urban industries and import-export commerce, expanded the domestic side of long-distance trade. Links were established with previously isolated peripheral communities, these places developed into minor commercial centres (minor LMTs), and the K=4 urban trade system evolved. Quezaltenango became the hub of a now internally oriented trade system. At the same time, the highland Indian population realised both greater economic freedom in the region, and greater importance in the marketing system. Separate centres developed for the distribution of Indian goods, these centres ultimately to grow into the RBC system. Because the RBC system was oriented to rural producers and consumers, and because it developed 'naturally', in the sense of being free from political constraints, its orientation was towards the competitive and rurally efficient K=3 system.

The only problem with this hypothetical sequence is how the relative separation of systems was maintained over time such that articulation between systems and across the major LMT hinterlands remains poor The answer to this lies in the fourth marketing pattern presently found in western Guatemala—the dendritic pattern. As noted earlier, central-place theory predicts that second-level centres (our intermediate LMTs) will be more distant from the primary centre than third-level centres (our minor LMTs). This is an essential feature of the theory, because only this pattern minimises consumer travel and maximises supplier centrality within his hinterland The distribution of LMTs in western Guatemala clearly does not conform to this prediction, but instead to the prediction for dendritic systems. Aspects of the dendritic system are now a major force in the present marketing arrangements of western Guatemala How did this new system evolve, freezing the separate elements of the other three systems into uneasy conjunction? About the time that an integrated domestic marketing system, oriented along either K=3 or K=4 lines, might have developed, a plantation economy was instituted in the region. Plantation trade now dominates the region rather than 'administered' trade, common to K=7 systems and once characteristic of the region. Plantation or import-export trade does not flow to and from different subregional sectors of an economy, it flows from production points directly to a national port or commercial centre, in this case Guatemala City. In other words, goods are *not* produced in the northern periphery to supply people in the southern periphery or elsewhere in the region;

the *same* goods are produced in both areas, collected first in the minor LMTs, then in the major LMTs, flow from there to Guatemala City, and because these goods happen to be coffee, end up on our breakfast table. Again the economy of western Guatemala is oriented to external markets.

Given the present dominance of plantation trade in the region, it is not necessary for the major LMTs in the system to be located so as to supply the different subsectors of the regional economy efficiently. In fact, a dendritic marketing system that links smaller centres to single larger centres in a direct line is probably the most 'efficient' system for this trade. Couple the dynamics of the plantation trade with the fact that the plantation owners and major middlemen (the recipients of plantation trade profits and thus the purchasing power it generates) almost always live in large urban centres, and one sees the basic dynamics of the present marketing system in western Guatemala. The effect of this system is to concentrate economic and political power in the centre of the region or near the primary centre, Quezaltenango. Profits to be gained from a more 'efficient' organisation of domestic trade are simply not comparable, so domestic trade is strictly secondary to the more powerful economic interests, suffering accordingly. Hence the development of a dendritic organisation of the major market centres (LMTs) in western Guatemala.[25] This development leaves the K = 7 system relatively intact, and prevents the kind of articulation between local subsystems one would expect in a true K = 4 system. The RBCs still orient to LMTs in a K = 3 pattern, but the dominance of external over internal trade interests in LMTs means that the K = 3 trade relationship is unidirectional. There is no competition between LMTs because they do not service RBCs The only competitive feature retained is that RBC wholesalers can choose among three LMT retail markets for the best price. This may in fact be common to dendritic systems—it is unclear from Johnson's description how this aspect of provisioning might work. It does seem to have the property of stimulating greater productive efforts from the rural hinterland.

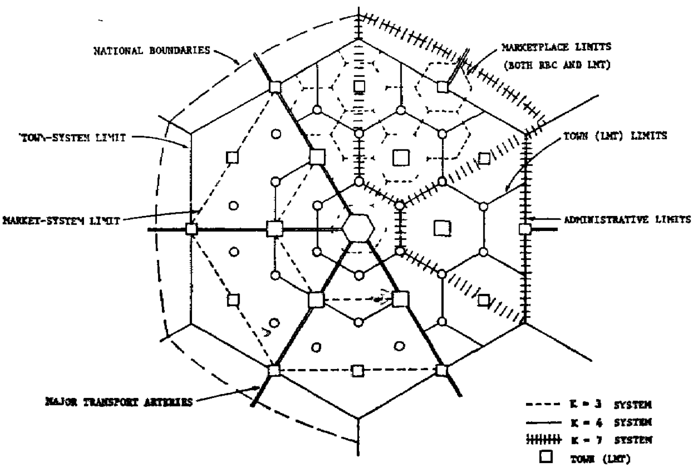


FIGURE 8 Nesting Patterns of LMTs and RBCs with Hypothetical Market-Town Hinterlands.

All the different central-place patterns in western Guatemala are important in the present articulation of trade. Figure 8 indicates how these different systems might nest within others. I have described the system as it is expressed in the western region of the country, but as might be expected the dominance or primacy of Guatemala City and its local system is even more pronounced, leaving western Guatemala a relatively poor region.

VI

Exchange and stratification

The population of western Guatemala is highly stratified, and the organisation and ownership of production systems clearly have something to do with it. But production does not explain how the system operates so that one small class (or ethnic group) controls most of the productive resources, while the rest of the population labours to produce much of the product (in individually rational ways as Tax (1953) has documented), only to control virtually nothing in the economy. The traditional answer for Indian-peasant poverty in Guatemala has been *minifundismo*—i.e., that individual Indian peasants own very small plots of land and correspondingly little in the way of other productive resources. However, Indians as a group own more of the important productive resources (land) than Ladinos—with the important exception of the plantation lowlands—and always have. Yet while land and independent Indian labour are the only significant production resources in the region, landless Ladinos are clearly wealthier than landed Indians.

Another problem with the *minifundismo* argument is that Indian peasant wealth is only poorly indicated by size of landholdings. The wealthier half of Indian peasants owns less land, per capita, than the poorer half. Central location in the marketing economy, e.g., access to major central places, explains the greater wealth and productivity of this group (Location within the region is not only a better predictor of peasant wealth than either size of peasant landholdings or population density, it is the only *significant* predictive variable.[26]) In western Guatemala, access to a strong market is a necessary condition for sound economic alternatives to subsistence farming and labour export. Thus differential wealth among peasants is best explained by their position or location in the exchange economy.

But what of the larger problem of general Ladino control of wealth—the Spanish and their *mestizo* descendants did, after all, acquire the now most productive plantation resources. The point here is that while power is frequently translated into control of productive resources, power is usually exercised by control of the distributive facilities in the exchange system—at least in agrarian societies. Ladinos did not *obtain* economic power through ownership of productive resources, in fact until recently they have not been prominent in productive activities of any kind. (It is probably a truism that status and power rarely come from *productive* activities in agrarian societies.) The economic power of Ladinos in both lowlands and highlands stems from control of the exchange economy—for by controlling exchange, they control the productive resources. How is an exchange system controlled? In Guatemala, it has been done in three basic ways: direct administration of trade, control of market-centre location, and ownership of trade infrastructure (rail-

ways, trucks, storage facilities, and the like) as well as power over aspects of it (roads, taxes, franchises, and so forth).

Trade can be 'administered' by a tribute system—instituted in Guatemala by the Spanish in the early colonial period; and by such means as head taxes and required-labour laws which force peasant involvement in a cash economy—instituted in Guatemala during several periods when peasant goods and labour were needed. Administration of trade is a very effective method for a militarily powerful group; but it has one serious shortcoming—it is costly to administer because of its inherent economic inefficiency and because the administrators must continually struggle against low rates of production (or non-participation). A 'free' marketing system is much less costly in the long run—peasants will compete among themselves to produce more goods, eventually driving down the costs of production; moreover, the peasants require little policing and no one need 'administer' the system. For these very reasons, political administrators are likely to allow a free marketing system to evolve in politically secure regions But by allowing a free marketing system, the administrators do not necessarily relinquish control. The marketing system can be controlled by concentrating the bureaucracy in a few centres and by bureaucratic administration of the means for redistribution. Bureaucratic wealth, obtained by 'legitimate' administrative means, together with heavy market dependence, will result in a marketing system that is naturally oriented towards the concentrated purchasing power of the primary centres. The final product will be either a simple solar system or a dendritic system.

A critical question about the control of 'free' marketing systems is: What prevents competitive market centres from arising that will give the rural producer-consumer a choice in marketing arrangements? Central-place theory predicts that new market centres will arise whenever purchasing power is sufficient to support suppliers in an area small enough that consumers would be willing to travel to a new centre. But purchasing power is unlikely to increase in the distant hinterland of an already established solar or dendritic system, because 'surplus' wealth is continuously drained by the primary centre Production costs in the centre are lower because of economies of scale, so central-place producers can almost always compete favourably with rural producers. As a result, traders from the central place can offer cheap, manufactured goods to the distant peasants at low enough prices for competing industries in the hinterland to lose their 'natural' market. Even in agriculture, commercial ventures near the central place can meet concentrated demand more effectively than can distant peasants. Moreover, excess rural labour is frequently siphoned off at low wages for central-place enterprises (such as the plantations in Guatemala) before it can be utilised for local development. In short, per capita production in and near the efficiently agglomerated centre is so much higher than in the rural areas, that the two are not even at the same level of energy utilisation. It is as if one group had only horsepower, while the other had the internal combustion engine. Under these conditions, the primitive 'hinterland' economy can never become strong enough to compete with the already established 'central-place' economy. In the past the advantages of an administratively based K=7 solar system were probably overcome regularly by its own monopolistic excesses or weaknesses in internal organisation. But today, the natural advantages of externally oriented dendritic systems are increased many fold by industrial

development and by the fact that modern development programmes pour virtually all that modern technology can offer into major centres for industry, neglecting the rural areas. The consequences for rural peasants are frequently disastrous.

How has the spatial organisation of trade in western Guatemala affected stratification there? I suggest that it has shifted over time as follows. During the colonial-administrative phase, when marketing followed the K=7 pattern, position in the economy was determined by position in the polity. Thus there were only two strata in the region: colonial administrators in the major 'solar' centres, and the subject Indians in rural areas. Outside the region was the third stratum, the administrators of the empire. Strictly speaking, the marketing system did not determine these divisions, the political system did. However, because of the administrative base of the economy, other or competing groups with some access to economic power in the region could not and did not develop. In the second, competitive, phase a more complex stratification system evolved Hispanicised Indians (*mestizos* or Ladinos) who lived in urban centres were no longer the retainers of a small political elite, but were competitors for first position in the region with them. At this point custom (being Ladino) became more important than race (being Spanish) in the stratification system An urban Ladino (*mestizo*) merchant would be more prominent than a rural Ladino (Spanish) administrator. Most Indians remained in the rural areas, disadvantaged in the urban redistribution (K=4) system. Indians developed their own rural distribution system, but an Indian hierarchy did not emerge, because their system oriented along rural K=3 lines did not give one group more advantage in trade than any other group. Ethnic dualism remained, fostered by the division of the market economy into rural and urban sectors; but within the two strata there was competition, and between them mobility. Economic rather than political classes began to emerge.

The stratification system today, affected by the dendritic market organisation, is even more complex. Although a significant gap remains between Indians and Ladinos, an economic hierarchy among Indians is becoming as prominent as the economic hierarchy of Ladinos. Indians located near Quezaltenango and the major LMTs make up more than 85 per cent. of the major Indian merchant class in the region. Not only are these Indians considerably more wealthy than Indians located elsewhere in the region (see Smith 1975 for documentation), but they have much higher status in the region. An Indian from the centrally located department of Totonicapan is considered a special and 'higher' type of Indian by most Ladinos and many Indians. While they remain ethnically distinct, these Indians participate in the domestic economy and local politics as much as most Ladinos. Yet Indians located in the periphery of the marketing system remain in economic and political positions today similar to that of all Indians in the colonial period. I see access to marketing opportunity in the dendritic marketing system as the determinant.

Needless to say, market access also affects the Ladino stratification system. The economic position of Ladinos is fairly well predicted from his spatial position in the region: if located in a major LMT, he is better off than someone with a similar office or occupation in a minor LMT or a rural place; if located in Quezaltenango, he is still better off. The chain continues so that the Guatemalan upper class is concentrated in Guatemala City. And, of course, people who are members of the 'external systems' to which the marketing system of Guatemala now orients are the

true elite. In all the phases, status was dependent on economic position, and economic position was at least partially determined by position in the exchange economy. Today, however, position in the exchange economy is more critical to economic position, economic mobility without spatial mobility virtually impossible, and the economic and social position of the region vis-à-vis the rest of the world much more depressed, as a result of a non-competitive market-exchange system which determines economic position by location.

I began this article with the assertion that an examination of marketing (exchange) systems would be as useful for explaining economic inequality as would an examination of production systems. I also stated that regional exchange models would illuminate the nature of certain kinds of stratification as well as the relationship between political and economic systems. I rest my case on the above analysis of stratification and marketing in western Guatemala. Of course, the particular kind of inequality found in Guatemala developed out of the Spanish conquest. The models I have used only explain how this inequality was maintained from that time to the present. I doubt, however, that the exercise of power in 'pristine' agrarian states is significantly different from that in 'conquest' agrarian states. In any agrarian state, control of distribution systems is a most effective way to combine political and economic management For when you control the means of distribution and exchange, it is not necessary to control the means of production.

The property systems that we associate with developed economies evolved with the capitalistic mode of production. These property systems are of great importance for explaining the present basis of economic inequality. But capitalism, contractual law, and private property post-date highly stratified societies by hundreds of years. So if we are to explain the development of inequality, I think that we must begin with a re-examination of exchange relationships and the spatial structures associated with producers and non-producers in pre-capitalistic stratified societies. Moreover, if we are to understand fully the broad international patterns of inequality today, as we move from the political to the economic phase of colonialism (described eloquently by Brookfield 1972, Frank 1967, and Myint 1964) we are also well advised to look at the structure of trade between the metropoles of developed countries and the rural peripheries of the underdeveloped countries.

NOTES

Research on which this study is based was supported by a field research grant from the National Institute of Mental Health Maps and diagrams were prepared by Ronald W. Smith. The Food Research Institute of Stanford University has kindly given me permission to reproduce several of the maps prepared by them for their publication (Smith 1972*b*) I would like to thank the following people for their useful suggestions at several stages of research, without in any way holding them responsible for my interpretations. G. William Skinner, William O. Jones, Benjamin D. Paul, Stuart Plattner, Hugh Gladwin, Harry Basehart, and Ronald W Smith.

[1] Most modifications of Christaller, following Lösch (1954) in the main, are designed for the special circumstances of modern industrial economies with agglomerated urban centres. Successful empirical applications of the Christallerian models have largely dealt with simple agrarian regions, e.g., Skinner 1964–65; Marshall 1969, Hodder & Hassall 1971.

[2] It has been known for some time that a hexagon was the most economical geometric form for the equal division of an area between a number of points Even though this often appears to be the crucial feature of central-place systems, hexagons can be found by manipulating hexagonal nets around any roughly equi-distant points. The crucial feature of central-place theory is that there is a direct *hierarchical* relationship between large and small centres so that small centres nest within the hinterlands of large centres in a regular pattern.

[3] Each different K value is associated with different ratios of numbers of centres in successive orders. K=3, for instance, runs 1:2.6:18.54.162; K=4 runs 1:3 12:48:172; and K=7 runs 1:6:42:294·2,058. (As the K value increases, so does the size of the hexagonal net to encompass more centres.) The formula for computing the number of centres in successive orders for each particular K value is: $N_t = K^t$, where N is the number of dependent places and t the level in the hierarchy (Haggett 1964: 118–22). While many K values are possible, not all are equally efficient or equally likely in the real world.

[4] Skinner (1964–65) has suggested that the K=4 system would also be an optimal arrangement in mountainous areas where village segmentation is slow and road building particularly costly. I would add the caveat that roads seem to become critical only in quite heavily commercialised regions, since peasants might be expected to scramble over hill and dale with their paltry bags of wool in a simple marketing economy—at least they do in mountainous Guatemala

[5] As described by Nash, a solar system is 'a regional marketing system based on economic specialisation [which] moves products among communities in a solar system with a major market . . , a subsidiary market . . ., and other smaller markets which have their special days' (1966· 98). This description does not identify anything special about solar systems except that they are hierarchically organised On the other hand, they *could* be special cases if it turned out that a number of small hierarchical systems in a single region were relatively isolated from and poorly articulated with each other. Since this frequently seems to be the case in the regions where solar systems have been identified, I use the term with that special meaning (see Smith 1974).

[6] My use of the dendritic model (further explicated in Smith 1974), rests heavily on analysis of certain empirical cases, detailed in Mintz (1960), Schwimmer (1973), and Kelley (1973)

[7] I mention the figure of market-dependency ratio, because it compares quite precisely with that given by Skinner (1964–5 34) for traditional China in regions with comparable population density. This would seem to indicate that portions of Guatemala and China in the same density class were similarly commercialised

[8] Population density would seem to be the most important determinant of production intensity and of market development, however, population density in western Guatemala is a poor indicator of purchasing power, the main determinant of market development.

[9] The status of marketing in the pre-conquest period is as yet uncertain Given the developed Quichean civilisation (Carmack 1973), there were probably pre-conquest market centres But I would argue that the disruptions of the Conquest were so profound, socially, demographically, politically, and economically, that the marketing system was completely restructured in the colonial period

[10] A definitive statement on the origin, composition, and distribution of Ladinos in western Guatemala can be found in Adams (1956)

[11] The derivation of the measures used are described in note 14. I used the centrality index of both town and marketplace for the Ladino market towns, and only the centrality index of the marketplace for rural bulking centres

[12] All marketplaces and towns in the intensive study area were observed and censuses were taken seasonally All township-centre marketplaces and towns in the main observation area were observed and censuses were taken at least once on every market day, in this area, hamlet marketplaces were simply inquired about as to size and location, each subsystem was completed within a major agricultural season Only major market towns in the South Coast area (the survey area) were observed and a census taken, except in one sample subsystem (that surrounding Mazatenango) that was completely observed. Twenty highland centres were observed monthly and at fiesta time; and two centres were observed weekly for one year, in order to evaluate seasonal fluctuations A full description of field techniques can be found in Smith 1972*a*. 21–7.

[13] Ladino market towns are distinguished from all other centres by a number of criteria, including marketplace function and size, town-shop development around the marketplace site, population size and composition of centre (all are predominantly Ladino), and distributive functions in the domestic economy

[14] The centrality index used here is of the town service functions, together with marketplace service functions. For each service (or market-place commodity), the amount of centrality contributed to a place is taken to be proportional to the number of establishments (or market sellers) of that service present in the place, with the total number of establishments of that service in the entire region having a combined centrality value of 100. The coefficient is defined as follows: $C = \sum (100\ t/T)$, where T is the total number of establishments of service t in the system. This method, following Davies (1967) and Marshall (1969), takes account of both the number and uniqueness of central-place functions, in the region as a whole.

[15] Using regression and scaling analysis, I was able to establish that the six intermediate LMTs were functionally equivalent in the region (Smith 1972*a*· 162–93).

[16] This was determined by the same techniques described in note 15.

[17] While I believe that the RBCs circumscribe the hinterlands of LMTs, I have not yet completed the analysis of consumer travel data that would allow me to demonstrate it. Moreover, while the RBCs are located between three LMTs, they are not precisely located in the positions that would be obtained from drawing Thiessen polygons around each LMT. Nevertheless, given the rugged terrain of Guatemala, the RBCs are clearly located in regular relationships to LMTs.

[18] It should be noted that most LMTs do not produce domestic items for redistribution to any significant degree. Most goods produced in the region for market distribution (even very specialised goods) are produced in rural areas In consequence, LMTs have relatively little to do with regional redistribution of specialised goods

[19] I have not yet attempted to analyse the spatial patterns of smaller centres. There seem to be different patterns in different subsystems, and by and large more small centres than expected in a regular central-place hierarchy. This is probably a consequence of the dendritic pattern that is the overriding feature of market organisation in the region.

[20] The measure is based on the numbers of sellers of goods. It is probably a fairly good indicator of volume as well, but not of value, since imported goods make up more than half the value of goods in most marketplaces, but less than 10 per cent of the bulk.

[21] There appear to be no significant differences in the minor LMTs serviced by three as opposed to two RBCs.

[22] The primary function of San Francisco el Alto is to supply long-distance traders with specialised Indian consumption goods Long-distance traders (defined as marketplace traders who sell outside their local LMT subsystem) come primarily from the central area, near San Francisco el Alto, and their trade patterns are not strictly determined by central-place alignment.

[23] Only six of the nine department capitals in the study area are major LMTs in our model Totonicapán is an anomalous administrative centre because of its predominantly Indian population; for that and other reasons I class it as an RBC Chimaltenango and Retalhuleu are more closely tied to Guatemala City trade, and play minor roles in the regional marketing system; I class them as minor LMTs.

[24] During the colonial period in Guatemala (1524–1821), Quezaltenango was a regional administrative centre; it has retained a special position in the region, both politically and economically, even though it is now only a department capital like most other LMTs

[25] One could argue that the dendritic pattern is attributable simply to population distribution in the region. Population is concentrated in the central area and tends to thin out at the peripheries. However, central-place theory is not based on uniform distribution of population so much as on uniform distribution of purchasing power. While the two might be coterminous, they need not be As I have mentioned earlier, in western Guatemala the purchasing power generated in the lowland plantation areas is considerably higher than elsewhere. For present purposes, I have assumed that it at least balances the high population density of the central area.

[26] Using several indicators of peasant wealth, I found that location in the region explained more than 90 per cent. of the explained variance in peasant income, while other possibly determining variables, including size of landholdings, explained virtually nothing (Smith, in press).

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HOW WILD ARE THE GOMBE CHIMPANZEES?

Review Article

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The idea that the Gombe chimpanzees are 'wild' is questioned, in view of the fact that for many years these animals have been artificially fed for purposes of study. Their so-called 'predatory' behaviour is examined in relation to artificial feeding techniques causing high levels of excitement, close proximity to baboons and competition with baboons for bananas. Comparison with other studies of chimpanzees in the wild and in captivity indicates that the incidence of killing of baboons is, or was, artificially high at Gombe during the time of Teleki's study. While there remains good evidence of limited killing and eating of other species by chimpanzees at Gombe and nearby areas, the evidence needs to be kept in perspective especially in view of its possible use in arguments about early man.

How wild are the Gombe chimpanzees? Judging from Geza Teleki's book '*The Predatory behavior of wild chimpanzees*', and writing as one who has studied truly wild chimpanzees, I would say that they do not appear to be very wild at all. This view is, I think, in line with the view of them one forms from reading some of the works of van Lawick-Goodall (1963; 1967; 1971), and from the films made at Gombe. All these show us in intimate detail the day-to-day lives of the Gombe chimpanzees but destroy any notion one may have had that one is looking at wholly wild animals. The Gombe chimpanzees have recently been described as 'feral' (Tutin 1974); perhaps 'free-ranging' would be a better word to use.

Fortunately for Teleki a recent paper by Wrangham (1974*a*) does put the issue of predatory behaviour into the perspective of feeding behaviour as a whole. The year described by Teleki in this book, 1968–69, seems to have been the year when tension between the chimpanzees and the baboons at Gombe was at or just past its height, owing to a particular form of banana feeding during 1967–68 which brought large numbers of each species to the Goodall camp-site in a highly competitive situation. I have observed a comparable situation, where fruit was given to a group of thirty or more chimpanzees at a couple of dozen food hoppers in a large building at one end of a 30-acre enclosure at the Holloman Air Force Base, New Mexico. Feeding took place early in the mornings and again in the afternoons. As feeding time approached, the number of aggressive interactions rose among this group of otherwise peaceful and friendly animals. Dominant males were especially liable to attack and bite others at this time. During feeding, aggression was less frequent and quite subtle food-sharing interactions between certain males and females, and between adults and youngsters, occurred. After feeding, the animals drifted off in groups or settled down to groom each other and calm returned to the colony.

It is clear from Teleki's detailed account that the attacks of chimpanzees at Gombe on other species, especially baboons, did not always, or even often, occur

Man (N.S.) 10, 123–125.

actually at the feeding-place or at feeding time. To this extent one cannot *simply* conclude that chimpanzee 'predation' at Gombe is a direct outcome of the competition and hostility arising from the artificial and highly unnatural situation produced by feeding. Yet Teleki clearly describes, on pp. 62–63, an attack by the adult male chimpanzee Mike on the young female baboon Amber as an outcome of, or accompaniment to, feeding tension.

We can be sure that the cases of predation on certain species other than baboons —red colobus monkey, bush pig and bush buck—were not related to the increased boldness and interspecific aggressiveness engendered by the Gombe feeding situations because they occurred before artificial feeding started. But in at least two earlier studies of truly wild, unprovisioned chimpanzees, that of Nissen (1931) and that of Reynolds and Reynolds (1965), there was no evidence at all of predation by chimpanzees. Nissen's study was made in woodland terrain in Guinea, Reynolds and Reynolds' in rain-forest in Uganda.

Kortlandt (1962; 1967) and his co-workers have made extensive studies of chimpanzees visiting food-rich plantation sites and provisioned areas in west and east Africa. They have been concerned to document chimpanzee reactions *to* a predator (stuffed leopard) but have reported no predation *by* chimpanzees. We should note that where this research team has used provisioning to tempt the chimpanzees into camera range for the leopard experiment, conditions differ from those at Gombe in two important respects. First, the provisioning was for short periods only. Second, no other non-chimpanzee species were present to compete for the food. This is clearly a very different situation from the one at Gombe. Albrecht and Dunnet (1971: 101) report no predation by chimpanzees on baboons despite considerable social tension between the two species.

Predation by chimpanzees is thus not a common or frequent event. At Gombe it takes an average of 200 observation hours on males to see predation (Wrangham 1974*b*), and elsewhere, e.g. in rain-forest, it may not be seen, even if it occurs.

Predatory behaviour has, however, been reported at a site to the south of the Gombe National Park by Kawabe (1966). Kawabe's account of the chasing, killing and carrying away of a redtail monkey by a group of six chimpanzees is thoroughly convincing. There is no mention of feeding, and Kawabe stresses that this incident was an isolated case. Further data on chimpanzee predation are given by Nishida (1974), who reports on seven predatory episodes, involving killing, eating, and carrying away prey species of suni, squirrel, mongoose, bushbaby, redtail and vervet monkey.

Sugiyama (1968) during a six-month study in the same area of the Budongo Forest as that in which Reynolds and Reynolds worked for eight months in 1962 again found no evidence of meat-eating (p. 228). He did, however, include in his report a personal communication from Suzuki to the effect that the latter had seen chimpanzees eat a blue monkey and a black-and-white colobus monkey in Budongo, and the data were later published by Suzuki (1971). Suzuki describes how he saw chimpanzees eating the flesh of (a) a baby chimpanzee, (b) a young blue monkey and (c) a young black and white colobus monkey. In the latter case, although the kill itself was not seen, there was evidence of a chase and a catch. He also describes in some detail the process of meat-sharing among the individuals involved in a catch. Teleki likewise gives data on meat-sharing.

Teleki himself argues that the Gombe predatory behaviour cannot be ascribed to provisioning because (a) it occurred there prior to the onset of regular feeding and (b) because of the reports of Kawabe and Suzuki already discussed. However, both Wrangham (1974*a*) and I feel that Teleki has seriously under-rated the impact of the Gombe feeding technique, especially during the year 1967–68, on chimpanzee-*baboon* relations and thus on chimpanzee-*baboon* predation.

From all this it follows that great caution needs to be exercised in drawing inferences from the meat-eating habits of the Gombe chimpanzees to those of our hominid ancestors. Such a speculative 'scoop' is not missed by Teleki, nor by Carpenter in his Foreword to the book. Indeed, everything the Gombe chimpanzees do—their 'tool-making' and their use of tools for termite fishing, their use of leaves as 'sponges' or as 'toilet paper'—tends to be seized on rather uncritically by those eager to prove something (what?) about early man.

Teleki's own treatment of the implications of the Gombe data for man is disappointing. He appears to be unaware of the excellent comparative study of predatory behaviour by Schaller and Lowther (1969) though it is highly relevant to the theme of his discussion (pp. 175–77). Nor are there any references in the text to other work on human/hominid origins, though the bibliography is quite extensive. In a book one does expect a fuller discussion than is presented here, and perhaps we can expect one at some future time?

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CORRESPONDENCE

Samoan tattoos and structural analysis

SIR,

G. B. Milner's comments in 'Siamese twins and Samoan tattoos' (*Man* (N.S.) 8, 108) deserve careful response for they illustrate, as Milner notes, problems of a broader nature than the structural analysis of Samoan tattoos. The following comments will be directed at problem areas addressed in an earlier letter (*Man* (N.S.) 7, 314–15), and they may help clarify certain points.

In his original analysis of Samoan tattoos (*Man* (N.S.) 4, 5–23), Milner contended that only Samoan males were tattooed and proceeded with a stuctural analysis seeking the female equivalent of male tattooing. Having found this equivalent to be childbirth, he concluded that the function of tattooing was to 'restore the balance of the sexes' (1969: 21). In my comments, it was noted that Samoan women were also tattooed and that this fact was of some relevance to Milner's conclusion. In his reply, Milner maintained that female tattoos were not central to his argument. Conceding that he 'should have mentioned' them, Milner reasoned that while Samoan women were 'treated in a manner which in English is called tattooing, within the *conceptual framework of Samoan language and culture, Samoan women are not tattooed*' (1973: 108). Therefore, female tattoos were not 'real' tattoos and for me to consider them as such was, according to Milner, 'ethnocentric and semantically unacceptable' (1973: 108).

By implying that his structural analysis follows logically from the Samoan conception of tattooing, Milner attempts to define away, for purposes of structural analysis, the relevance of female tattoos and, at the same time, to lend credibility to his conclusion. By focusing on the issue of 'interpretation by translation,' the real issue—interpretation by structural analysis—is obscured. I shall attempt to demonstrate that Milner's conclusion about the function of tattooing follows not so much from Samoan conceptions of tattooing as from the structuralist point of view and that, as new data and perspectives are considered, this point of view seems less logical and more arbitrary.

Although Milner regards the issue as largely one of semantics, i.e., that an understanding of the differences between male and female tattoo designs (*tatau* and *malu*) would have obviated much of our disagreement, these differences were already clearly noted in my earlier letter. In fact, Milner and I agree for the most part on the translation and use of these words. What we disagree on is their functional significance in the interpretation of tattooing. Milner seems to believe that the differences between *tatau* and *malu* justify his conclusion about the function of tattooing; he states that the function of male tattooing is 'to restore the balance of the sexes' and implies that, because of conceptual differences between male and female tattooing, female tattoos could not perform this function. However, to make this argument convincing, it would have to be shown that 1) male tattoos do indeed perform the function of restoring the balance between the sexes and that 2) female tattoos could not perform this function in spite of conceptual differences. It is precisely because Milner couches his conclusion in vague functional terms that female tattoos become relevant.

It is important to note that Milner's functional conclusion does not derive from an examination of the multiple functions that tattooing may perform either at the manifest or latent levels. Nor does it derive from Samoan conceptions of tattooing. Rather it seems to stem directly from the assumptions of Milner's structural analysis: the assumption of complete symmetry and opposition, and the use of bipolar oppositions such as male: female. If one makes such assumptions, it is hardly surprising that the object of analysis would have the function of restoring 'the balance of the sexes.' Yet to make such an argument convincing one would have to demonstrate why, *apart from structural assumptions*, male tattoos should fulfil this function and, conversely, why female tattoos should not. How do we know that a cultural need to restore the balance of the sexes exists in Samoa? Or anywhere else for that matter? Why is it that this need should be expressed only in the institution of male tattooing? And why should it find resolution only in the cognitive sphere? Milner provides no substantive answers to these questions nor does he provide evidence that male tattooing does indeed serve this purpose. Given the lack of evidence supporting Milner's conclusion, it could conceivably be argued either that male tattoos do *not* serve to restore the balance of the sexes *or* that other phenomena, like female tattooing, perform this function. At this point, the matter is purely speculative.

In the presence of this functionalist dilemma and in the absence of empirical data, Milner's

conclusion is defended in terms of 'logical and symmetrical structures' that 'make sense.' Yet it is not defended in terms of the Samoan conception of tattooing despite references to 'the framework of Samoan language and culture.' Milner admits that he does not know what the Samoan reaction to his analysis might be (1961: 21). Nor does this seem terribly important; in his words, 'what matters is not that we have a grip on structure, but that structure has a grip on us' (1969: 21–2). Although the point is worth pursuing, it is difficult to understand how Milner moves from an unconvincing conclusion about the function of Samoan tattoos to such a generalisation except by assuming that structure is ineluctable. Here Milner acknowledges that his interpretation owes 'a very great deal' to the approach of Lévi-Strauss (1969 17) and that it is 'conditioned by [Milner's] cultural background and intellectual training' (1969: 21) (although he finds my view 'ethnocentric'), but beyond this, there is very little to persuade us that it is more than a self-fulfilling prophecy.

Another means of discerning the strengths and weaknesses of this structural approach is to consider an alternative point of view that tries to answer the kind of questions that Milner raises about Samoan tattooing The list of 'enigmas' in Samoan tattooing that Milner resolves through 'logical and symmetrical structures' seems equally amenable to a more common-sense approach. For example, Milner asks:

> Having gone to so much trouble and suffering to get himself tattooed, why should a Samoan male be careful to drape his kilt around his thighs when he is sitting cross-legged (as he does normally)? For the effect of this is that hardly any portion of his tattoo is visible. Is it the tattoo itself which it is not proper to expose, that is to say, or is it the part of the body which is tattooed which is indecent to expose, and if so why should the two be co-extensive? (1969. 17).

According to the structural analysis, this may have something to do with the oppositions:

> A woman is secluded at child-birth and covers her pregnancy.
> A man is secluded at tattooing and covers his tattoo (1969: 20).

But does this really explain why men cover their tattoos?

If we view Samoan tattooing historically, we find that prior to European contact the usual dress of male Samoans was 'a mere apron of leaves, tied around the middle of the body, which covers only in front' (Hale 1846: 39); the net effect of a man's tattoo was 'to give the person the appearance of being dressed in short, dark-blue trousers' (Hale 1846: 39). In other words, the pre-European Samoan conception of modesty did not require covering tattoos. However, with the coming of Christian missionaries, the Samoan code of conduct of dress underwent far-reaching changes, including the attempted abolition of tattooing. J. W. Davidson describes the prevailing Christian concerns:

> The reformulation of conduct was seen, primarily, as the changing of attitudes towards sex and the exercise of authority. Samoan acceptance of polygamy, of extramarital intercourse, and of easy divorce was inevitably regarded with horror, as were the performance of 'lewd' songs and dances and the public testing of virginity at marriage. But the missionaries sensed incompatibility with Christianity, as they understood it, in much else besides: in the practice of tattooing, in the wearing of long hair by the men and short hair by the women, in the scantiness of Samoan dress, and in the lack of privacy provided by Samoan houses. In all these matters, and in more besides, they strove to impose their own standards (1967 35).

The present dress of Samoan men is thus largely a product of accommodation to missionary tastes and can be explained without reference to Samoan women covering themselves during seclusion at pregnancy.

The same kind of answer can be provided for Milner's query as to whether or not it is proper to expose that part of the body which is tattooed In pre-European times, it was certainly proper to expose tattoos, the mode of dress virtually dictated it. With missionary denunciation and new dress codes, Samoan men (tattooed or not) donned kilts or lavalavas which are almost co-extensive with the tattoos. However, it is still very easy to discern who is tattooed and who is not, as well as the degree of tattooing expertise. Men with tattoos are very proud of them and often sit in positions that allow their tattoos maximum exposure, given the constraints of modesty. As Buck pointed out

> ... in sitting cross-legged in the kava circle and meetings with the kilts tucked up to free the knees, the lower anterior and inner surfaces of the thighs ... were well exposed. Good tattooing thus had an excellent chance of being displayed and admired (1935: 652).

These brief sketches provide answers to Milner's questions without resort to bipolar oppositions or assumptions about complete symmetry and opposition. Yalman, in his

discussion of Lévi-Strauss' work, has doubts about the validity of such an approach:

> No recourse is had to historical explanations or extraneous accidents: for, it is noted, if the analyst were to open the door to arbitrary explanations then all would be lost. How would one then choose between a logical explanation in one case and an arbitrary explanation in the next (1967 81).

Yet it was Lévi-Strauss himself who stated that the historical approach 'should not be excluded from the field of structural studies' (1963: 290). I would suggest that it is not a matter of choosing between 'logical' and 'arbitrary' explanations but rather one of trying to shroud structural explanations in the guise of logic. Is it logical to exclude data that might indicate that certain oppositions are more a product of the analyst's perspective than they are inherent in the data? Is it logical to exclude potentially relevant data in a functional interpretation of tattooing? And is *Milner's research dictum—to discover* 'by structural analysis, what kind of equivalence, whether conscious or unconscious, is suggested by the tattooing of Samoan males' (1973 108) —less arbitrary than the historical perspective used here?

I have attempted to demonstrate that Milner's conclusions do not follow so much from Samoan semantics as they do from the structuralist point of view and that, as different kinds of data and alternative approaches are considered, this point of view seems less logical and more arbitrary. In retrospect, Milner seems to have found as many hitherto unappreciated virtues in his analysis as I have found flaws in my critique of that analysis. In the event that this letter does not aid in clarifying our respective positions, its publication will at least make Milner and me doubly opposed. Perhaps this is what he means when he says that 'structure has a grip on us.'

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Alliance or descent: some Amazonian contrasts

SIR,

We would like to bring to the attention of your readers certain information regarding both patrilineality among the Yanoama tribe and the professional background to Shapiro's recent letter about these tropical forest Indians of Venezuela and Brazil (*Man* (N.S.) 9, 305–6) As anthropologists who have also worked among the Indians of the Yanoama tribe and as academic acquaintances of Shapiro's we are especially taken aback by two aspects of her letter.

To our knowledge, as of April 1974, Shapiro has not returned to the Yanoama (tribe) for any continuation of her initial fieldwork with these Indians, completed in 1968. In her Ph.D. thesis, in which she severely criticises Chagnon's and Lizot's accounts of patrilineality among the Yanomamo (linguistic sub-group), while apparently not totally rejecting the possibility of such patrilineality, Shapiro presents not one word of evidence for its presence (or absence) among the Indians of the Yanomam (linguistic sub-group) with whom she did her own abbreviated fieldwork. We find it strange, then, that in her letter to *Man* she goes so far as to say, 'However, it is evident that agnation figures as a principle of some importance in Yanomamo ideology and in some areas of social practice as well' (1974 305)

The second aspect of Shapiro's letter which puzzles us is her complete lack of any reference to the possibility of regional variation within the overall Yanoama life-style. This is particularly surprising since in her thesis she devotes considerable attention to the matter, discussing several examples of such variation (1972: 84–5, 98–105, 169–84), and refers to Chagnon's earlier discussion of this question of regional variation (Chagnon 1966: 192–208). Our own data from the upper Auaris river valley Sanumá show a number of contrasts with material from other regions of the Yanoama territory (see Ramos 1972: 191–4 and Taylor 1973*a*: 44).

As long ago as 1967, Migliazza had published a partial (partial because referring only to the Indians of Roraima Territory, Brazil) analysis of language and dialect sub-divisions of the Yanoama (tribe), and Shapiro cited this paper in her thesis (1972). In his Ph.D. thesis (1972) Migliazza presented a revised and complete version of this analysis. According

to his analysis (and we, like Shapiro, are not linguists and do not presume to judge its validity) there are four main linguistic sub-groups of the Yanoama tribe. These are: the Yanam (where Migliazza himself had worked—see Migliazza 1964); the Yanomamö (where Chagnon and Lizot have worked); the Yanomam (where Shapiro worked); and the Sanumá (where we have worked).

Specifically as regards patrilineality: Migliazza (1964) does not report its presence among the Uraricaá river Yanam; Shapiro (1972), as mentioned above, does not report its presence among the Catrimani river or Parima river Yanomam; Chagnon (1968 and in his more recent book, 1974) and Lizot (1971) report its presence among the Mavaca river and Orinoco river Yanomamö, though with regard to an empirical situation (in *that* region) which does not seem to permit of any very satisfying evidence; we find patrilineality to be unmistakably present among the Auaris river Sanumá (Ramos 1972; 1973; 1974; Taylor 1972, 1973*b*; 1974).

In terms of Needham's recent discussion of the concept of descent (1971: 8–13), we would characterise the upper Auaris river valley Sanumá as follows.

name transmission	patrilineal
residence	matrilineal (uxorilocal)
group membership	patrilineal
leadership succession	patrilineal

The groups in question, whose membership is determined patrilineally, do not, however, engage in joint action of any kind, nor are they corporate (on this last point Ramos is indebted to Dr Peter G. Rivière for helpful comment on her thesis (personal communication, 1972) and takes this opportunity to reaffirm her agreement with him that corporateness is not, in fact, a characteristic of these groups).

What needs to be remarked, in view of Shapiro's needlessly limited representation of Yanoama reality (we assume that she did not intend to speak only for the Yanomamö linguistic sub-group, given that her own fieldwork was with the Yanomam sub-group), is that all three features which Kaplan mentions (1973: 569) as lacking for these Yanomamö are present in the case of the Sanumá. That is to say, the Sanumá we have studied have *named* patrilineal groups and categories, with local descent groups of typically *three* generations (see especially Ramos 1972; 1973), and these have a number of interesting *ideological elaborations* (see especially Taylor 1973*b*; Ramos 1974).

Our relevant material on the Sanumá has not been unavailable to Shapiro. She received copies of our Ph.D. theses, for example, within only days of these being completed. On 25 June 1972 she wrote Ramos a fairly detailed comment on Ramos's thesis.

We regret very much the necessity for this comment on Shapiro's letter, but feel that the facts speak for themselves and, in the interests of responsible anthropological study of the Yanoama (tribe), hope that they may be of some interest to your readers.

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Alcida R. Ramos

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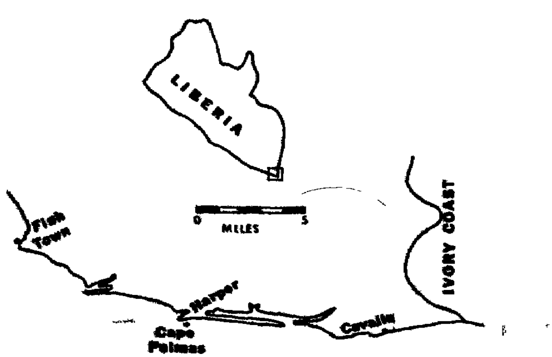
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A modern ossuary in southeastern Liberia

SIR,

During August, 1973, the two of us visited Cape Palmas on the southeast coast of Liberia in the course of conducting an archaeological survey of the Republic. It has been well known for some time that Grebo and Kru tribal peoples in this region followed the practice of burying their dead on offshore islands in the Atlantic. The most prominent of these burial places is Russwurm Island ('Dead Island') at Harper, the administrative capital of Maryland County and for a brief period in the early nineteenth century the centre of an independent Americo-Liberian colony. The island is now connected to the mainland by a short causeway, and a modest port facility has been developed in conjunction with logging operations a number of miles inland. During the erection of this complex, a great many burials were uncovered and proved to include large quantities of old European trade goods. As construction on the site proceeded, most of these items were removed by local residents and foreign visitors, two of whom undertook some salvage archaeology in 1968 (Delafield n.d.). They recovered more than 600 copper and brass ornaments along with clay pipes, trade beads, and other imported objects which appear to reflect a fairly lengthy and sustained period of outside contact. All the burials are said to have been situated on the leeward side of the island, placed in shallow soil atop the rock base. While our own later investigations led us to conclude that virtually the entire cemetery has now been destroyed, we did find one apparently intact segment of shell midden in this area. This was no more than about 50 cm deep and contained only large bivalves and a few potsherds. A shell sample removed for radiocarbon analysis subsequently yielded a date (GX-3307) of 225 ± 170 years (A.D. 1725) which, while of no great value in terms of accuracy, would seem to be in keeping with the presumed eighteenth-to-nineteenth-century origin of most of the imported artefacts previously discovered.

Our attention also was directed to a cemetery at Cavalla, a few miles to the east (fig.1) and very close to the Ivory Coast border. Cavalla is an old and prominent Grebo town, whose inhabitants still use a burial ground located in a palm grove along the ocean front. As a consequence of grave-digging and the uprooting of large trees during tropical storms, bones and grave goods derived from earlier interments continually find their way to the surface. As at Russwurm Island, European trade goods are much in evidence, including clay pipes, crockery, glass beads, and brass and copper ornaments. With the possible exception of some iron-work, the metal artefacts almost certainly were imported, either as finished pieces or as bars, tubing, or wire ready to be fashioned into ornaments. Presumably these were made to order for traders working the West



African coast. Some of the bracelets and anklets are made of hollow tubing and even display machine-stamped seams, but others are solid casts with circular, oval or hexagonal cross-sections, and a few of these have raised designs. Also found were two bell anklets, one of iron and one of brass with iron clappers. The crockery on the whole again suggests an eighteenth-to-nineteenth-century date, although earlier imports might be expected to appear here or in the immediate vicinity in view of the fact that Europeans have been visiting Cape Palmas since the fifteenth and sixteenth centuries.

A few miles west of Harper, and closer to the Kru coast, is another old Grebo settlement known as Fish Town. Its inhabitants, like the earlier indigenous residents of the Harper area, also dispose of their dead on a small offshore island. However, the mode of disposal in this instance is rather different. Instead of being interred—which would be quite difficult on this rocky islet—coffins containing the bodies are taken out by boat and simply stacked up in the brush, new ones lying on top of older ones in various states of disintegration, all exposed to the elements. The island is littered with skulls and post-cranial bones, many in good condition, as well as rotting bits of cloth, brass bracelets, and eating or cooking utensils discarded after funeral feasts. There is no indication that, once deposited, the coffins or their contents are of much further interest to the living or that the island itself is particularly venerated. The town elders are adamant that the island has served this function for as long as anyone can remember (which in Liberia does not necessarily indicate great antiquity) but that bodies used to be wrapped in mats and left ashore in the dugout canoes employed to transport them.

Christian Grebo are buried in an ordinary cemetery in the centre of town, but attached to trees in this burial ground are headstones or plaques commemorating those non-Christians whose remains lie in the island repository. The elders at Cavalla state that their 'big men' also were taken to the Fish Town island at death until a few years ago when a funeral canoe was swamped by heavy seas and it occupants lost (it is not the case, however, that only the Fish Town elite are accorded a final resting place on the island since many of the still-recognisable caskets are those of children). It is not surprising that both communities used the island, for each of them belongs to the Klemonweh chiefdom and is traditionally regarded as one of the 'seven original Grebo towns' (Schulze 1973: 48).

The Fish Town repository and its skeletal remains, aside from constituting an ethnographic curiosity, may be a research source of some significance. For example, one might carry out a genetic study based on dental and osteological characteristics, especially if combined with judicious use of town records, genealogies, and oral traditions. How far back in time the island may have served its peculiar function is questionable, although it must span a number of generations. Certainly in a general sense these mortuary practices of the Grebo and some of their Kru neighbours have the potential of revealing many details on both tribal movements and patterns of European trade during the past several centuries

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Robert Borden

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Radcliffe-Brown's contribution to kinship studies

Sir,

Page lxxxiv of Rodney Needham's 'Introduction' to *Rethinking kinship and marriage*, 1971, contains the following passage:

> Beattie, claiming Radcliffe-Brown as one of the 'founding fathers of social science', says that [in the study of kinship] he 'introduced clarity and order into what had hitherto been something of a jumble of ethnographic *minutiae*' (1969: 185) [the reference is to 'A. R. Radcliffe-Brown (1881–1955)', in *The Founding Fathers of Social Science*, ed. T. Raison, Harmondsworth]. This judgement seems actually to be lifted from Lévi-Strauss, who some years earlier had written that Radcliffe-Brown 'introduced some kind of order where there was only chaos' (1963: 303) [the reference is to the English version of *Structural anthropology*]. . . . Now in fact Lévi-Strauss was not talking about kinship in general but specifically about the study of Australian systems, and in this context the tribute is definitely undeserved.

And so on.

I am reluctant, Sir, to take up your space on a matter of such minor importance, but after some reflection, and bearing in mind that the relevant meaning of the verb 'to lift' is 'to take up . . . with dishonest intentions; hence, to steal' (SOED), may I please make three comments?

(1) The date Needham ascribes to my article on Radcliffe-Brown, 1969, is not the

date of its first publication. It originally appeared in *New Society* in 1964.

(2) In the sentence he quotes from it, he omits what might be regarded as the rather important opening qualification 'for many students....'

(3) My judgement of Radcliffe-Brown's contribution was based entirely on my own and others' experience as students. It was not 'lifted' from Lévi-Strauss or from anyone else. Taken in context, I can see no reason why it should be thought to be so.

Needham's judgement of the value of Radcliffe-Brown's contribution to 'kinship' studies will no doubt be accorded the respect it merits. But when a senior anthropologist unambiguously implies that a colleague is guilty not only of plagiarism but also of having misunderstood or misrepresented the plain meaning of the matter plagiarised, then it seems to me that unless guilt is to be tacitly admitted, a mild protest needs to be made.

J. H. M. Beattie

University of Leiden

SIR,

It is distressing to find that I have inadvertently given offence to an old friend and former colleague, and on that account I readily offer Professor Beattie my apology. But I suppose he will look also for some response to the particulars that he adduces, and on that score too I must do what I can to mollify him.

It is now over four years since I wrote the lines complained of, and three since they were published, so I cannot answer to what precisely was in my mind at the time other than to say that I surely was not accusing Professor Beattie of any form of theft. It did strike me, though, that there was a significant similarity between Lévi-Strauss's assertion that Radcliffe-Brown had 'introduced some kind of order where there was only chaos' and Professor Beattie's subsequent wording that he had 'introduced clarity and order into... a jumble....' I think I assumed (as I should do now, if it were not for Professor Beattie's disclaimer) that Professor Beattie had lifted this judgement, in the sense that he had taken up the phrase at issue, from Lévi-Strauss. If Professor Beattie is maintaining, however, that in expressing his own judgement he was not influenced by that of Lévi-Strauss, then there is no more to be said.

I am not sure what point Professor Beattie is making when he comments that his article on Radcliffe-Brown was first published in 1964. Lévi-Strauss's claim on Radcliffe-Brown's behalf was first printed in 1953 (Lévi-Strauss 1953: 541), in the identical words that I quoted, and it was published in French as 'introduisant un ordre là où il n'y avait que chaos' in 1958 (Lévi-Strauss 1958: 334).

On what seems to me the real point of substance, in the history of ideas, if Professor Beattie's concluding sentence means that in his own independent judgement Radcliffe-Brown really did 'introduce... order' into the study of kinship, or into the study of Australian systems, then I am afraid I can only repeat (cf. Needham 1971: lxxxiv–v) that he is mistaken.

Rodney Needham

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British Museum ethnographic collections

SIR,

Perhaps you will allow me to use your journal to remind all students of material culture that the ethnographic collections of the British Museum are now open to *bona fide* research workers. These collections contain over 800,000 objects from most areas of the world, and access to the new store in which they are housed can be obtained by applying to me at the address below.

I would also remind readers that the Museum of Mankind has an extremely good anthropological and archaeological library which can be consulted by members of the public.

I will be pleased to provide any further details your readers may need.

M. D. McLeod

Museum of Mankind, London

'Taung, Botswana': the genesis of an error

SIR,

In the jubilee year of the discovery of the holotype of *Australopithecus africanus*, it seems desirable to correct an oft-repeated error concerning the location of its place of origin. For example, such authoritative works as Michael Day's *Guide to fossil man* (1969: 162), Clark Howell's *Early man* (1969: 39) and David Pilbeam's *The evolution of man* (1970: 7), all place Taung in Botswana.

Taung is not in Botswana, but in the northern part of Cape Province in the

Republic of South Africa: 27 32'S, 24 45'E (Oakley & Campbell 1967: 99).

It is easy to see how the mistake arose. In his original publication of the fossil Professor R. A. Dart (1925) wrote: 'I learned that this valuable fossil had been blasted out of the limestone cliff formation ... at Taungs, which lies 80 miles north of Kimberley on the main line to Rhodesia, in Bechuanaland.'

Dart was using the name 'Bechuanaland' to indicate that area of the Union of South Africa predominantly inhabited by Tswana, by which name it was known and which is the name inscribed on maps of the time for that part of the Union. Dart was not using the name to indicate Bechuanaland Protectorate, but this is the meaning given to it in a large number of text books and this has been translated into 'Botswana' following the independence of the country in 1966.

Thurstan Shaw

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Physical Anthropology

BERNHARD, WOLFRAM, & ANNELIESE KANDLER (eds). *Biology of human populations: contributions to their structure and dynamics*. xxiv, 730 pp., illus., tables, bibliogrs. Stuttgart: Gustav Fischer, 1974

This imposing book is a *Festschrift* for Ilse Schwidetsky, on her 65th birthday. It contains 46 papers by 53 contributors, mainly from Central and Eastern Europe.

There are three major sections, on the structure and dynamics of living populations and of prehistoric and historic populations, and on the evolution of man. Each section is further subdivided. The first has subsections on the role of genetic factors in population structure, the anthropological structure of ethnic groups and the effects of social processes, the second on palaeodemography and palaeopathology, skeletal anthropology and evolutionary trends in prehistoric and historic populations, and the third on biological evolution, social evolution and the future of man.

As might be expected the approach of the different authors varies considerably Some merely report the present stage of their research programme, but an appreciable number of papers are up-to-date reviews of work in some of the main fields of human population biology

Perhaps inevitably, the book leans fairly heavily towards a European concept of human biology as being concerned to an extent that most British and many American readers will regard as excessive, with the use of mensural characters in the establishment of taxonomic interrelationships between extant and/or extinct human populations. This is perhaps one field of biological research where the advent of computers has enabled an almost moribund horse to get up and gallop away! Fortunately in the face of some 15 papers of this type, the elegant scepticism of Count comes as a welcome rejoinder.

For me the best subsections are those concerned with the effects of genetic and social factors on population structure. The papers on the genetical theme by Chopra, Wolanski and Nemeskéri are excellent, as is that by Walter on environmental adaptation. However, Fleischhacker's contribution on 'bastard populations' suggests that in some quarters the concept of the 'pure race' still has some significance.

The papers on the effects of social processes on population structure include two which are excellent, by Olivier on inter-group differences among Frenchmen and by Cliquet on the results of national fertility surveys in Belgium. In particular this latter paper is a mine of information.

Generally the book is well produced with few typographical errors. However a major fault is the awful standard of English in the summaries and in some of the papers written in English There is no excuse in a book of this size and cost for completely inadequate translations from the original languages, or for inadequate stylistic refereeing of papers written in English.

To sum up, much of the appeal of this book will be to those interested in inter-population differences and similarities in polygenic characters, especially cranial and post-cranial dimensions. To the extent that contributions of this type constitute almost a third of the book, it gives a somewhat uneven view of modern trends in human population biology. However the inclusion of at least a few papers on population dynamics and structure—which after all are what the book is supposed to be about—as well as on human evolution does redress the balance to some extent and the book must be given a cautious, but nonetheless sincere, welcome

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SCHWIDETSKY, ILSE. *Grundlagen der Rassensystematik*. 180 pp , figs., maps, tables, bibliography Zürich. Bibliographisches Institut AG, 1974

This is an interesting book on a topic which seems largely out of favour among English-speaking human biologists Perhaps the reason for its neglect lies in the depressing necessity of wading through masses of largely unscientific nonsense which have been and continue to be written about race, in order to get at the much smaller amount of genuinely interesting and informative material. For those put off for this reason this book provides a succinct historical account of the development of 'Rassenkunde' and a critical review of the present state of the art.

The book is divided into four parts; an introduction, in which the concept of race as an aspect of systematics is discussed briefly; an account of methods of racial classification; the results of the application of such methods,

described geographically; and a final section (which might have been placed nearer the beginning) on some of the problems of racial classification. The absence of an index is an unfortunate omission.

My initial reaction was that the approach was largely uncritical, but this was dispelled in the last section, which is easily the best part of the book. Dr Schwidetsky rejects typological methods of classification (which occupy so much of the second and third parts) in favor of a population genetical approach. She emphasises the usefulness of multivariate analysis, but points out the dangers of uncritical use of this powerful analytical tool.

Despite all these good things, one cannot help feeling that much of the labour is in vain. Establishing a taxonomy of man—almost always on grounds which are in the widest sense morphological rather than functional—takes him out of his relationship with his external environment. Hence we regard his peculiar and varied attributes as interesting because of their discriminative efficiency, not as the result of evolutionary changes which have fitted him for the many environments he now inhabits. Granted, the one approach does not necessarily exclude the other, but the ecological attitude seems so much more likely to give us important insights into man's nature that it seems a pity to spend time on other less rewarding and essentially artificial aspects of his biology.

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MONTAGU, A. *Man's most dangerous myth: the fallacy of race.* xviii, 542 pp, tables, bibliogr New York: Oxford Univ. Press, 1974

This work is aptly termed 'a splendid piece of liberal polemic' in one of the reviews cited on the dust jacket. It is to the author's credit that this bias is clear from the very title and throughout the book His stated purpose (p. 425) is to 'clarify the reader's thinking upon the much-vexed and usually tendentiously discussed problem of "race", to induce the reader to rethink his basic convictions regarding human relations, and to present the whole problem in such a way as to encourage him to draw his own conclusions concerning the kind of solution or solutions that would be most effective in solving the "race" problem.' The clarification, inducement, and encouragement processes are all obviously directed.

The exposition of the nature of the 'race' problem begins with a historical review of thought on human geographical variation. After describing the inability of typological race concepts adequately to reflect human variation, the author presents his well-known contention that a concept of ethnic groups, temporarily reflecting varying degrees of dispersal and isolation of ancestral populations, better reflects the true nature of human variation. The biological unity of mankind is emphasised next, then the probability that natural selection has favoured educability and plasticity of adaptation in all human groups. 'Race' is presented as linguistic and sociological; races, socially defined as castes; and racial prejudice as the outlet for economic and psychological frustration. The author argues (the argument is elaborated in an appendix) that the term *race* should be excluded from discussion of human variation as hopelessly nonspecific and emotive. Genetic studies of interracial crosses are reviewed, the lack of evidence of any deleterious effects of such mixture emphasised, and historical examples cited of societies which may well have benefited from 'race' mixture. The book ends by advocating as the solution to the 'race' problem a programme of education in human relations for the young—education to new appreciations of human differences, of the place of the individual in society, and of the significance of being human.

A somewhat disjointed and occasionally repetitive character is given the book by the inclusion, in addition to the early chapters which carry the bulk of the author's argument, of additional material of varying age and pertinence. Of the three topical essays added to the present edition, that on the meaning of equal opportunity (ch. 18) fits well and makes some good points. That on the disadvantaged status of the American Indian (ch. 16) has not been related to the main argument of the book. Chapter 19 on 'sociogenic brain damage' combines valuable (though again not strictly relevant) insights with an impassioned attack on Jensen's studies of racial differences in intelligence test scores which makes up in sarcasm for its lack of quantitative argument. What can best be called a non-chapter (ch. 9) parallels race prejudice with antifeminism. Chapter 17 on the non-existence of the Jewish 'race' and ch. 15 on the physical characters of the American Negro (including cranial capacity, brain anatomy, and body odour), as well as ch. 20 which advances an enlightened attitude on human variation as a challenge to American (U S.) patriotism, give the impression of having been topical once.

It is lamentable that the author indulges in some of the worst vices of the racists he so rightly takes to task. How can he, in a book of this type, present the bald statement 'In the Western world the German people have especially excelled in the art of creating myths' (p. 48) or quote the sweeping assertion that interracial crossing produces in the Brazilian

'that ideal type of man for the Tropics, the European with Negro or Indian blood to revive his energy' (p. 226) utterly escapes this reader. Pure speculation on the motives and thoughts of American Negro students is presented without apology or qualification (p. 408).

Lapses of logical consistency also detract from the book, notably the use and damnation as convenient, of extrapolations from animal behaviour and genetics to the human situation (chs. 10 and 19). A similar attitude though admitted by the author, is shown towards the cephalic index.

This book fights the good fight, so much so that in large part it seems almost to mimic the racist propaganda it attacks. Even today, when—to a notable extent by the efforts of its author—its once iconoclastic assertions seem almost commonplace, it is a work of aggressive optimism.

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McGUIRE, MICHAEL T. *et al. The St Kitts vervet* (Contrib. Primatol. 1). xii, 202 pp., illus., maps, tables, bibliogr. Basel. S Karger, 1974. £8·10

RUMBAUGH, DUANE M. *Gibbon and Siamang.* 2, *Anatomy, dentition taxonomy, molecular evolution and behavior.* viii, 230 pp., illus., tables, bibliogrs. Basel: Karger, 1973 £17·15

RUMBAUGH, DUANE M. (ed.). *Gibbon and Siamang* 3, *Natural history, social behavior, reproduction, vocalizations, prehension* viii, 209 pp., illus., plates, tables, bibliogr. Basel: S. Karger, 1974. £8·85

The three volumes reviewed here deal with a variety of aspects of two widely different primates, the vervets of St Kitts and the gibbons and siamangs of south-east Asia. *The St Kitts vervet* is a report on current research into the history, behaviour, and population aspects of a 300-year-old stabilised population of African green monkeys on the Caribbean island of St Kitts. The habitat of the vervets of St Kitts varies from dense forest—like that of the south-east Asian gibbon and siamang habitat —which is unusual for this species, to a hot, dry savannah-like peninsula which resembles more closely the vervet's known African habitat. The report covers the history and ecologies of the island's population of vervets, social behaviour, population genetics of island populations and subsequent adaptations, and a comparison between the vervets of St Kitts and of Amboseli Park of Kenya. Free ranging behaviour and behaviour in large and small cages are studied at St Kitts. The *Gibbon and Siamang* volumes are the second and third of a continuing series on these interesting lesser apes. Volume 2 deals with the anatomy, dentition, musculature, recent palaeontology, molecular evolution, dermatoglyphics, manipulatory behaviour, serial discrimination reversal, and some aspects of captive behaviour, especially births. Volume 3 has a far more behavioural perspective; dealing with the natural history of populations on the Malayan peninsula, adult social interactions within a colony, reproductive behaviour, and a study of the thumb of these lesser apes. These two volumes provide an interesting and valuable addition to the general store of knowledge on these lesser (known) apes.

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Archaeology

SINGH, PURUSHOTTAM. *Neolithic cultures of western Asia.* xii, 240 pp, illus., maps, plates, tables, bibliogr. London, New York: Seminar Press, 1974. £4·00

In a book written during a sabbatical taken at the University of London, Professor Singh provides a useful compendium of the floral, faunal and archaeological data which document the transition from a hunting and gathering mode of subsistence to one in which domesticated plants and animals play important roles Restricted geographically to western Asia and temporally to the Early Holocene (11,000–6,000 B P), the text is divided into three major sections

The first part (ch. 1) is a cursory review of models which purport to explain and/or describe the appearance of domesticates. Economically based, crudely evolutionary, trait oriented stage/era models of Thomsen, Nilsson, Morgan and Tylor are mentioned for historical perspective; Childe and Braidwood merit more detailed treatment, and the work of the latter provides an implicit structural framework for chs 2–5. Systemic/ecological proposals by Binford and Flannery are given short shrift. This is unfortunate because these models have considerable explanatory potential, whereas those incorporated into the text are essentially descriptive A 'nuclear area' perspective is adopted, no real consideration is given to alternatives. The 'neolithic' in the title (*pace* Braidwood) restricts the discussion to those extinct societies from which there is primary (morphological) and/or secondary age/sex ratio data) evidence for domestics.

The long second part (chs 2–5) consists of a site inventory broken down geographically into the Levant (10 sites), Turkey (9 sites), Iraq (18 sites) and Iran (9 sites, site groups). This section is the strongest part of the book because it summarises data which are widely scattered and often difficult of access. Descriptions of architectural remains, artefactual

material, burial practices, evidence for trade, chronology and stratigraphy are provided, but floral and faunal remains are especially emphasised as these constitute the *sine qua non* for inclusion in the inventory. These chapters suffer from an overabundance of descriptive minutiae, but still provide a handy, single-volume reference work for the specialist, and a valuable data summary for the underclassman confronted with an 'origins of domestication' course. Because this section is organised according to contemporary political states, the extent to which one can discern developmental trends in a mass of culture-historical and natural science data is a function of prior theoretical grounding. In other words the 'big picture' does not emerge so clearly as it might have if these encapsulated site reports were organised in terms of Braidwoodian stages and eras.

The last part (ch. 6) compensates for this to a degree by summarising the evidence for the earliest West Asian domesticated plants (emmer, einkorn, barley) and animals (dog, sheep, goat, pig, cattle). Architecture and ceramics are also considered, almost as an afterthought and probably because of their residual value as criteria in Childe's scheme. A long series of radiocarbon dates is included as an appendix.

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Clark, Grahame. *The earlier Stone Age settlement of Scandinavia*. xxvi, 282 pp., illus., maps, tables, bibliogr. Cambridge: Univ. Press, 1975. £7·50

This is an important and a readable book, not merely for archaeologists concerned with northern Europe, but for a much wider audience concerned with the social life of early man. With the self-confidence of long experience the author may cut away a difficulty occasionally, but the concern is always with the link between the evidence and the ways in which early man exploited the environment. The book is an almost total re-write of the *Mesolithic settlement of northern Europe* of 1936, indeed a different book. While many of the illustrations will be familiar, the epistemology has evolved a long way. The period covered is that from the Late Glacial through the Pre-Boreal and Boreal up to the end of the Atlantic period; a second volume is in preparation and will cover the neolithic up to the establishment of metallurgy.

The first chapter includes a statement of the premisses of a 'Social Archaeology' which constitutes one of the more significant aspects of the book as a whole. The idea that 'economic life cannot be understood out of its social context' is implicit both in the work of the anthropologists whom Clark cites, and of others such as Daryll Forde; but Clark's usage must be understood in the context of the long perspective of research he summarises for his chosen area. The application to archaeological situations also involves ecology and ethology; but while Clark is right to emphasise (p. 4) that models from these may not be adequate to account for the complexities of transmitted culture, the biological sciences have long ago come to grips with problems not only of taxonomy, but of behavioural complexity to which it seems as if archaeologists are only belatedly awakening. One could put it with more restraint perhaps by saying that archaeology falls among the unrestricted sciences as Pantin (*Relations between the sciences*, C.U.P. 1969) describes them. These include the study of living systems with emergent properties, and there is no reason to suppose that mere cultural transmission sets human societies outside such systems, although they are admittedly among the most complex examples of them. The merit of this chapter is that it gives rise to a whole series of such reactions. In relation to stability and change (as discussed e.g. pp. 26–7) one could ask whether equilibria in society as Clark describes them cannot be seen as an application of Le Chatelier's principle of least action much as can for example mechanisms of habituation as defined in animal learning. Clark also here discusses the case for an optimistic archaeology (most welcome), and ideas of territory and seasonality, social hierarchy and redistribution mechanisms, all in ways which give rise to a desire for further elucidation, rather than as is nowadays commonly the case, being so cautiously put as to be meaningless. The distinction between micro- and macro-changes (p. 26) is a valid one which archaeologists often fail to make. The notion of 'inherent dynamism' or the inclusion of the social with the natural environment (both on p. 27), as also the 'primitive egalitarian' basis (p. 21), leave one less happy, except in so far as happiness lies in seeing the chance of an argument. The notions of techno-territory and social territory are both very interesting ones, and I think bear some relationship to the notion of the 'Techno-complex' outlined in Chapter 8 of David Clarke's *Analytical archaeology*.

The next chapter outlines changes in the ecosystems of the chosen area, while the following three deal with the late glacial, early post-glacial, and Atlantic developments respectively in the southern part of it. The final chapter considers the northern area (the Fenno-Scandian shield) as a whole. This is clearly

logical, and there is much material in these sections which cannot be discussed here. It may be said that the reasons for choosing this part of Europe as given in the Preface are very good ones, and that it is in many ways an ideal region. Clark's work on this testing-ground in fact emphasises the complexity of what is relatively a simple case, 'marginal to the inhabited world' (p. 27), in which the removal of the ice left a pristine situation, in which archaeological and environmental researches have been highly developed, and in which what is extraneous to the aboriginal course of development can be relatively easily detected throughout prehistory. Even within Europe these conditions rarely obtain. Within each chapter consideration is given to the chronological framework, to settlement, subsistence and technology, with some particularly good sections such as the discussion of the discrimination in use of different materials in the section on early post-glacial technology, and the general elucidation of the Maglemose–Kongemose–Vedbaek–Bloksbjerg–Ertebølle sequence of developments. The point about the use of beaver incisors (p. 122) is noteworthy, and it might have been worth mentioning the Janislawice mesolithic burial in this context.

There are one or two misprints (p 1–1949 for 1939; 22–Petrosavodek for Petrosavodsk; 65–andromous for anadromous; 68–Younger Dryas described as zone IV; 118–distaff for distal; 150–knob-less for knotless; and some specific names of animals). The dates for zone boundaries, which are variable enough in the literature in any case, are confusingly shown in Table 1 (p. 46) both as initial (Nilsson etc.) and final dates (Tauber) for their respective zones. (The question of what zone boundaries represent is not in any case an easy one.) Population estimates are never difficult to criticise; but if a total reindeer population (330,000) and an annual requirement per family unit (500 deer) are postulated (p 98), then surely some other calculation as to the number of family units must be made than one (660 units) which implies the prompt extermination of the reindeer population? The whole point about *successful* reindeer herders was that they *managed* the animals, whether consciously or not, in accord with the carrying capacity of the biome and habitat. The faunal tables also largely restrict themselves to presence/absence.

It is to be hoped that the idea of social archaeology, that of the social life of prehistoric man as a fundamental topic, towards which this work looks, may be amplified and clarified in the future, perhaps even by those who feel critical of it as it stands. It is vain to hope that playing with methodology, however rigorous, will mechanically supply a satisfactory epistemology, and there is no substitute for insight into the realities of existence in a given region such as Clark provides. He throws out points which others might labour to elaborate into a paper, and many of them are worth taking up.

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Social Anthropology

LOMBARD, JACQUES *L'anthropologie britannique contemporaine* (Collection SUP 30). 260 pp, bibliogr. Paris: Presses Universitaires de France, 1972. F22

It is always interesting to find out what others think of you. Jacques Lombard is the latest French scholar, and in many ways the most thorough, in examining British anthropology

The book is one of a series directed by Professor Balandier which is intended to summarise and make easily available various subject matters in the social sciences. It is therefore not surprising to find that this book is in no way polemical Perhaps it might even be said that the author is a little too retiring with his own judgements, considering the sharpness of his remarks when he occasionally chooses to be critical Rather, the work is an attempt to present clearly the thought of the more notable British social anthropologists by reviewing the various positions and placing them in relation one to another. The anthropologists considered in detail are first and foremost Malinowski and Radcliffe-Brown. The chapters dealing with their work take up over three-fifths of the book; the other authors studied in detail are Fortes, Eggan, Evans-Pritchard, Firth, Nadel, Gluckman, and Leach. Not only are these authors' works set out but the major criticisms of their work are also noted.

If there is a surprise in the book it is the overwhelmingly dominant role accorded to Malinowski, not only as an ethnographer but as a theorist. If we compare the number of pages allocated to Malinowski in the recent book on the same subject by Adam Kuper, a book of similar length, we find that it is only 37 as against 100 here. This difference reflects well the towering role which French anthropologists consider Malinowski to have exercised over their British colleagues. I find myself in complete agreement with this point of view, and as Lombard suggests, the reason why Malinowski has been so often and so effectively criticised is less because of any exceptional failure on his part but because he was not afraid to say clearly what he believed. The controversies which this quality gave rise to, taken together with the influence exercised by

Radcliffe-Brown is what gave British anthropology the vigour and relevance which Lombard admires. He finds himself much more sceptical of the claims of later anthropologists, especially Evans-Pritchard and Leach, and he doubts whether they have broken away from the influence of their two predecessors as much as they would have us believe. It is perhaps because of the stress on Malinowski and Radcliffe-Brown that the least well represented writer is probably Evans-Pritchard; Lombard does not distinguish enough between his earlier work and his later move towards an anti-rationalist idealist position which contrast so sharply with his colleagues.

Two unexpected points are also worth noting. First, Lombard stresses the importance of the concept of culture in the work of Fortes, and of psychology in that of Firth, secondly *The Andaman Islanders* is given a very large place in the pages devoted to Radcliffe-Brown. Finally, and this is perhaps inevitable in such a book, the importance of the ethnographic contribution made by British social anthropology seems to me not to be given its due place. Often the genuine theoretical position of these writers is more clearly discerned from a critical examination of what they did rather than of what they said they were doing.

This is a very valuable, fair, and accurate book, an excellent account. It is perhaps regrettable that it is also a little dull.

MAURICE BLOCH

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HATCH, ELVIN. *Theories of man and culture.* xii, 384 pp., bibliogr. New York, London. Columbia Univ. Press, 1973. £5·75

Professor Hatch has given us a gratifyingly lucid and scholarly account of some of the minds who have contributed most to the formation of anthropological thought in our age. In his discussion of ten theorists from France, Germany, Britain and the United States the author identifies two foci round which anthropological ideas have developed: the image of man and the concept of culture. He shows how Boas, finding Tylor's intellectualist approach inadequate to deal with the materials he was collecting in North America, was led to develop the ideas of culture as a phenomenon *sui generis* and of the primacy of subjective interpretation which have guided and provoked American theorists ever since Hatch draws an interesting parallel between Boas and Durkheim, whose independently developed ideas have a remarkable structural resemblance to the mature thought of the German-American, and the Frenchman was destined to provide the theoretical guidelines for another stream of anthropological thought, that of British anthropology.

Hatch writes illuminatingly of the connexion between the thought of Boas and later theorists in American anthropology, notably Kroeber, Steward, Benedict and White. The two latter thinkers exemplified extreme developments, in opposite directions, of Boas's central ideas. For Benedict the ideational structure of any culture was absolutely determinative of individual personality and behaviour; for White a society's relation with its environment, recognised as a limiting factor by Boas, entirely determined individual thought and action through the medium of technology. Thus in both opposed streams derived from the anti-positivist Boas a new positivism asserted itself.

Turning to Britain, Professor Hatch has a rather similar tale to tell in his interpretation of the thought of Malinowski, Radcliffe-Brown and Evans-Pritchard. The last he sees as returning, in reaction against Radcliffe-Brown's reification of society, to Malinowski's emphasis on the reality of the human individual. Hatch correctly recognises the implicit and undeclared nature of much of Evans-Pritchard's most profound intuitions, but nevertheless gives the impression that Evans-Pritchard's ideas have transformed British social anthropology. Would that it were so! The sad truth is that positivism reigns as triumphantly as ever, in its structural-functional, neo-utilitarian and neo-marxian versions.

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COHEN, ABNER. *Two dimensional man: an essay on the anthropology of power and symbolism in complex society.* xii, 156 pp., bibliogr. London: Routledge & Kegan Paul, 1974. £2·75

This short, stimulating book reiterates and extends the concluding chapter of the author's *Custom and politics in urban Africa, a study of Hausa migrants in Yoruba towns* (1969), restates the distinction made in his article 'Political anthropology: the analysis of the symbolism of power relations' (1969) between power and symbolism in anthropological analysis, and attempts to examine the dualism here asserted as a 'dialectical interdependence between power relationships and symbolic action in modern, complex society'. However, it becomes clear that man, for Cohen, is basically a political animal, hence one-dimensional. Social life is a struggle for power and privilege, even if many believe it not to be. Man is one-dimensional, though bifurcate, since all rests

on interest. For Cohen, therefore, it is all the more telling if he can demonstrate that it is not only formal, explicitly organised political groups and quasi-groups (parties, factions, coalitions, and the like) which exemplify the typically human life thus defined. His programme is, therefore, to study systematically 'informally organized interest groups' or 'invisible organizations', particularly in modern society. He provides several case studies of such groups: 'ethnic groups, elite groups, ritual groups, groups with secret patterns of symbolic action, and groups organized as cousinhoods' (p. 91). He discusses the following groups Hausa diaspora in Yoruba towns, City of London financial elites, the Tijaniyya Islamic Sufi brotherhood among the Hausa enclave in Ibadan, Black Muslims in American cities, Creole Freemasons in Sierra Leone, the Anglo-Jewish gentry ('the cousinhood') of Britain, and Arab pseudo-patrilineages objectively 'held together through an elaborate body of symbolic beliefs and practices related to the women of the group' (p. 115) and focused on norms of parallel cousin marriage (an 'Arab cousinhood').

Although these and similar groups do not or cannot (p. 154) organise themselves explicitly for action in political arenas, they 'manipulate different forms of symbols in order to articulate informal organizational functions' (p. 14). Thus 'religious beliefs, sentiments, rituals and organization become also *instrumental* [my emphasis] in co-ordinating the corporate organizations of interest groups' (p 15) Again, 'some groups exploit ethnicity, manipulating values, myths, rituals and ceremonials from their cultural tradition (p. 15) in a political way, vis-à-vis other groups in the contemporary situation, and to solve their internal organizational problems'. Cohen focuses on the *manipulation* of religion and its symbols in their 'instrumental' capacity and fails to consider the *practice* of religion and its positive consequences for political action. Although he announces (p. 14) that 'everywhere, Man the Symbolist and Man the Political act on one another', he does not pay the same attention to the 'action' of the former as he does to the latter. Man the Symbolist is acted on, rather than acts. That is why his account is not truly 'dialectical' (p. 13), for dialectic is based on the principle that an idea or event generates its opposite, leading to a reconciliation of opposites. The antithetical quality of symbolic action, the ways in which it may reverse or invert 'power relationships', is not probed; instead we are given terms such as 'beliefs', 'sentiments', 'myths', 'rituals' and 'ceremonials' which are 'manipulated' like so many inert tokens by actors in the political process. It is true that Cohen, in chapter 4, does admit that symbols of selfhood and cosmological symbols are 'never the mechanical reflections of power relations' (p. 64), but he is more concerned with their 'exploitation' than their meaning. The rich symbols of religion, myth and ritual remain for Cohen a residual category of cultural phenomena: 'the less formal the group, the more the exploitation of symbolic patterns of action in it. Where contract fails, moral or ritual obligations are put to use' (p. 64).

I would suggest that symbolic action is rather more than *faute de mieux*. Gregory Bateson, Clifford Geertz and others have recently stressed the important role of certain cultural performances as 'metasocial commentary', 'metalanguages', 'social reflexivity', as sets of discourses which go beyond or transcend the languages of the home, forum or marketplace, commenting on these critically, presenting alternative models for living, assigning new meaning to old facts, and generally stimulating, altering, modulating or reinforcing patterns of social behaviour. 'Power' and 'interest' are key words for Cohen; I would suggest that metasocial symbolic action, often occurring in privileged, liminal, sequestered times and places, has the capacity to scrutinise, assess, and, if necessary reject, the criteria by which cultural standards of power and interest are traditionally judged. For example, the metalanguage of the salvation religions frequently proposes disinterest in place of interest, the renunciation of power and wealth, withdrawal from political arenas, abandonment to God, and so on. Symbolic action based on these proposals often affects behaviour initiated by the raw pursuit of interest or desire for power or authority. Of course, anything institutionalised can be manipulated, and institutionalised forms of religion can be manipulated as readily as any others. But religion is not the only source of forms of symbolic action, art, literature, music, sport, games, in fact the whole domain of the 'ludic' as opened out for us by Huizinga, provide interpretations of the current 'human condition' and imply remedies for its ills. All these symbolic processes and phenomena incite people to act, and often in ways truly antithetical to the power transactions and interactions which form the real monistic staple of Cohen's account despite his lip-service to dual motivation.

Yet Cohen's book has the great merit of recognising cultural life as constituting a 'dynamic of continuity and change'. But he should pay as much attention to 'action semiotics', as to 'power relationships'.

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MAUSS, MARCEL. *A general theory of magic*; translated by Robert Brain; [preface by D. Pocock]. vi, 148 pp. London, Boston: Routledge & Kegan Paul, 1972. £2·25

There are a number of things to say about this translated edition before it is possible to consider its basic merits. The first is that the essay was not first published, as stated opposite the Contents page, in *Sociologie et anthropologie* which appeared in 1950, but of course much earlier in the *Année Sociologique 1902–1903* (1904). The fact that the present translation appears to have been made from its later republication in a volume of Mauss's collected essays presumably explains the second, more egregious error which is the omission of Henri Hubert as co-author (despite the fact that the co-authorship is acknowledged on pp. viii and 1 of *Sociologie et anthropologie* and that the *nous*/we of the 'Prologue' must refer to them both). If either Dr Brain or Dr Pocock knows of any reason why Hubert should not share with Mauss the credit of authorship they should have said so, or should say so now. The use of the later text for the purposes of translation (which incidentally omits the important, historically placing first sentence of the original: 'Les travaux que nous présentons aux lecteurs de l'Année Sociologique ont, avant tout, pour objet d'introduire un certain nombre de notions définies dans l'étude des phénomènes religieux.') also helps to explain, what is accounted for in a note preceding the earlier text but is here blandly ignored, the untypical—and in some ways crucial—absence of detailed, scholarly references to the sources the two authors are drawing upon. (The relevant note explains the omission of reference notes by pressure of space but promises that when the authors republish the work they will then include the references, a promise which seems never to have been kept.) It is a pity, to put it most mildly, that a book that is intended for serious study and certainly to be used by students should have its value so detracted that it can be used only with a verbal erratum slip of such extraordinary proportions.

But putting these matters to one side: why is it that Mauss's and Hubert's 'theory of magic', despite the suggestiveness that it has obviously had for many writers up to the present, cannot ultimately be sustained? Why is the attempt to provide such a theory self-defeating? David Pocock tackles this question in his Preface obliquely, by summarising and pointing Lévi-Strauss's commentary on the *Théorie de la magie* in his Introduction to the volume of Mauss's essays referred to above. Pocock's view is that 'in the perspective of Lévi-Strauss's own achievement... the importance of the present work is that it contributes to the dissolution of "magic" as a category'. If not so elusive a paradox (which Dr Pocock explicitly acknowledges) this conclusion would surely be a tombstone to sink the book. For a paradox it surely is, since Mauss and Hubert pin so much of their methodological faith (and here they are orthodox Durkheimians) in defining 'magic' as a distinctive class of empirically identifiable social facts; '... our aim is not only to define words, but to set up natural classes of facts and, once we have established them, to attempt an analysis which will be as explanatory as possible' (present edition p. 7) Can a book contribute to an effect which runs clean counter to the stated intention of its authors? The answer, I suppose, is that it can if it passes through the catalytic influence of Lévi-Strauss's mind; and indeed what Pocock says about what Lévi-Strauss says about Mauss's (and Hubert's) essay is relevant.

Yet to tackle the same problem rather less obliquely, the fact is that while many of the categorising concepts deployed by the writers of the *Année Sociologique*, and perhaps especially by Mauss, *could* be used of a distinctive class of social institution or fact (e.g. 'sacrifice', 'gift'), the term 'magic' is far less easily pinned on the observable universe of events. It is a word, one might say, which is hardly ever claimed, or attracted, or inherently attached by or to an activity or set of activities: rather it is a categorical description attributed by others, operating from a different position. A is 'magic' primarily by virtue of B's saying so. This does *not* mean that magic lies only in the eyes of the beholder, for clearly B's attribution of 'magical' to A depends in part upon the nature of A itself. Rather 'magical' is a kind of conversation-word: a category which cross-cuts situations and cultural contexts and enables them to be related to each other while recognising their difference. (Consider, for example, the force of the word in the title to Una Maclean's recent book on *Magical medicine*. But however one may account positively for the term, the term certainly does not denote the kind of empirically identifiable class of facts ('natural classes of facts') that Mauss and Hubert have taken it to do.

What then this essay achieves is to concretise a chimera by a wide-ranging synthesis of facts and observations. It is possible to document this process in detail from the descriptive argument of the book, the absence of bibliographic references means moreover that the facts must cling to the putative class concept ('magic' as something out there in the universe) for their very intellectual coherence. The presentation of *mana* as a concluding exemplar of the magical does nothing to re-

solve—but descriptively intensifies—this confusion of explanatory context. (Is *mana* 'magic' or *magic* 'mana'?) Although it is difficult to state briefly, Mauss's and Hubert's final theory brings the matter full circle, for their explanation of magic is to refer its reality not to the class of facts alone but to their social context, the reality of the collective life of people in society. Thus, in considering *mana* '... we come face to face with the whole idea on which magic is founded, in fact with magic itself. It goes without saying that ideas like this have no *raison d'être* outside society, that they are absurd as far as pure reason is concerned and that they derive purely and simply from the functioning of the collective life' (p. 121). Magic is analytically presented as the force of social situation, an expression of the idea of 'power-milieu'.

> 'The two separate notions of magical virtue and social position coincide in so far as one depends on the other. Basically in magic it is always a matter of the respective values recognised by society. These values do not depend, in fact, on the intrinsic qualities of a thing or person, but on the status or rank attributed to them by all-powerful public opinion, by its prejudices. They are social facts, not experimental facts' (p 120).

This conclusion, published in 1904, should surely remind us not of Lévi-Strauss but of Durkheim, whose *Rules* had already been published seven years earlier but whose *Elementary forms* was not to be published until eight years later and whose own theory of religion takes over from and extends the essential argument of this essay If, as the world has found, it is a more satisfactory, or at least fruitful theory, its failing is curiously the obverse of Mauss's and Hubert's· instead of concretising a chimera, Durkheim as we know 'in escaping from the toils of positivism' rather '... overshot the mark [to go] clean over to idealism'. But this is to give an account of Mauss's and Hubert's essay using a different perspective from Lévi-Strauss's achievement, although it is one in which his achievement would still find place

As always, Dr Brain holds a lively pen and the translation reads well.

Malcolm Ruel

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Southall, Aidan (ed.) *Urban anthropology: cross-cultural studies of urbanization*. vi, 489 pp., tables, bibliogr. New York, London: Oxford Univ. Press, 1973 £2·25

Although this volume appeared in 1973, it is based on papers prepared for a conference in 1964. Any reader must bear this in mind. For although most, if not all contributions have been revised, the issues to which they are addressed have sometimes been by-passed in subsequent developments.

There are sixteen pieces concerned mainly with urban systems in the Third World (though the image of the contemporary American city hovers in the background). Besides an Introduction, there are three theoretical papers by Banton, Leeds and Southall, and a series of case-studies covering a wide geographical range Some go over familiar terrain. The work of Banton and Southall on roles, Bruner on the Batak in Medan, Lewis on Mexico City, Lloyd on the Yoruba, and Mangin on the *barricades* of Lima will be well-known from other publications. Others will reach a wider audience, perhaps for the first time, e.g. Shack on the Gurage in Addis Ababa, Rowe on Bombay and Bangalore, and Nyakakalou (with Southall) on Fiji There are two contributions on Japan· a historical survey by Yazaki, and a fascinating analysis of eighteenth- and nineteenth-century census evidence by R. J. Smith, perhaps the most interesting chapter in the volume There is also another elegant piece of quantitative analysis from the pen (computer?) of J. C. Mitchell.

There are several themes First the problem of definition, which troubles several contributors but is discussed most fully by Southall. His advocacy of the 'Density of role relationships as a universal index of urbanization' is a continuation and refinement of an earlier argument. While the central notion may provide a framework for comparison, it is extremely difficult to operationalise. To apply such a measure rigorously would involve the nearly impossible tasks of identifying and counting 'role relationships'. It may have value as an ideal-typical construct, but where in the end does it lead us? A second, related, theme is the nature of the 'traditional' or 'pre-industrial' city: in general terms, Southall (pp 92–98) has a good discussion of Sjoberg, and in particular situations, such as Japan, Nigeria, and India. Rowe has some interesting things to say regarding the conceptual system reflected in Indian city structure. The problem here is continuity and change in the modern context. Thirdly there are patterns of migration, the significance of rural ties in urban settings. This relates to a fourth theme, ethnicity, though the contributions here should be set against more recent work in this field Note, however, Bruner's material on the process by which Batak migrants acquire information about the urban ethnic scene Finally, two important questions are raised by Leeds: the nature of the urban anthro-

pologist's unit of study, and the relationship between what he terms 'localities' and 'supralocal' institutions. This, the problem of the micro and the macro, is also mentioned by Banton who sees role-analysis as providing the necessary link.

In all a somewhat mixed bag which does not really hang together as a collection and which does not always reflect the contemporary concerns of urban anthropology. There is a useful bibliography by Gutkind, but, unforgivably, no index.

R. D. GRILLO

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CASSADY, RALPH JR. *Exchange by private treaty*; foreword by E. T. Grether (Stud. Marketing 19). xxiv, 287 pp., tables, bibliogr. Austin. Bureau of Business Research, 1974. $6

From an anthropological point of view this is an unsatisfactory book empirically and theoretically. It purports to be a study of 'private treaty' or negotiated pricing methods, i.e. the situation in which the price paid for an object is arrived at by negotiation between the buyer and the seller (Cassady does not discuss services). This would be a useful exercise if it did not suffer from a number of serious flaws, since Cassaday attempts to explore this theme on a comparative basis, drawing his material from a number of very different kinds of economies.

The attempt fails however for several reasons. At the level of data, Cassady's material would fail to satisfy the most undemanding anthropologist, being for the most part shallow, second-hand and uncritically based on a very limited range of published material. The section on negotiated pricing in the Middle East for instance rests almost exclusively on one journal article which is accepted as a full account of the material under discussion, although it clearly is not. At the level of theory the author's thesis is even weaker. He fails initially to justify his idiosyncratic concept of 'exchange', since this is not normally a term which is taken to apply to buyer-seller situations, where the transaction involves money. Indeed the central role of exchange in 'exchange economies' distinguishes them from cash economies. Secondly Cassady is the victim of a crude evolutionism. 'Primitive', 'peasant' and 'modern' economies are ranked on the basis of the extent to which they conform to the market principle, pre-modern societies being those in which this principle, as largely revealed by the extent of the 'negotiated pricing' mechanism in them, has not fully 'developed'. This is the central theoretical weakness of the work, since the whole exercise becomes purely an attempt to show that the simple logic of the pure maximisation hypothesis applies to all 'exchange' situations in which there is an element of negotiation. Where the logic does not seem to apply, Cassady explains this away by reference either to the (as yet) embryonic nature of the market principle in the society in question (as in 'silent trading' situations) or by attempting to show that non-maximisation of advantage is simply a ploy to retain 'goodwill' on the part of the trading partner, who is then available to be exploited on some further occasion. Although there is evidence that Cassady (a marketing economist) has referred to a considerable body of literature in economic anthropology, he is serenely unaware of the major theoretical controversies which surround much of the literature he cites. In short the book's exclusive emphasis on assumed and abstract concepts of supply, demand, strategy and tactics renders it sociologically vacuous. It neither asks nor answers the questions an anthropologist would ask of Cassady's data, and is chiefly of interest for the clarity with which it reveals the behaviourist assumptions of what is evidently still a flourishing approach to economic activities.

JOHN CLAMMER

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LANGNESS, L. L. *The study of culture*. xiv, 181 pp., illus., maps, bibliogr. San Francisco: Chandler & Sharp, 1974

If you want to know what anthropology looks like from the United States, this is your book. Anthropology, which is the study of culture, concerns itself with the different ways man has related to his material environment and the worlds of meaning he has created on the basis of his technology. Both American and British anthropology share some at least of the same founding fathers (Tylor, Morgan and Malinowski) but they talk quite different languages. This book has almost nothing to say about the post-Malinowski tradition of social anthropology in Britain, though Radcliffe-Brown rates a brief mention, probably because he spent a long time in the United States. I imagine that Victor Turner appears for the same reason. Langness is fairly comprehensive in his references to the principal theorists in American anthropology, but his discussion of such key figures as Sapir, Steward, White and Harris is hurried and superficial. The book concludes with the customary exhortations about the contributions anthropologists can and should make to the building of a brighter and better world.

This book will do nothing to dissipate the lamentable prejudices about American anthropology which are so deeply ingrained in Britain. Someday someone should write a book about *that*

R. G. Willis

Edinburgh University

Naroll, Raoul & Ronald Cohen (eds) *A handbook of method in cultural anthropology*. xxii, 1017 pp., illus., plates, tables, bibliogr New York, London· Columbia Univ. Press, 1973, £18

The Handbook edited by Naroll and Cohen, amounts to some 650,000 words organised into 7 sections and 49 chapters. Among its 47 contributors are many who are well-known for their methodological contributions to cultural anthropology in America. The work is directed particularly at those who are contemplating becoming professional anthropologists It is unfortunate, therefore, that many of the essays are dull and that some are little more than rather tedious summaries of their authors' previous works. The tediousness is also increased by far too much repetition of basic arguments and citations.

Social anthropologists will find the first four sections which discuss epistemological issues, causal and diachronic analysis, fieldwork and ethnographic analysis, the most congenial. The last three sections deal in the main with the methods and techniques of cross-cultural surveys.

In the second section, Kobben contributes a thoughtful essay on cause and intention in which he argues that anthropologists cannot proceed in analysis without some concept of cause, whilst Vansina examines the problems of integrating data from diverse sources in diachronic analyses. Boudon's well-known, but highly technical, paper on the statistical analysis of causal models is republished. Oddly, the anthropologist is urged to battle through it without being told that Boudon's technique is only one of a number recently developed, and not really suitable for the analysis of variables measured at the nominal or ordinal levels.

In the section devoted to fieldwork, Levine discusses sympathetically the means by which more problem oriented fieldwork might be conducted without sacrificing the anthropologist's desire to seek out what seem meaningful problems to him in his particular field situation. For those contemplating fieldwork, the four essays dealing with entrée to the field, of which Middleton's and Uchendu's are the most interesting, will be useful. They also reveal the marked contrast in methods between those anthropologists who work through interpreters with regular informants and those who do not. Particularly outstanding is the essay by Margaret Mead, in which she offers much sound advice on a whole range of issues concerned with undertaking and preparing for fieldwork.

The section on models of ethnographic analysis seems in many ways redundant. Dalton, Cohen and Middleton contribute essays on economic, political and religious systems, respectively, whilst Hsu discusses the study of literate civilisations. However, the serious student will almost certainly have read these authors' earlier works. Only Keesing's essay on role analysis, in which he builds on Goodenough's earlier work, has anything really new to say. Miller's treatment of the personality as a system seems particularly dated: he refers to the lack of literature on social networks, for example, and cites only the early work of Barnes and Bott.

The last three sections of *The Handbook* are devoted largely to issues concerned with cross-cultural surveys. Despite the elegance of many of the techniques discussed, the considerable improvements which have been achieved in data coding and quality control, the sophistication of the treatment of Galton's problem, the ingenious efforts to circumvent the non-randomness of the samples, and so forth, this reviewer remains sceptical of the claims for the method. Kobben, in a stimulating essay, looks critically at the history of the debate between comparativists and non-comparativists. But, even his reasoned account is marred by the implication (which is also found in other essays) that a statistical test of significance constitutes a 'mathematical proof' superior to other forms of verification. This reflects a basic confusion of statistical with scientific inference. This reviewer would argue that an adequate theoretical structure which enables a variety of hypotheses to be derived from it can be tested without recourse to random sampling and significance tests, since the theory itself will indicate the scope of its application and the critical instances which will provide the test. Let us have bigger and better *Ethnographic Atlases*, by all means, but let us not be trapped into naively counting scores in favour of one hypothesis as against another without reference to its explanatory adequacy and scope of application.

Pierre Maranda's work, by contrast, is not directed to would-be professionals, but to young students (at about sixth-form or first-year university level). Its aim is, through a series of relatively simple fieldwork exercises undertaken in a local community, to give students some insight into anthropological

fieldwork and analysis. All the major fields of traditional ethnographic enquiry are covered. For each field there is a brief introduction and a short bibliography. The student is then asked to formulate a series of 'commonsense' hypotheses which he is guided to test on data collected in the local community by means of a number of more or less formal protocols. No doubt many social anthropologists would have objections to some of the material, but anyone contemplating establishing a practically oriented course at this level will find much of interest in Maranda's basic approach.

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EVANS-PRITCHARD, E E (ed) *Man and woman among the Azande.* 197 pp, plates, bibliogr. London: Faber & Faber, 1974. £4·50

This is a book not of theoretical analysis but of texts. Evans-Pritchard's aim is best described in his own words as he states it in his brief preface. 'to introduce to a European audience in a series of sketches what Africans, or at any rate some Africans, are really like, how they talk and think .. In these texts I am asking Azande to say in their own way what they want to say The texts are on a variety of topics, but all are in some way or another concerned with relations between men and women and domestic life'

The texts in the book were mostly taken from dictation between 1927 and 1930, but there is also a section—Part II 'Modern times' —specially written by two literate Azande between 1961 and 1964 They range in length from a few lines to several pages, with one fascinating story of a man's difficulties with a reluctant wife, running to ten pages and cover a variety of topics from the etiquette of eating, the contrast between men's and women's work or the rituals of widowhood, to dialogues between lovers and extremely explicit sexual descriptions.

The publication of this collection of texts is typical of Evans-Pritchard's approach to fieldwork and to anthropology. He attributes his interest in recording texts to Malinowski's influence, but one suspects that his earlier training as a historian and his resultant instinct for amassing written documentation also had something to do with it. In addition he makes the characteristic point that such texts 'allow the native to speak for himself, give his point of view without the anthropologist interrupting and, as he thinks, being an interpreter'. In this way Evans-Pritchard hopes to present 'his' people as fully human and personal in a way they do not appear in traditional accounts which, he claims, 'dehumanized the Africans into systems and structures and lost the flesh and blood.'

The wider accessibility of texts like these can only be welcome, and for the reasons Evans-Pritchard himself gives. But it could be a mistake to accept too uncritically everything that is claimed in either preface or texts. In the first place—as Evans-Pritchard himself was of course well aware—it is naive to claim that texts of themselves necessarily demonstrate what the Azande 'are really like'. Texts may indeed be more 'primary' than some sources, but there is nothing absolute about this 'primacy', interpretation and selection by both speakers and collector inevitably enter in, and in any case texts comprise only one type of source, however important, among many others Again, the detailed circumstances of the recording of the various texts, and the names and statuses of the speakers are very seldom explained. Yet nowadays surely even the most elementary training in sociology would indicate the need to note the social context of a 'text' before attempting to understand its true import. Without this the texts are inevitably less reliable sources than they appear at first sight

A further temptation for the reviewer is wholeheartedly to recommend a book like this as a magnificent source book for all those interested in Azande life, or indeed in the relations between men and women But it has to be admitted too that—despite many gripping, moving and even amusing passages— many of the brief accounts and generalised descriptions are really rather tedious to read through when one is faced with page after page of them. One finds oneself wishing that —in spite of his very proper principles—the editor had after all selected the highlights only, decided for us what was 'significant' or 'memorable'. But perhaps this is, in the end, one of the enduring virtues of a collection like this. One is forced to recognise more explicitly than usual that most anthropological accounts do in fact pre-select a very narrow range of 'facts' from an immense mass of possible sources. The near infinite extent of 'sources' and 'data' on modern industrial society and the difficulties of how to select from them is a well-known problem. *Man and woman among the Azande* reminds us that this can equally be a problem for non-industrial societies, and that anthropological accounts which fail to recognise this problem can only be naive. Massive documentation can have its tedious side—but an awareness of its existence and extent is surely a prerequisite for scholarship, whether in anthropology or history

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FEIERMAN, STEVEN. *The Shambaa kingdom: a history*. xii, 235 pp., maps, bibliogr. Madison: Univ. of Wisconsin Press, 1974. $12.50

It is now thirteen years since Edgar Winans's book, *Shambala: the constitution of a traditional state* (London 1962) was published. If *Man* had honoured him with a review (which it appears not to have done), I suspect that attention would have been drawn to the casual way in which he slipped through time coupling vague statements about a pre-colonial past with unsystematic observations of the late colonial present. The result was a benign and diffuse account interspersed with conventional anthropological comments and jargon that seemed to have no special relevance to the Shambala. One felt as though this ground had all been covered before, and Shambala—or Shambaa—was not anthropologically remarkable.

Now it is the turn of a historian to attempt to place this important kingdom of the Kenya-Tanzania border on the academic map. The result is fascinating It is not simply that Feierman's structural analysis of the Mbegha myth outclasses that of the anthropologist, but also that he demonstrates vividly a nineteenth-century pattern of development that may serve as a warning to us all when we are tempted to refer loosely to traditional pre-colonial society in static terms. The clue to his success is surely the systematic way in which he covered the ground with different informants, piecing together a pattern that is both original and perceptive Anthropologists may fault this approach on the grounds that oral statements do not allow us to trace out the past with any accuracy, but much of our understanding of traditional African states is necessarily derived from historical inference from such sources; and the Shambaa at least were important enough and near enough to the coast to attract considerable attention in the early literature

According to this reconstruction, each reign entailed a developmental cycle from the tenuous period when a new monarch was opposed by powerful senior members of his family to the subsequent phases of consolidation when he successively replaced them as chiefs with his own sons and *protégés* Competition within the royal clan was, however, eased during the first half of the nineteenth-century by the creation of new chiefdoms as the kingdom expanded into the fertile highland areas towards the coast. It is Feierman's personal view that during this period, the Shambaa kingdom held itself aloof from the slave trade: the elite relied on tribute from the area of densely populated upland, and accepted in return a responsibility to protect their subjects. Slave raiding and trading were confined to their sparsely populated neighbours in the plains. However, the opportunity was there, and following a long reign and period of stability there was a succession dispute when expansion was no longer possible, and in the hiatus, local chiefs and opportunists asserted their independence, remoter Shambaa subjects revolted, and former loyalties and acknowledged responsibilities atrophied. For a period until the advent of German administration, the former kingdom broke down into petty chiefdoms, slave raiding, anarchy, treachery and large-scale population upheavals.

The merit of this work is the clear and lucid style in which the period of stability followed by radical breakdown is presented. The author has clearly felt his way into the nineteenth century and has emerged with a convincing and well supported analysis. He stops short, unfortunately, of developing this analysis to add to a broader model of centralised political systems and of political transitions, although he is clearly aware of its relevance (pp. 166–7, 189). In tracing the political processes of the nineteenth century, he makes no attempt to derive additional insight by utilising his initial analysis of the Mbegha myth which also concerned a contrast between the life-giving hills and the death-dealing plains. He accepts almost without question that Mbegha, the culture-hero of Shambaa, was the eighteenth-century founder of the royal dynasty (ch. 3), whereas an anthropologist could argue that the evidence is slender and Mbegha's historical existence is irrelevant to the subsequent theme. He only makes one reference (in a footnote, p. 85) to Winans's book, and yet after all this does provide additional evidence which on a number of counts appears to contradict Feierman's thesis this avoidance (coupled with the alternative spelling of Shambaa), is one assumes, no mere scholastic lapse. There is also a certain lack of caution in handling the broader history of East Africa, as when he confidently relates the Mbugu neighbours of the Shambaa to the Mogogodo, relying without qualification on some tentative (and as it happens misleading) comments of earlier writers (pp. 74–5)

It is not, then, a complete book from every point of view. However, we are promised a further monograph, *The concept of sovereignty among the Shambaa*, submitted as a doctoral thesis in social anthropology at Oxford, that could well complement the historical analysis of the present volume and answer some of these criticisms. As it is, we already have here a thoroughly readable exposition that implicitly challenges the impermeability of interdisciplinary boundaries.

PAUL SPENCER
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HAMMOND-TOOKE, W. D. (ed.). *The Bantu-speaking peoples of southern Africa*; [second edition]. xxii, 525 pp., illus., plates, maps, tables, bibliogr. London: Routledge & Kegan Paul, 1974. £7·95

This book is the successor to the one that Schapera edited in 1937, *The Bantu tribes of South Africa*. The only contributor to the first book who has a chapter in this one is van Warmelo, but parts of the sections written by Schapera are repeated, and his later writings provide the main source for the Tswana.

The highlight of the book is the brilliant analysis by Basil Sansom of traditional political and economic systems This is linked to an ecological division of southern Africa into a relatively well-watered eastern and an arid western zone. The typical economy of the east is that of the Nguni, the extreme form of the western economy, which varies more widely, is that of the Tswana. Political types are correlated with these, the Sotho forming the western type, the Nguni the eastern. Sansom describes these as Tribal Estate and Chequerboard Realm respectively. The difference lies in the type of resources controlled by the ruler. The Tribal Estate was a single unit of management in which the ruler made grants of pasture-land to headmen by allotting them cattle posts. The policy was held together by the requirement that the herdsmen return to the town after the harvest, there only through the chief's *kgotla* could conflicts over pasture resources be settled without fighting

In the Chequerboard Realm the boundaries of land holdings were fixed and resources could not be constantly re-allocated. Here the ruler maintained his power by making appointments to fixed offices. In an ingenious contrast Sansom argues that political rituals emphasised tribal unity in the Chequerboard Realm, separate categories in the Tribal Estate. It would be most interesting to see whether this stimulating treatment could be applied to other African kingdoms—Ashanti for instance, or Buganda.

Hammond-Tooke deals with religion in two chapters concerned respectively with belief and action. In this and in the section on kinship (Eleanor Preston-Whyte) one finds a rather naive surprise at the recognition of matrilateral kin in patrilineal societies, something that surely has come to be taken for granted. On the other hand, the ritual functions of a father's sister among Venda and Lobedu are unusual enough to have deserved the mention they receive. Half a page of statements on the treatment of 'convicted' witches shows striking differences among the various peoples, and plenty of evidence of condign punishment. In a book of this kind there is not much space for speculation as to the proportion of cases when the more ferocious penalties were actually inflicted; some reference might have been made to Schapera's Huxley Lecture on the subject. Hammond-Tooke's statement of the essential values of southern Bantu peoples is masterly.

J. A. Benyon treats of the political and D. Hobart Houghton of the economic incorporation of the African peoples in a white-ruled state. The former has to grapple with the problem that confronts every historian who is asked to compress his knowledge into a small space, causal relationships are not always obvious. He shows, however, how the seeds of *apartheid* were seen long before it became explicit policy, and also how and why governments at Westminster insisted on the codification of customary law. Houghton's central interest is the inefficiency of migrant labour and its harmful effect on family life. He makes the point that if it were not for *apartheid*, many more Bantu would have become townsmen; yet one wonders whether the attachment to the home place is entirely the product of insecurity in town. B A. Pauw shows how African Christians interpret their religion. Allie Bubb surveys the literature on urbanisation, and quotes some interesting unpublished material on the new elites whose ambitions are thwarted by *apartheid*. he is not of those who see them as the beneficiaries of the system.

The frontispiece map unfortunately contains a good many mis-spellings of Bantu names, and shows the boundary of Lesotho as it was before the Treaty of Thaba Bosiu.

LUCY MAIR

London

BAHR, DONALD M *et al Piman shamanism and staying sickness (Ká. cim Múmkidag)*. xii, 332 pp, illus, tables, bibliogr. Tucson: Univ. of Arizona Press, 1974. $7.95

Donald Bahr spent 1½ years talking frequently to Juan Gregorio, a Papago shaman, about Piman concepts of illness, and was assisted in this work by two other Papago. The book is an examination and explanation of their twenty-four taped conversations, with an emphasis on 'staying sickness' (*ka:cim*), divination (*dúajida/kúlañmada*), songs, and prayers.

The detailed analysis of Gregorio's texts is impressive. Bahr also used historical sources, but Gregorio's texts are the core of the book, and the following quotation virtually chosen at random on 'staying sickness' provides much of their flavour. In Gregorio's words:

. . And sometimes, furthermore, it happens that children

who never take things seriously,

they think it will be fun [to run into the
whirlwind].
A whirlwind will come along,
they will run to it,
and they will get into it,
yet it is dangerous.
Then later on
whether quickly or perhaps not immediately,
well, it will reach them,
and it [wind strength] will sicken them
(pp 74–5).

This is why Gregorio answered Bahr's questions Bahr wove his more and more sophisticated questions and Gregorio's answers into a Piman world view—with illness as the focus. I feel that anthropologists who have worked with shamans may well relive their experiences in reading this book

'Staying sickness' is Piman—exclusively 'Wandering sickness' is universal—a germ theory of Anglos and others The shaman divines illness, discovers the cause of 'staying sickness', he deals with 'ways', with spirits, and 'strengths' The reader is led through a maze of concepts at the shaman's level of explanation These concepts are then analysed in sections of the book which deal with the nature of *ka cim* sickness, *dúajida*, ritual curing etc. This is a linguistically-oriented book and there are three levels of interpretation. Piman, literal translation, and free translation which involves anthropological analysis

Shamans divine, laymen cure The cure is through prayer 'The prayers are directed at the spirit ..' (p 232) We are scarcely told how or why Gregorio is one of an estimated one hundred and fifty shamans in Papago society Thus we have a single informant, his world view, and yet no indication of his position in the social organisation of the Pima-Papago The authors point out that their work 'is based on the theories of a single informant and is not sacrosanct' (p 22), and this is a weakness of the study

In the concluding chapter a plea is made for a western acceptance of the Piman theory of illness as an established one, and further for the training of Pimans to write about their own values and concepts

A methodologically dubious but imaginative study.

Louis C Faron

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Kochman, Thomas (ed.), *Rappin' and stylin' out: communication in urban Black America.* xviii, 424 pp., illus., tables. Urbana, London: Univ. of Illinois Press, 1972 £5·90

This volume of some twenty-eight pieces explores the 'communicative habits and expressive life style' (p. xi) mainly of black Americans in a variety of contexts and situations. The aim, perhaps a familiar one, is to provide as near to an inside view as possible while avoiding the (white middle class) assumptions of mainstream social science concerning the 'deviant' nature of black culture.

The range of articles, about half of which have appeared previously, is very wide, from technical pieces in the fields of kinesics (Cooke), ethnomusicology (Keil, Sithole), and socio-linguistics (Labov), to autobiography (Rap Brown, Iceberg Slim's 'Pimp: the story of my life'), and fiction. The academic contributions, mainly by anthropologists, linguists and educationalists, vary in approach, but the influence of Dell Hymes is apparent throughout They also vary in quality. Some have the feel of working papers (e g Horton's 'Time and cool people') which often raise points of considerable interest while not always following them through in any great depth (e.g Williams's 'Dynamics of a black audience') Others are primarily ethnographic. Not a few, however, make contributions to theory or method which have wide application The autobiographical extracts, the fiction, and the pieces by non-specialists writing from their own experience (e.g. Kohl and Hinton on naming) are well-chosen and provide vital support for the more analytical writing

There are four sections· 'Nonverbal communication', 'Vocabulary and culture', 'Expressive uses of language', and 'Expressive role behaviour'. While each has much to offer, the third contains most articles of interest to British anthropologists Of these the pieces by Labov (an innovative study of the 'Rules for ritual insults') and Abrahams ('Joking .') are perhaps the pick of the book. Note also Holt's 'Stylin' outta' the black pulpit' for its analysis of church ritual and the relationship between religious and political rhetoric.

The usefulness of the volume is, however, weakened by four omissions. A comprehensive bibliography and a glossary would have been helpful, the latter especially for foreign readers. An index one would have thought essential if the reader is to collate and compare the references to particular concepts discussed in several articles. Finally, a fuller introduction could have examined the implications of the contributions for our understanding of black American culture as a whole How exactly does the emphasis on verbal dexterity—stressed throughout the volume—fit in? And what implications are there for our understanding of other cultures which similarly value rhetorical prowess?

R. D. Grillo

University of Sussex

PRICE, RICHARD (ed.). *Maroon societies: rebel slave communities in the Americas*. vii, 429 pp., illus., bibliogr. New York. Anchor Press/Doubleday, 1973. $2.95 (paper)

This collection of readings can be seen to have two purposes: the first is to provide documentation (some of it never before available in English), on an important and fascinating aspect of Afro-american history; the second is to contribute to our understanding of social and cultural process in groups severely disrupted by European colonial expansion. Most of the selections, which deal with those communities of rebel slaves which grew up from time to time in practically every New World country where slavery was institutionalised, are extremely interesting and the book should be widely used in courses dealing with Afro-american problems. It can be added to the growing literature on slave revolts as a corrective to the commonly accepted view of slave submissiveness and apathy.

Of even more interest to anthropologists are the questions raised by Professor Price in his excellent Introduction He tells us that from one point of view the book is '.. an extended argument that maroon societies form a class or type that can yield unique insights about the Afro-american experience' (p. 4) But he also tells us that he is biased in favour of getting at the internal dynamics of these groups rather than seeing them merely as part of the European or colonial societies in which they occurred. Unfortunately there are very few studies which deal with maroon communities from the inside and with an adequate level of field research, most of those relate to the Bush Negroes of Surinam and they are represented by only three selections taking up less than 70 pages out of a total of nearly 400 It is true that there are a number of selections from accounts by captured rebels or sympathetic Europeans, but these are very different from first-hand field studies.

Professor Price puts his finger on a number of issues which deserve more extensive exploration in a different kind of comparative framework. He rightly questions that point of view which concentrates upon the search for 'survivals' of African culture traits, and while taking issue with Bastide's characterisation of maroon societies as exhibiting a 'split between infra- and superstructures', he argues that there are fundamental principles which integrate these societies, and which give them their characteristic shape These may partially derive from Africa, but they cannot be understood as simple retentions of cultural traits, they are to be found at 'deeper levels' in the form of 'underlying cultural principles' which interact with varying historical circumstances in the creative process of social action.

It is precisely here that we are faced with a quite general problem in social and cultural analysis. It surfaces in the study of East Indian communities in the Caribbean and elsewhere; it may be said to be an important issue in the study of 'New Nations' generally; it is analogous to the study of creoles and pidgins in linguistics; and it also raises the whole question of what constitutes social and cultural 'integration' anyway. Professor Price must be commended for having brought this problem to our attention and shown the relevance of maroon societies for its study, but I do not think that the selections printed in *this* book can carry the enquiry very far As a general survey illuminating a particular phase of Afro-american history it is excellent, the more theoretically exciting venture into comparative analysis of socio-cultural process outlined in the Introduction remains to be carried out and will usefully include a wider range of cases than those subsumed under the term 'maroon societies'.

RAYMOND T SMITH

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ANTOUN, RICHARD & ILIYA HARIK (eds). *Rural politics and social change in the Middle East* (Stud Dev. 5) xiv, 498 pp, tables, bibliogr Bloomington, London. Indiana Univ Press, 1972 £6·20

In 1969, the International Research Center in Indiana University sponsored an anthropologically based, but interdisciplinary, conference on rural politics and social change in the Middle East This heavy well-produced tome, contains sixteen papers (one of them written after the conference), an introduction, and five distinguished 'summary comments'. The cast is star-studded In the opening section, five authors, F. G Bailey, Henry Rosenfeld, Robert Fernea, Xavier de Planhol, and Richard Antoun review 'The state of theory and literature' The next section, under the title 'Case studies. local political processes', comprises papers by Emrys Peters (Druze village re-analysed), Fuad Khuri (Beirut suburbs), Lawrence Rosen (a local election in Morocco), Michael Meeker (Black Sea Turks), Nadia Abu-Zahra (Tunisian village), Iliya Harik (bureaucrats and villagers in Egypt) and Janet Abu-Lughod (migration in Egypt). Finally, under the title 'Economic change and reform', Harik interprets recent Middle East history in terms of the rise of domestic national markets, Nikki Keddie interprets Iranian land reform in historical terms, Wyn Owen offers some economic logic on land reform policy in general, and John Simmons reports a survey on Tunisian citrus growers.

The editors have done a competent job, the

book—like most books—contains far too many words, and no-one but a misguided reviewer would read it all. Each author presents his fieldwork to best advantage, or rides a private hobby horse, or both. The result is plenty of information, a variety of Aunt Sallies, some silly, some inviting, some provocative; several common themes and some mutual relevance; in short, a cross-purpose group marriage of high if variable fertility. The first seven pages of introduction provide a polite guide to the collection, and Clifford Geertz's 'comment' summarises trenchantly and memorably many issues raised, and castigates some fallacies and errors perpetrated; yet offers some hope

Several authors emphasise again how little anthropology has so far been done in the Middle East; and the book does report new and re-analysed ethnography and history—the well-tried method of accidental comparison. What is more striking is the soul searching. Rosenfeld attacks us all sweepingly Peters offers a moving, honest and fascinating recantation of his previous analysis of his Druze village—one of the best criticisms of British traditional so called 'structural-functional' assumptions I have read, but this new use for the word 'structure' left me unconvinced Meeker exemplifies a combination of fieldwork data and historical analysis applied not to a village but to a region. Antoun takes us back to face-to-face 'social control' and its relation to politics, producing an analysis of 'variables' from a non-accidental comparison of seven societies.

Among the non-anthropologists I personally enjoyed the two historical pieces by Harik on Nasser's political methods. But while village anecdotes, even village analyses, appear parochial and trivial, the high-level simplifications of historians, economists (Wyn Owen produces an absurdly cogent *reductio ad absurdum*) and political scientists are not far off 'a kind of sociological morality play populated by stock characters—landlords, peasants, élites, bureaucrats, migrants and so on—running through stock melodramas called revolution, reaction, democratization, or progress' (Geertz p. 463)

Those who paid for it—I deduce—wanted the conference to further 'modernisation' (perhaps the emptiest of all words of rhetorical approval) in the non-communist third world The conference—which in Bailey's remarks contained virtually no peasants, politicians or 'regionals'—was more interested with a few exceptions in looking and understanding. (Fernea (p. 101) seemed to advocate looking before trying to understand.) Numerous issues are raised in this attempt; yet already some of the soul searching is dated, and in particular, Marx hardly appears at all, even to be refuted. Clearly a book important both for Middle East scholars and for political anthropologists. It demonstrates that we have barely begun in either field; yet it leaves me wondering what anthropologists should best be getting on with next.

PAUL STIRLING

University of Kent, Canterbury

BEALS, ALAN R. *Village life in south India; cultural design and environmental variation* (Worlds of Man). 189 pp., illus, tables, bibliogr. Chicago Aldine, 1974. $7 50 (cloth), $2 95 (paper)

Over the past two decades, Professor Beals has made field studies of three villages lying within the Kannada-speaking plateau region of South India The villages are of different size and social composition, and have different degrees of contact with cities. Elephant is small, single caste and in a forest area until recently isolated Gopalpur is larger and multi-caste, and lies in a region with a long history of contacts with ruling regimes· and Namhalli is socially and economically heterogenous and situated near the metropolis of Bangalore.

Professor Beals outlines the social characteristics of the villages in a series of chapters on such topics as economics, religion, politics, kinship and marriage, etc Where comparisons are made, there is some illuminating material. For instance, the chapter dealing with the villages' relations with the government contrasts Elephant (until recently exploitation by officials, provoked concealment and flight) with Gopalpur (a succession of landed village aristocrats with clients, largely independent of the outside administration) and Namhalli (acceptance of administration from outside, recently changed to the active manipulation of governmental agencies). The reader with some knowledge of South Asian material will regret that little detailed analysis and case material is possible in the short space allowed but for readers wanting a general picture, the accounts are interesting and very readable.

Because the book is part of a series, the villages are seen as parts of ecosystems and their institutions portrayed in this light. Thus 'a successful marriage . . . is essential to every aspect of the ecological system' (p 113) and this is made through the 'technology of marriage' i e. largely by selecting a suitable spouse. Nobody would dispute that the villages form parts of larger physical and social environments, but the terminology used does not always seem entirely necessary.

More debatable is the author's view of south Indian villages as forming parts of a distinctive civilisation, since we are not shown in what ways this civilisation is distinct. Pro-

fessor Beals stresses that a South Indian world view—based on the notions of *dharma* and *karma*—underlies the villages' social and cultural organisation: but he could then have indicated how the world view of the rest of India differs in its use of these concepts, as well as discussing the pan-Indian approaches of Dumont and others. Further, though he says that there is great variation within South India, he does not explore this in any detail. In a sense, then, this book has a framework which is too large for what it actually contains (and to which Professor Beals disarmingly limits his canvas at the outset)—which is a useful though compressed account of the main outlines of the three villages that Professor Beals has studied.

ADRIAN C. MAYER

SOAS, London

KESSINGER, TOM G. *Vilyatpur 1848–1968. social and economic change in a North Indian village* xiv, 227 pp., illus, tables Berkeley, London. Univ of California Press, 1974 £6

This study by an anthropologically-minded historian is based on an examination of village records supplemented by ten months fieldwork.

As a relief from despondent evaluations of development prospects in rural India, Vilyatpur—a Jat dominated village in the Punjab district of Jullundur—offers a modest success story Though the population has more than doubled since 1848, increased production has outstripped it and has generated a fairly evenly distributed rise in the standard of living Even the low caste landless labourers have experienced substantial economic (and political) gains as a result of new employment opportunities, though for many the cost has been a loss of the security they enjoyed in the traditional *jajmani* system, and Kessinger's assessment here would inspire greater confidence had he provided data on the purchasing power of wages Slogans about a 'green revolution' and the prospect of it 'turning red' seem remote from Vilyatpur realities, where growing prosperity stems from the cumulative impact of several long-term developments

Much of the book is concerned with the adaptive strategies by which the villagers adjust the supply of family labour to their capital resources by leasing land in and out, exploiting the local job market, and by migration. Migrants tend to be drawn from expanding families with insufficient land and their ultimate goal is to add to the family holding. At the turn of the century the village provided numerous recruits for the canal colonies and Australia, though migration now occurs on an even larger scale with Britain as a favourite destination. The result is that the importance of the community as the sole arbiter of success has declined. Outside employment and the new agricultural technology are gradually transforming an economy based on 'custom' to one based on the market, though the emphasis on family labour and the 'iron chest' system of accounting (which inhibits the calculation of gross production costs and net profits and the separation of the domestic from the farm economy) still largely govern production strategies and the kinds of innovation most readily acceptable. Kessinger's findings here are convincing and well documented, if for the most part unsurprising.

The same cannot be said of his discussion of the joint family viewed as a property holding group. He concludes that 'there has been a general decline in the proportion of families that embrace all of the appropriate kinsmen .. Before 1910 coparcener and close kinsman were likely to be synonymous, afterwards they seldom were' Quite apart from the difficulty of knowing what is meant by 'all of the appropriate kinsmen', the precise nature of this 'property group' remains obscure since the data suggest that the groups owning land, wells and houses may not be coterminous (The problem perhaps derives from Kessinger's 'substantialist' tendencies which lead him to gloss terms like *zat* and *khandan* as though they have absolutely fixed empirical referents) In a region where land often remains undivided on paper long after it has been partitioned in practice, one is also bound to feel uneasy about his central assumption that the revenue records accurately reflect the composition of the joint family.

Although the analysis is uneven, the book contains much to interest South Asianists of several disciplines as well as those who seek a wider understanding of immigrant communities

J. P. PARRY

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MALONEY, CLARENCE (ed). *South Asia: seven community profiles*. xvi, 304 pp , illus , tables, bibliogrs New York: Holt, Rinehart & Winston, 1974

The declared object of the enterprise here is to provide an introductory text for teaching high school or undergraduate students some of the complexities and diversities of South Asian societies. Its editor recommends that this book should be used together with his text *Peoples of South Asia* (Holt, Rinehart and Winston, 1974), while assuring readers that it can also be used by itself. Not having had a chance to read the accompanying volume I

cannot judge whether the two books jointly make a useful text, but I find it difficult to fathom out the precise function this book on its own is meant to serve. On the one hand some of the contributions are far too elementary to be used by staff as a teaching device, while on the other the book is unlikely to be useful to students either, since it lacks the necessary overall introduction and/or conclusion, which might help to integrate the seven snippets into one coherent whole. Instead, the editor introduces each chapter with a brief bibliographical note on its author and a summary of his paper. Such method of presentation is likely to encourage students to read the editor's summary and ignore the individual papers, which after all would defeat the purpose of this symposium.

The quality of the individual contributions varies greatly from the most interesting papers by G Obeyesekere on 'a village in Sri Lanka' and Lionel Caplan on the 'Limbus of Nepal' to K. Ishwaran's extremely pedestrian and unimaginative article on 'a village in Karnataka'. I suspect though that the considerable variation in the levels at which individual contributors pitched their analysis reflects more the poor editorial arrangements rather than differential abilities of the various participating authors There are as yet only few good textbooks available in social anthropology and fewer still which focus on South Asian societies in particular. To produce texts for students requires not only appropriate analytical ability but more important a perception of how best to stimulate young minds. This book shows that its editor may possess the former but seems to lack the latter.

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Islam, A K M Aminul. *A Bangladesh village conflict and cohesion an anthropological study of politics* xii, 196 pp, illus, tables, bibliogr Cambridge, Mass.. Schenkman, 1974 $6 75

This volume makes an important contribution to our understanding of the complex interaction and mutual interdependence between village and national politics. It focuses on Badarpur, a village situated within sixteen miles of Dacca, the Bangladesh capital. The book begins with a brief but scholarly review of the relevant political anthropology literature and underwrites propositions borne out by the Badarpur study while rejecting those which were found inapplicable. The author's breadth of vision and depth of study is very impressive He discusses not only the socio-economic framework within which the village polity operates, but also provides an illuminating account of Bengal's overall socio-cultural history. In doing so he enables the reader to view the village political process in different dimensions, that is diachronically, by seeing how it fits into the pattern of socio-political changes over time and synchronically, by examining the way individual village polities reflect and form part of the larger national entity like pieces in a jigsaw puzzle Like so many other anthropological village studies this one too overemphasises locality, kinship and religion as the main organising principles in society while failing to give due weight to the importance of vested economic interests affecting political activities. This slight deficiency, however, is made good by the abundance of case material and conversations, often translated verbatim, which enables the reader even to re-analyse the data The book illustrates both the advantages and disadvantages of a researcher indigenous to the culture he studies the author had no language problems, moreover, he was more sensitive to, and perceptive of, the nuances in social relations, which often escape the outsider and therefore he excels in presenting lively and fascinating case material, on the other hand, being himself embued with the local culture he failed to notice things which an outsider would have found strange, for instance he took no account of the role of women in the political process—even if they do not hold office they are likely to exert distinct influences over their menfolk. This possibility Aminul Islam ignored altogether. Though he completed his field work before the independence of Bangladesh, he carefully analysed the many different variables which together resulted in the struggle for the country's independence In doing so he indicated the important part village studies can play in a better understanding of the overall development process On this he must be congratulated.

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Jones, Schuyler. *Men of influence in Nuristan: a study of social control and dispute settlement in Waigal Valley, Afghanistan*, with photographs by the author and pencil drawings by Kenneth F. Campbell (Seminar Stud. Anthrop. 3). xiv, 299 pp, illus., maps, tables, bibliogr. London, New York: Seminar Press, 1974. £4·90

Robertson's *The Kafirs of the Hindu Kush*, a classic of late nineteenth-century British political-agent ethnography, retains its value today partly because of its apparent accuracy

and objectivity, but mainly because the society described began to be transformed in 1896, the very year in which the book was published. The Kafirs were subjected to Afghan rule and converted to Islam, and Kafiristan became known as Nuristan, the Land of Light.

The book under review is the first substantial ethnographic account of the former Kafirs to have appeared this century, though by no means Dr Jones's first publication on them. He has made several field trips into Nuristan, particularly among the Kalashum villagers of the Waigal Valley—which as it happens Robertson himself did not visit. This book describes their traditional social and political life and how it has changed. There were no chiefs or headmen; decisions were made, disputes settled, laws and customs affirmed or adjusted, by 'men of influence', who formerly achieved their rank either by giving a series of public feasts, or by raiding and killing Muslims. The author uses several fascinating case studies to illustrate how the Kalashum still largely control their internal political affairs, and concludes that the few who have been educated outside the Valley will seek to preserve this situation.

Jones undoubtedly knows the Kalashum well, and this book is a clearly written and authoritative document of their way of life He has not, however, made enough concessions to the average anthropological reader One notes that little reference is made to the general body of anthropological theory, even in the description and analysis of the topics which are of most theoretical and comparative interest, such as the feasts of merit, the complex ranking system, and the processes of mediation and dispute settlement Moreover, although verbatim texts are frequently given (in translation), along with a large number of Kalashum words referring mainly to objects, roles, social groups and categories, yet we are not told how the Kalashum express certain key notions on which the analysis relies, such as 'honour', 'status' and 'prestige' in the chapters on 'Village government'

More daunting perhaps for potential readers are a number of features of presentation. The first chapter, describing 'The events of 1876–1900' in a straight historical narrative, appears to have been added as an afterthought. At the end of the chapter, the author properly argues that the political and historical background 'is essential to any understanding of the current scene in Waigal Valley', but the account lacks a map and any explanation of the abbreviations in the references (to material from the India Office Library), and in its present form should be an Appendix. The transcription system used throughout the book is unusually complicated and not always consistent, nor is there any note of explanation of the plethora of diacritical marks, which leave the reader with no idea of how Kalashum words should be pronounced. Also unnecessary are the words given in Persian script, and the unattributed quotation in Arabic script. The style of the bibliography does not differentiate articles from books, and does not include all the works referred to in the text. On page 230 there are no less than fifty-nine page references to one of Jones's previous publications on Nuristan, where one reference would have sufficed. Photographs and drawings are plentiful, useful and evocative, though a list of them is not included. The maps are not particularly helpful and do not show many places mentioned several times in the text

These shortcomings aside, Jones has written an interesting and valuable book, which deserves a readership beyond that to which it appears to be addressed.

RICHARD TAPPER

SOAS, London

SKINNER, G WILLIAM (ed) *Modern Chinese society, an analytical bibliography 1 Publications in Western languages 1644–1972*; assisted by Deborah B. Honig and Edwin A. Winckler lxxviii, 802 pp, maps Stanford Univ Press, 1974 $35 00

It would be quite natural, at least in this country, to nod politely at such a bibliography and then pass on to more serious matters I had better say, then, that the volume under review (the first of three volumes published together—the others, dealing with works in Chinese and Japanese, not being considered here) is one of the most important contributions to the social anthropology of China to appear since that subject took shape. And I feel obliged to record an aspect of the enterprise that Professor Skinner has modestly passed over the monumental work has been built upon a personal sacrifice which those of us who have stood by have observed in awe as the eleven years of gruelling labour and struggle have unfolded. I can assert that no other scholar known to me would have had the resources of knowledge, intellect, technical ingenuity, entrepreneurial capacity, and moral and physical energy to bring off this masterpiece Heroic is the word for it. And of course one has to add· only in America ..

The bibliography is not the least useful for the Skinnerian view of Chinese society upon which it is based. (It would be instructive to compare it with the only other work of like scope, the now largely forgotten Spencerian compilation made by E. T. C. Werner, 1910, which, by the way, is not listed here, presumably because the rules for inclusion barred it:

a pity.) The bibliography is about Chinese in China: the cultural definition cuts out non-Chinese (principally 'aborigines') in China, while the geographical definition excludes what the cultural could have admitted: overseas Chinese. (But Hong Kong and Macao are safely in.) There are several theories built perhaps into the selection of the items, certainly into their arrangement; on the separation of elite and folk cultures, on the significance of 'local systems', on the nature of social stratification, and so on. We may not always agree with the theories, but at least we are not faced by a mindless listing of titles. I have already spent many happy hours *reading* this book. On how many bibliographies could one make that sort of comment?

It takes Professor Skinner many pages of succinct prose to explain the system of the bibliography and the mode of its compilation. Within the limits imposed upon me I can show only a few characteristics The team led by the Editor (feeble description) gathered in 90,000 titles by combing the libraries, evaluated them, and in the end annotated 43,000. There are just over 13,000 items in this volume. The main body of it arranges them so that each appears once within a framework of nearly 100 headings (e g., National Legal System, Peasant Associations and Movements, Property and Inheritance, Fertility and Mortality); under each heading the items are grouped in time-blocks. As it appears, each title is given a code number and provided with full biographical details, including a translation of the title if it is not in English, and birth- and death-dates, where possible, of the author (a very useful piece of information). There follow coded data on the library where the item can be checked, whether the work is based upon field research, personal observation, primary sources, or substantial secondary sources, on the type of place it deals with (urban or rural, in a number of categories), on the part of China it treats; and on the period of time it covers. At the base of each item there is a series of code numbers indicating, first, the subjects covered in the work that are 'judged to be of primary utility', and, second, those subjects considered to be of 'secondary utility'. This is the browsable and readable part of the volume.

There follow three indexes which sort the item numbers by historical period, by geographical entities, and by 'local systems' (so that, for example, in this last index one may find assembled all the items dealing with market towns in a particular province). The next indexes are by author (individual and institutional), and the final pages are given to a general index.

The general index apart, the book rests upon computer work from an early stage right to the end. (And it is probably the general index, I suspect, that will prove to be the least satisfactory part of the volume.) On the theme of the computer three points may be made. The first is that Professor Skinner devised a special system (now known as the Stanford Automated Bibliography System) which is available for use by any scholar wishing to embark upon a similar enterprise. The second is that the book demonstrates that the computer can produce well printed and handsome volumes The third is that corrections and additions can easily be incorporated; in principle, a new version could be produced whenever it is required. (But given the economics of the thing, can we really expect to see new editions appearing with any regularity?) As for the corrections that might be made at once, I ought to say that I have spotted very few errors, all of them minor

The imagination of even the dullest reader must be stirred by this book. Here is one point. Professor Skinner says, 'The need for analytical control of the relevant bibliography became apparent only in the 1950s, when the impossibility of doing field work on the mainland turned many faces toward the libraries' Problem: what benefits have accrued to sinological anthropology as a result, and how are they demonstrated by the modern items listed in the bibliography? Footnote: the subdued optimism expressed in Professor Skinner's next paragraph (China is now 'relatively close at hand . . , accessible to a broad spectrum of visitors, its leaders confident and seemingly open-minded') is, I suggest, misplaced in this context. As anthropologists, we are going, for a very long time yet, to have to rely upon 'residual China' and our ability to read.

MAURICE FREEDMAN

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ARCHAIMBAULT, CHARLES. *La course de pirogues au Laos: un complex culturel* (Artibus Asiae, suppl. 29). 126 pp., illus., plates. Ancona: Artibus Asiae, 1972. Sw F 70

As a ritual idiom, competitions are known in many parts of the world and specially in the Far East and south-east Asia. Here they find expression in canoe races, tug-of-war, hockey, fencing etc. Often it seems as if the competition itself is more important than who wins

In Laos as in some other Thai territories the most spectacular competitions are no doubt the canoe races. Dr Archaimbault, who has already given us many interesting results of his investigations concerning ritual actions among these people, presents in this book an admirable analysis of the canoe races in the small Thai states along the Mekong River:

Luangphrabang, Vientiane and Champassak (Bassak). As several rituals have disappeared, he has had to use various Thai texts. But his most important material he obtained through his own participant observation. Owing to this he has been able to contribute new and particularly rich ethnographical material which he could never have got from written sources. It goes without saying that not all documents can be complemented by field-work, but I think it is necessary to state that orientalists who are usually limited to a study of the texts, no doubt would be able to interpret these with better results if they had been able to observe the ethnographic reality

These races are rituals primarily connected with the cycle of production and that is why they are repeated once a year They have to do with the succession of the dry and rainy seasons. During the monsoon rains the rivers increase and the paddy fields are irrigated. At harvest time they must be drained anew The time for these operations is triggered off by the rituals which at the same time promote these events The races are thus a kind of regulation of the water-level and fertility and in this sense they control the agriculture. The water-courses are thought to be populated by water-spirits of various kinds controlling the flow of the water. The canoes, with the spirits they contain, can even be regarded as living 'sexual' beings. The competitions thus indicate the season and stimulate the spirits, which are also guardian spirits of the territories they supervise, to perform their tasks

In Luangphrabang they used to have canoe races twice a year, during the 9th laotic month (moon-year), i e about July, and in the 12th month, i.e. in November-December. The first of these, however, has not been performed since 1940 owing to the war. Nevertheless through interviews and recourse to laotic manuscripts, Dr Archaimbault has managed to obtain much interesting material. Earlier research by Przyluski and his pupils considered that the races were meant to conciliate the water-spirits and in this way stop the high water. Dr Archaimbault can with good reason show that the races had other functions too and that the variation from one part of Laos to another is considerable.

His most abundant material the author obtains from Champassak where the canoe races have a different character from that of the other states, owing to a different political evolution. The society here was considered to have been in a state of disintegration connected with a sexual transgression in mythical time, and this must be reversed, because the disgrace remains. For a few days during the races the subjugated aborigines (the *Kha*) are symbolically liberated from the sexual taboos normally attached to them and are regarded as rulers of the state. At the end of the rituals a great buffalo sacrifice is made, reinstating the social order and purging the society from sins. This means, after having succeeded in capturing the vital forces, a return from chaos. Every year the social order must be restored and renewed. It is easy to recognise the pattern.

In Champassak the Kha are the representatives of the local spirits and mediators in the social sphere in which the races are enacted. The sexual relations between the two peoples are here stressed. The competition does not restore the hierarchy in Bassak, but the administration of the principality is re-inaugurated by reinforcing the centre of the state. Thus through reinforcing the Royal authority the races also have a political element and the ties between village and capital are in this manner strengthened.

In Luangphrabang and Vientiane the royal power as well as the cosmos is recreated. It is in this way resanctioned for the conquering Lao who once took the country from the Kha. The king is here at the top of the hierarchy and below come the dignitaries and officers according to rank. All this is restored by the races.

The analysis presented here by Archaimbault is extremely thorough and his hypotheses give us a good basis for future studies of Thai cosmology, on which the rituals and the political structure depend Besides the text he has a collection of footnotes almost as exhaustive as the very rich text itself There are several indexes and a glossary which perhaps could have been made more detailed for the sake of those who are not used to the laotic terminology The book by Dr Archaimbault is an important contribution and an outstanding work.

KARL GUSTAV IZIKOWITZ

Goteborg

ROTH, G. K *Fijian way of life*, second edition with a new introduction by G. B. Milner. xl, 176 pp., illus., plates, bibliogr. Melbourne, London. Oxford Univ. Press, 1973. £4·90

Kingsley Roth's study of Fijians, published over twenty years ago and long since unobtainable, was the first of its kind since Sir Basil Thomson's *The Fijians* (1908). Three works following Thomson's, Wallace Deane's *Fijian society* (1921), A. B. Brewster's *The hill tribes of Fiji* (1922) and A. M. Hocart's *Lau Islands* (1929), were important but less broad in scope Roth brought to his study his stern, analytical mind which could never be diverted from anything but the fairest, the most meti-

culous standards that he expected of a writer and a worker in the field (like himself). He could have produced from his accumulation of knowledge a work at least twice the size but at the risk of loss of a pre-eminent economy and clarity of style All that he noted was the result of sharpness and understanding. He worked fervently to maintain Fijians' virtues and to help in their shortcomings. He would not allow a consuming devotion to Fijians to obfuscate openings for constructive criticism. as an Administrative Officer and Head of the Fijian Department he would not recoil from taking positive action without fear when he regarded it as necessary From the end of the 1920's he zealously sought to arrest any decline in the customs of the Fijians which he considered (when Fijians themselves sometimes had doubts) their life-force Thomson's *The Fijians* had as its sub-title *A story of the decay of custom*. Roth two to five decades later was only too aware of this trend his recording of what he saw and the culture he lived with remains first-class material

In the 1960's, if still not virtually exhausted, a small group, short in qualifications to do so, tried to find fault with basic aspects of the way Fijians lived, attacking particularly the tradition of Indirect Rule dating back to the Cession of Fijians to Britain exactly a century ago. These denigrators, following the facile fashion of criticising for the mere sake of it, revealed their shallow imagination, lack of depth of understanding and want of perspective. After the brief publicity that sniping attracts had started to die down, Professor Milner finally kills it off with a clear, calm, much more knowledgeable assessment than the detractors could ever have mustered in unison In addition to deracinating the would-be demolishers and to bring Roth's study up-to-date, Milner contributes valuable new material on Fiji and collates a bibliography essential for balanced knowledge of Fijians He restores the author of *Fijian way of life* to his rightful place as observer, recorder, judge and leader of the highest standing in his chosen and beloved territory The re-publication of this work, too important to be beyond the reach of students of Oceania, is well executed and timely.

PHILIP SNOW

Rugby School

LEVY, ROBERT I. *Tahitians mind and experience in the Society Islands*; illustrated by Pierre Heyman. xxviii, 547 pp., illus, bibliogr Chicago, London· Univ. of Chicago Press, 1973. £5·65

The author of this major study of culture, temperament and personality, devoted more than two years to field-work in two small communities in French Polynesia, a rural one in Huahine in the Leeward Islands and an urban one in Papeete, the capital of Tahiti. His specialised training in psychology, psychiatry and cultural anthropology have been put to good account. He also has first-hand knowledge of the mass of information, now covering a span of 200 years, which concerns Tahiti.

Early visitors found the Tahitians 'graceful, gentle, expressive, superficial, casual, and sometimes deceptive' (p. 99). In the main this agrees with the author's impressions of them today he calls them 'soft, gentle, reserved, controlled but not cramped' (p. 102) After well over a century of colonial and missionary influence, some of the widespread characteristics of Polynesian society, such as its emphasis on rank and status, have vanished from Tahiti. For instance there is by contrast with the pre-contact situation 'a marked absence of vertical relationships' (p 203). Nor has the Tahitian language of today any special terminology of respect

Physical and temperamental differences between the sexes, moreover, are not strongly marked and 'men . are not particularly more aggressive than women Women do not seem to be much "softer" or more "maternal" than men' (p 234) There is a noticeable tendency to avoid any conflict likely to culminate in violence and as the author points out 'a major effect of the breakdown of the traditional culture was to deprive the male's adult role of its aspects of power, ceremony, wisdom and adventure . ' (p 196).

In this unavoidably selective review of a book of 500 pages, two chapters need to be singled out for adverse comment. Chapter 8 on 'Thinking' emphasises what is alleged to be the 'concreteness' of Tahitian thought, but it does not make sufficient allowance either for the difficulties of the native informant or for those of his Western interpreter; both are handling abstractions in a highly artificial context and across an immense cultural gap. Chapter 10 on 'Moral behavior' explores the distinction between guilt and shame but fails to come to grips with the important sanction of public ridicule or with the social dimensions of 'face', both when it is 'saved' and when it is 'lost'

On the other hand, not content to describe and to underline the characteristic patterns of Tahitian society, the author goes on to draw some remarkably interesting conclusions Chapter 14 in particular on 'The question of maintenance' will repay close study Two of his insights seem to be sufficiently original and important to be mentioned in some detail.

First, the androgynous impression that various observers have formed of the Tahitian

character is underlined and epitomised by an endemic phenomenon which was first reported before the end of the eighteenth century and which is apparently still prevalent today: there are men known as *māhū* who wear women's clothing, have feminine traits and preferred occupations, who keep company with women and occasionally practise *fellatio* on normally heterosexual males. (The author calls this homosexuality but on his own evidence there are grounds for believing that at least some of the *māhū* are in fact transexuals; cf. pp. 130–41; 471–3.) What is remarkable however is that at any time there is one and only one practising *māhū* in every village, and that like the village chief or pastor he is considered to fill an exclusive function

The author suggests, with cogent arguments in support, that a *māhū*, by virtue of his feminine characteristics, performs a publicly-demonstrated role reversal, i e. that because of the relative lack in Tahitian society of marked contrast between manhood and womanhood he 'helps stabilize men's identities by contrast and negation' (p. 486). The presence of a recognised *māhū* in a neighbourhood conveys to every Tahitian male the message 'There but for the grace of God go I'

This is not as fantastic and far-fetched an interpretation as it may sound. Indeed it gains support from the structurally analogous situation presented by the traditional role of the Samoan village pastor. The latter is addressed with the same honorific term (*feagaiga*) which is used for a father's sister (*tama fafine*) at least partly because he has the same or similar sanctions at his disposal (notably the power of the angry curse). So, just as it can be argued that the spiritual role of a Samoan pastor *vis-à-vis* his appointed village or congregation is notionally equated to that of a male father's sister, the role of a Tahitian village *māhū* may notionally be that of a male wife. There can only be one *māhū* in a village at any given time, just as we speak of *the* (and not *a*) village idiot, and as Bateson (on whose theories Levy has drawn) has shown that at any one time there is always only one comic or one active schizophrenic in a family, the other members as it were constituting the 'audience'.

Secondly Professor Levy has an equally insightful interpretation of child adoption which is as frequent in Tahiti as in other parts of Polynesia. The message of adoption is that '*all* parent/children relationships will tend to be seen as contingent on choices, as not categorically necessary consequences of biological parenthood' (p. 483). Put in another way, a Tahitian brought up by his biological parents might agree that his attachment to them was increased by the fact that they had actually chosen to bring him up instead of giving him to foster parents as they might well have done and were perfectly entitled to.

The Polynesian view, that is to say, is the inverse of the Western view of adoption, namely its object is 'to establish between parents and *natural* children relationships which coincide as nearly as possible with those between parents and adopted children' (p. 484). This suggestion seems to throw new light on parenthood. It would imply that in any society, widespread adoption lessens the tensions between biological parents and offspring. Neither need feel trapped into an exclusive and inescapable relationship. One could even argue that the Tahitian adoption system is the expression in the area of parent/children relations of norms governing all association between men and women. Sexual intercourse and child-bearing in other words do not impose on genitors the obligation to bring up their offspring, any more than they entail a permanent union or marriage. In both cases it is a matter of grace and favour, of options and contingencies.

G. B. MILNER

SOAS, London

CNRS. *L'Aubrac* etude ethnologique, linguistique, agronomique et économique d'un établissement humain 4 vols. Paris: CNRS, 1970–3

This carefully prepared and lavishly produced series about a high-lying region of the Massif Central is closely associated with the Museum of Popular Arts and Traditions in Paris Methodical introductions and monographs are accompanied by copious references, tables, maps and excellent photographs. L'Aubrac is suitable for such intensive investigations because documents about large-scale exploitations by two abbeys, and late eighteenth century records about small and medium holdings are preserved.

Volume 2 is devoted to clearances, viticulture, the reduction of sheep rearing, the end of cereal production in favour of the evolution of a hardy breed of cattle, the historical development, social structure and common land of villages, the effects of the dissolution of the abbey, and the characteristics of the people.

Later volumes deal with the ethnology of the mid 1960's. Wealthier, easily accessible villages with fertile soil sent their cattle to the vast alpine pastures, while poorer villages provided the dairy men. The consequences of the War, mechanisation, the flight from the country and also the slow disappearance of archaic traits and gatherings with songs and dances are discussed. Domestic life did not greatly differ from other French regions.

Some box-wood blessed in Church on Palm Sunday protected the house against lightning and diseases. In the large living rooms, where meals were prepared and eaten, the parents slept in curtained off alcoves. During the winter old people still cooked on open hearths, in which a special log was burned at Christmas. The neighbour lent a hand when somebody died or a pig was killed.

Volume 4 depicts the team-work of three or four cowmen and cheese-makers in the alpine hut. These weather-wise, realistic *buronniers* were given neither to religious nor to magical practices. Accordion playing, wood carving, amusements and scuffles at fairs and bonfires on St John's Eve had ceased, but true or humorous stories were still told and the drive of the cattle to the mountains, the finest cows wearing the largest bells, remained spectacular. The growing demand for meat led to the decline of milk production. The life of the *buronniers* who left for Paris is vividly described. There is an efficient chapter on the various aspects of folk medicine ancestral remedies, officinal plants, the moon's influence, healing Saints and the predominantly male healers. The preparations for tourism which I saw in 1970 in the neighbouring Cantal are not mentioned

ELLEN ETTLINGER

Oxford

MARANDA, PIERRE. *French kinship. structure and history* (Janua Ling. 169) 160 pp, illus, map, tables, bibliogr The Hague, Paris Mouton, 1974. Dfl. 42

It would be agreeable to find more to praise in a book, evidently the result of much labour, on a subject which could be of great interest. But what Mr Maranda has produced can only be described as worthy of Mr Casaubon. It is a dreary miscellany of semi-information, almost all from secondary sources, interlarded with 'analysis' of a facile and pretentious kind. The topics with which he mainly concerns himself—French 'social structure' in medieval times, French kinship terminology, proverbs and folktales—do not come through as connected, nor is the direction of his discussion of each very clear Certainly no systematic picture of French kinship or its history emerges. Matters are not improved by a rebarbative style, with frequent plunges into German and French quotation, or by pointless information about the climate of France (p. 18), banalities (perhaps one should say non-sequiturs) like 'divorce rates have kept increasing. This had as consequences the multiplication of step relationships' (p. 133), and tendentious links such as: 'Like folk literature, law consists of relatively stable messages. In point of fact, it is difficult to distinguish between some proverbs and legal aphorisms' (p. 120). The list of kinship terms the author has compiled from dictionaries may prove serviceable. Apart from that, the best one can say of the book is that it contains interesting observations: for example, that in France a man might marry up but not down (p. 129) or that 'beau-' came to encompass step- as well as in-law relations (pp. 15, 54–5, 68) (both the reverse of the English situation). But these are not developed, and more can be learnt about French kinship in half an hour with the *Encyclopédie* than from the whole of this book.

There is a danger that what Maranda calls his modest results (p. 11) may be blamed on the subject rather than his treatment of it: and it seems worth pinpointing certain shortcomings in his researches. One is his neglect of primary sources: French law, contemporary legal and theological writings, the literature of particular disputes, novels, etc. have not been tapped at all. It is doubtful if he has even turned to 'parenté', 'mariage', etc. in his dictionaries. Another, possibly connected, defect is his omissions, some of them appalling. Neither the Church nor civil law receives more than a passing mention. The Reformation is not mentioned at all, the Revolution twice. Calvin's name never figures. The author appears unaware of the existence of canon law. There is no study of the nature of marriage, the system of reckoning kinship, inheritance, any changes in the law relating to divorce or anything else. Nor is there any account of the spheres in which obligations or prohibitions connected with kinship have featured. Last but not least, a theoretical distortion. One would infer from this book that the French had never systematised their own kinship. Their efforts are never described. Instead we have a whole chapter of 'analysis' based on a division into 'orders' of the author's invention. In France, as in England (and one need go no further than 'parenté' in the dictionaries to discover this), husband and wife are 'as one' (of one flesh), and the blood relatives of each are affines of the other to the same degree. By one mode of reckoning, the canon law, a generation up and down constitutes a degree, parents, children and siblings (here and always, by blood and in-law) being in the 1st degree (of consanguinity and affinity), grandparents, aunts and uncles, first cousins, etc. in the 2nd, and so on. By the other, the civil law reckoning, a generation up *or* down is one degree: parents and children are in the 1st degree, siblings and grandparents in the 2nd, uncles and aunts in the 3rd, first cousins in the 4th. In Maranda's system (pp. 50, 85), husband and

wife, parents and children and siblings share the 1st order, their 1st order relatives the 2nd, and so on. What is the matter with the indigenous structuring? We aren't told.

If Maranda's book opens up research into French kinship, it will have performed a service. European kinship, and especially its history, has been poorly studied, except by lawyers and theologians, in spite of its importance and the rich material available. There is room for a dozen pieces of research of value on French kinship alone, more if we add comparisons e.g. with England. 'Mais qu'importent les mésalliances dans une nation où l'égalité est en recommandation . . .?' is a contemporary French criticism ('mariage' in the *Encyclopédie*) of the Act of 1753, which abolished 'common law' marriage in England; one need only go to the primary sources to find endless matters of interest.

SYBIL WOLFRAM

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KAUFMANN, CHRISTIAN. *Das Töpferhandwerk der Kwoma in Nord-Neuguinea Beitrage zur Systematik primarer Töpfereiverfahren* (Basl. Beitr. Ethnol. 12) 265 pp, illus, plates, tables, bibliogr. Basel: Hans-Rudolf Schwabe, 1972

This work, a doctoral dissertation, is primarily concerned with pottery manufacture amongst the Kwoma of the Sepik District, New Guinea However, equal emphasis is given to the theoretical aspect of material culture studies and their relationship to social structure The book therefore divides itself into two fairly independent sections; one on methodology and theory, the other on ethnographic description.

As a prologue to theoretical considerations the author provides a brief history of the rise and decline of ethnology Other than establishing the importance of material culture studies, this section gives some interesting information on ethnological collections in Europe, particularly Germany.

Having discussed various levels of approach which the fieldworker may adopt as an observer, the theoretical interpretation of collected data is considered A somewhat idiosyncratic structuralism based on a combination of Lévi-Strauss and Leroi-Gourhan is used to establish cultural principles. Using Schefold's work (*Basl. Beitr. Ethnol* 4, 1966) a classificatory system for pottery manufacturing techniques is proposed. This individualistic approach to structuralism combined with an intrusion of psychological ideas (a leitmotiv in German anthropology) limits the appeal of this theoretical section. I was not convinced that Schefold's findings should have relevance for a different range of materials from a geographically close though culturally distinct group. Further, Schefold's study may be criticised on the grounds of biased sampling and subjectivity in labelling elements However the very real contribution here is the attempt to link ethnological studies to the major issues of current anthropological theory.

The final section gives a very full description of pottery manufacture amongst the most westerly Sepik group with such a tradition. With the accompanying photographs the reader is provided with an almost cinematographic account of the process throughout all its phases. Data are provided on almost all aspects associated with pottery manufacture. Division of labour, trade, analysis of motifs, apprenticeship, ceremonial and ritual are discussed even if rather briefly.

Lastly, Kwoma pottery is compared with neighbouring related groups which in turn leads to an attempt to clarify the problem of the Melanesian pottery tradition. It was to provide data for a reliable examination of this last question that the author undertook fieldwork along the Sepik River during 1965–67 The present work does little to clarify the situation Consistent comparisons are not attempted though this is expected in a forthcoming work by the author

Objects from the Kwoma and other ma-language groups have only recently been correctly ascribed to these peoples This and a previous paper by the author (*Verhandl naturf Ges. Basel* 79, 1968) will help identify material from 'among the most prolific artists of the Sepik area' (Newton, *Crocodile and cassowary*, New York, Museum of Primitive Art, 1971).

It is disappointing to find a neglect of certain aesthetic issues particularly in view of Newton's comment. Whiting (*Becoming a Kwoma*, Oxford Univ. Press, 1941) remarks that prior to the cult ceremonies, judgements are made on the cult objects and due praise given to their carvers. This intriguing aspect of Kwoma culture deserves more attention than it is given

The book provides useful information for a wide range of students. However, a more lavishly illustrated work would have increased its usefulness, and for an otherwise detailed study, the scanty index will give some difficulty to the reader searching for specific information.

M. JOY

London

SALMON, PIERRE, *Le racisme devant l'histoire.* 195 pp. Paris: Fernand Nathan; Bruxelles; Editions Labor, 1973

In dealing with the racial problem Pierre Salmon continues a tradition of international studies in which many Belgians have been distinguished in the past. Let me recall the classic monograph of Th Simar, *Étude critique sur la formation de la doctrine des races . . .* (Bruxelles, 1922), where the author traced the rise of the racial doctrine in the 18th and 19th centuries. Salmon's approach is similarly historical, and although he deals with his subject in much more general terms than Simar, he covers the whole field of the subject from antiquity to modern times, including an account of contemporary racism. A critical appraisal of racial doctrine and prejudice is found in the fourth and last chapter, where the author discusses several points of theory in relation to the actual incidences of racial prejudice.

Salmon rightly insists on the confusion between cultural differences, which are differences of degree, and racial ones, which are purely biological Racial prejudice is, in the last analysis, the product of cultural differentiation, and ethnocentrism is reduced to the incapacity of comprehending other men's cultures The merit of Salmon's work is to show in clear terms the importance of the cultural factor in the formation of racist consciousness This might provide the clue for explaining the logical contradiction between the two terms, viz. the theoretical assertion that race is a myth (implying a judgement of value) and the objective reality of racial conflict (a judgement of reality) In fact the racial (and ethnic) conflict is logically distinct from the scientific ascertainments about racial (and ethnic) facts, and for its treatment one should seek in the first place the objective causes of it consisting in real inequalities couched in cultural differentiation. The inductions of anthropologists are not for this reason invalidated, their normative implications being useful in the definition of the ends of any policy aspiring to a solution of the racial question.

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JORDAN, WINTHROP D. *The White Man's burden: historical origins of racism in the United States.* xvi, 229 pp. New York: Oxford Univ. Press, 1974. £5·25 (cloth), £1·20 (paper)

In 1968 Jordan published a magnificent 650 page volume entitled *White over black: American attitudes toward the Negro, 1550–1812* The present work is an abbreviation of it. It shows how Englishmen brought with them to Virginia unfavourable impressions of black men. By the end of the seventeenth century in all the colonies of the English empire there was racial slavery of a kind that would have been unfamiliar to an Elizabethan Englishman but not to his nineteenth-century successor. How did the initial sentiments get translated into the later institution? Jordan concludes that the evidence is insufficient to explain how and why between 1619 and 1680 blacks came to be regarded so differently from white indentured labourers. He goes on to discuss the influence on developing racial attitudes of early American society and of eighteenth-century theories of racial difference. An examination of the thought of Thomas Jefferson brings several of these themes together.

Jordan's interpretation of the 'unthinking decision' of the seventeenth century rests heavily on psychological speculations. Though he states that 'it may be taken as given that there would have been no enslavement without economic need' he sees white prejudice as in part the white man's attack on 'the animal within himself'. He pushes 'the American dilemma' back to the country's origin This part of his argument, like the use of the expression 'origins of racism', a term he earlier abjured, is unfortunate as the main effect of his work is to show how racial attitudes have acquired new dimensions in new circumstances We need to disentangle different elements in this complex and pose more specific questions about variations of circumstance instead of assuming unilinear development.

One line of attack—indicated by G. M. Fredrickson—is to contest the suggestion that critical changes occurred in the middle of the seventeenth century In other colonies blacks seem to have been slaves from the outset so that Virginia was temporarily a deviant case. But such an interpretation does not weaken the supposition of psychological causes. This debate is now entering a fascinating phase. As Jordan's studies illuminate one of the great issues of our age, this useful abridgement of his monumental judgement can be warmly recommended to any anthropologist without the inclination to peruse *White over black.*

MICHAEL BANTON
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STACK, CAROL B. *All our kin strategies for survival in a Black community.* xxii, 175 pp, illus., tables, bibliogr. New York: Harper & Row, 1974. $7 95

The publishers present 'White Caroline' as a latterday Mary Kingsley, but the significance of this book is apparent from reviews by the author's mentors. Not only did she go to

live with a black family in a mid-western ghetto, she has actually reported what she experienced as a criticism of received opinions in the U.S.A. What Whyte, Gans and Suttles have done for lower class Italians, Carol Stack is trying to do for the urban blacks. Unfortunately she has not contributed as much to the development of urban sociology. Her analysis of the economics of social network exchanges could have been usefully condensed into one longish article.

The author is primarily concerned with (1) recruitment to kinship networks, (2) the relationship between household composition and residence patterns, and (3) the relationship between reciprocity and poverty. Her observations are reminiscent of research by Young and Willmott in Bethnal Green and R. T. Smith in British Guiana twenty years ago. Despite her criticism of the concept of a matrifocal family, she seems to accept that men are indeed marginal to their procreated families because of their economic insecurity and the operation of the U S. welfare system, and her focus is on women and child-rearing. Men generally aid their family of origin whereas fathers are 'friends of the family'. Similarly, the author's dismissal of a poverty trap is belied by her own evidence Poor blacks are certainly not disorganised and helpless as they mobilise extended kinship networks as a means to survival But any attempt to break out is virtually doomed to failure, because marriages lack the economic basis for support and capital is exhausted by agency requirements and social network obligations. *All our kin* describes the social characteristics of an 'urban subsistence economy', which has many parallels to its rural prototype John Lombardi, who collaborated in the conclusion, could have served the author better by drawing these wide comparative points from her data

This short book is surprisingly rambling and repetitive The substantial section devoted to a coding key for an A F.D.C. survey seems superfluous because the data are largely unused, despite the fact that what is included strongly supports the author's case. If this material had been systematically presented and the case data had been used in other than summary form, we should have had a much more useful and less scrappy account to work from. Given the established tradition of the Chicago School and the published research from urban Africa by Mayer, Pauco, and Mitchell's research team, we should expect something conceptually and methodologically more penetrating and accomplished from urban anthropology in the U.S.A.

DAVID BOSWELL

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RIGBY, ANDREW. *Alternative realities: a study of communes and their members.* 341 pp. London: Routledge & Kegan Paul, 1974. £4·95

Andrew Rigby appears to have written this book with two aims in mind, requiring rather different orientations. The result is an inconsistent mixture which swings from the heights of mystical philosophy to the depths of banal description. One object is clearly sociological: to describe the social world of those who live or try to live in communes, their perceptions of reality and their ways of relating to that perceived reality. In doing this, Rigby adopts the approaches of phenomenology and symbolic interactionism, drawing on the work of, for example, Schutz, Berger and Becker. His other object, it seems, is to erect around this description a treatise or sermon on the correct posture of the individual man faced with the exigencies of contemporary life. At the same time, therefore, the book is firstly a study in sociology, using its frames of reference and concepts, and secondly, a strong argument against those assumptions on which contemporary social structure is thought to be founded, of which, of course, sociology forms a part. The two projects are thus logically contradictory and this ambivalence detracts from the impact of the book.

The philosophical argument is loose and sometimes grandiose and the material on which these arguments are based is often lightweight It is not just that the data are 'soft': they are imprecise and insubstantial. The author does not do justice to the amount of time and effort he must have put into collecting this material by presenting the results of his labour in such a raw and unanalysed form. A more systematic and thoughtful presentation could have been produced without undue distortion.

However, although the book is in these ways unsatisfactory, it is saved by the sensitivity and sympathy with which it is written, providing, at times, vivid images of life in communes, the difficulties experienced by those who attempt this way of living, their hopes and their dreams: these seem, however, so extraordinarily naive that they must surely be doomed to failure. Not that this comment will worry Andrew Rigby who will have already condemned me to the ranks of either the 'curdled idealists' or the selfish cynics who must form, unfortunately, the majority of his audience.

SUSANNE WOOD

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WOOD, MANFRI FREDERICK. *In the life of a Romany gypsy*; edited by Jihna A. Bruno, illustrated by Andrew Young. viii, 130 pp., illus. London: Routledge & Kegan Paul, 1973. £2·50

The author's aims in writing this book are 'to clear up some of the misunderstandings that exist to this day between my people and the Gorgios' (Gorgios being non-Gypsies) and to describe the Romany's traditional way of life. Yet the book has another implicit aim, to capture the remaining vestiges of Romany life and language before they disappear. The book conveys a sadness at the decline of this life with its absorption into industrial-bureaucratic society: in a humorous section on methods of poaching, the author comments that he has not done any recently, 'nowadays I have a shooting licence, an occasional fishing licence and sometimes even get asked to a hunt'. While recalling herbal medicines, he regrets that Gypsies mostly go to G.P.s and chemists now: 'the plants we used to use for medicines might be poisoned by insecticides and other sprays, artificial manures (and) exhausts from cars.'

The book also portrays the ambivalent relationship between Gypsies and the host society While emphasising their separateness, Romany Gypsies are intricately involved with Gorgios, as symbolised in the traditional burial practices· 'the handling and laying out of the corpse was always supposed to be done by people outside the tribe and if possible not by Gypsies but by Gorgios'. However, many customs and values described here distinguish the social situation of the poor and persecuted generally and are not exclusive to Gypsies. Wood's descriptions of unique features of Romany life (e.g. the discussion of taboos and marriage customs), are rather unsatisfactory. Further questioning might produce answers. the author says a list of all taboos would fill a book; sometimes one suspects the knowledge is already lost.

The book is of interest therefore as a document which captures one perspective on the Romany way of life. It is humorous, its anecdotes bear the quality of a 'tale', telling as much of Gypsy values as of past events. And particularly, the final section describing the author's sentiments, living close to nature, have quite simply a poetic beauty: 'the grunt of the badger in the undergrowth .. the buzzing of the bees as they fly from bloom to bloom along a hedgerow . . the feeling of uncertainty.'

SUSANNE WOOD

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DORSON, RICHARD M. (ed.). *Folklore and traditional history*. 118 pp. The Hague, Paris: Mouton, 1973

This collection of essays attempts to confront a serious problem, 'the question of the historical validity and ethnocentric bias in oral history' (p. 7). For both anthropologist and historian, non-written sources of information are increasing in importance. In many societies they are the *only* material with which to reconstruct the past; in others they are the only source for many of the intimate details of ordinary life, for insights into the fundamental yet normally unrecorded assumptions upon which people base their lives. In principle, therefore, the volume can be welcomed. Yet the result is disappointing. The introduction, and first brief essay by Bynum on 'Oral evidence and the historian· problems and methods', say very little. There follow two short and specialised pieces on 'Homer, the Trojan War, and history' by Lord and 'Old Norse epic and historical tradition' by Ciklamini There is then a useful discussion of sources for Fulbe (Mali, Guinea, Senegal) history by Robinson, including sensible advice on the collection and depositing of data (p. 31). The sources are carefully compared and 'argue for close relationships among these three categories of sources and between the oral and written modes in general' (p. 37). Daaku then discusses the oral traditions of the Akan, very broadly interpreted to include the music of the drums, oaths, etc (p. 45). The results are somewhat inconclusive. The most intriguing essay is a well-written and balanced investigation by Klein into the folklore tradition that King Charles XII of Sweden was killed by one of his own soldiers firing a metal button at him. It is a minor incident, but useful evidence that oral traditions do sometimes preserve historical facts which are otherwise unrecorded. Finally there is a long essay by the editor on 'Sources for the traditional history of the Scottish highlands and western islands', which is a discussion of oral material gathered by the School of Scottish Studies, the Dewar Manuscripts and, somewhat surprisingly, the historical novels of John Prebble. There are some pleasant descriptions, but at an analytic level the results are superficial In conclusion, it is possible that specialists in the above fields, particularly in the areas studied by Klein and Robinson, might find some of the discussion of value, but the more general reader will probably feel that the collection should have been published in a folklore journal, rather than as a separate monograph.

ALAN MACFARLANE

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General

BUQUET, LEON. *Démographie* (Coll. Droit Sci. écon.). 186, v pp., illus., maps, tables. Paris: Masson, 1974. F38

After an account of censuses and vital statistics, this book presents a systematic exposition of formal demography (pp. 22–76). The methods developed are then used to analyse population changes in France in the nineteenth and twentieth centuries, and to compare population changes in Italy, the United Kingdom and the United States. The book also includes sections on the distribution of world population, migrations, and the history of demographic ideas and techniques.

The historical sections are uneven. Buquet twice endorses Montesquieu's notion that a depopulated Roman Empire 'succumbed under the weight of the numbers of the barbarians' (pp. 6, 156): in fact, the Empire showed classic symptoms of overpopulation, and, except in Britain and in Gaul north of the Somme, the German conquerors were small ruling minorities The Breslau bills of mortality reached Halley not from Leibniz (p. 77) but from the Huguenot refugee Henri Justel. The condensed paragraph on family planning history conveys the impression that Francis Place (1771–1854) founded 'the association for Birth Control' (p. 169), a term first used by Margaret Sanger in 1914. The account of the history of demographic technique is distorted by the omission of many key figures, including Price (except in a list of names quoted from Malthus), Milne, Süssmilch, and even Graunt.

The social biology of population is inadequately dealt with. Fertility and fecundity are not distinguished, and though Buquet discusses ecological principles he does not appreciate the ecological factors that limit settlement in the humid tropics, for instance in the Amazon basin (p 150). The demography of tribal societies is not covered, and anthropology students approaching social demography will benefit more from Krzywicki's *Primitive society* or the marvellous second edition (1803) of Malthus's *Essay*.

However, Buquet is clearly primarily interested in formal demography, and those not daunted by elementary calculus will find this book a rewarding approach to the subject, with imaginative use of graphical methods. In the historical sections, the development of ideas is related with insight to contemporary movements of population, there is a good account of Malthus's work in both demography and economics, and an extremely interesting study of the demographic work of Buffon, including his ingenious alternative to moving averages for overcoming problems of rounding. The sections on migrations are excellent. Full of useful factual information, lucidly and elegantly written, the book is well worth the attention of those interested in population.

W. M. S. RUSSELL
Reading University

FORGE, ANTHONY (ed.). *Primitive art and society*. xxii, 286 pp., illus. London: Oxford Univ. Press for Wenner-Gren Foundation for Anthropological Research, 1973. £6·50

In the summer of 1967 a conference on Primitive Art and Society was held at Burg Wartenstein under the auspices of the Wenner-Gren Foundation for Anthropological Research. Some fourteen specialists attended, most of them anthropologists, and the present volume contains the papers they presented, three of which were subsequently read at meetings of the Art Panel of this Institute.

As is usual with symposia of this nature held under as broad an umbrella-title as possible, both the subject-matter and the quality of the papers vary considerably. The subjects can be divided very roughly into, on the one hand, studies of art objects or artists, and, on the other hand, studies of the social context of specific areas into which the art fits. In the latter case, primitive art mainly serves as a jumping-off point for a discussion of anthropological practice.

By far the most lively paper is that by Edmund Leach on 'Communication and taboo in primitive art'. Concerned with the cross-cultural appreciation of art, he approaches the problem through a discussion of other forms of ambiguity, including the incest taboo and—very wittily—the symbolism of the Catholic priesthood's headgear. A complementary paper, which includes the analysis of a Balinese painting, is Gregory Bateson's 'Style, grace and information in primitive art', which is already well known from its appearance in the paperback edition of *Steps to an ecology of mind*. Whatever one may think of the ideas put forward (and incidentally Leach refers to the famous figure from Rurutu in the British Museum as a carving of 'the deity Tangaroa' which it almost certainly is not: it appears to represent the deified ancestor A'a), these two papers provoke thought.

In 'In search of meaning in African art', William Fagg returns to one of his favourite hobby-horses, the concept of increase expressed in Nigerian art through the use of the 'exponential curve' found in animal growth —an extraordinary misapplication of D'Arcy Wentworth Thompson's great work *On growth and form*. He also surprises by implying that European art employs no such 'curves of growth'.

In 'Tikopia art and society' Raymond Firth discusses and illustrates a surprisingly wide range of designs of Tikopian headrests, which he considers to be 'minimal art', though not in the contemporary art sense. In 'Style and meaning in Sepik art' Anthony Forge analyses in considerable detail some of the many paintings he 'commissioned' from the Abelam, and a number he illustrates in full colour. Philip Dark, who had been carrying out fieldwork in New Britain, outlines in 'Kilenge Big Man art' the social structure and ethnography of the Kilenge in order to ask a number of questions about art which Leach in part attempted to answer in his paper. Simon Kooijman provides what is really an abstract of his monumental work *Tapa in Polynesia* (Honolulu, 1972).

Denise Paulme discusses 'Adornment and nudity in tropical Africa', taking up a subject she had dealt with at much greater length in her book *Parures africaines* (Paris, 1956). Roy Sieber continues his art historical studies of the very interesting terracotta funerary heads of southern Ghana, extending the corpus and offering the results of recent fieldwork. His paper is aptly entitled 'Art and history in Ghana'. Warren L d'Azevedo once more writes about the Gola in 'Mask makers and myth in western Liberia', in which he describes at length the social situation of the Gola artist and gives a rare account of the women's Sande society. Whereas anthropologists are ever keen to demonstrate the differences between our own society and those of other peoples—and no less so when it comes to questions of art, as this volume testifies—yet it is important to realise that among many of the tribes of Liberia, extending into western Ivory Coast, the position of the artist in society is very similar to that of the European artist. He depends on patrons, he is both treated with disdain and held in high respect, and he is expected to practise a sexual freedom which would be shameful in others.

'Spatial presentation of cosmic order in Walbiri iconography' by Nancy D. Munn is a paper—every conference has at least one—whose inspissated perissology obfuscates its own significativeness. Certainly it is a bit of a let-down to find, after unravelling a particularly tortuous passage, that all it means is that a straight line represents the penis and a circle represents the vagina or the womb. These enlightening interpretations of elementary signs 'intrigued' (his own word) Frederick J. Dockstader, who once worked as a silversmith among the Pueblo Indians. In his paper on 'The role of the individual Indian artist' he remarks that in his experience artists usually create a design for purely decorative purposes and ignore any symbolic meanings.

For anyone concerned with primitive art, this is an essential volume. It not only contains the results of much recent and important fieldwork, but also it serves as an excellent introduction to how anthropologists think about art. And here it is interesting to note that whereas any art historian who ventures into the field of primitive art must—and generally does—acquaint himself with a considerable body of anthropological knowledge, it is rare, at least in this country, to find an anthropologist who has any knowledge of art history, or the vocabulary of art criticism, or more than the most superficial knowledge of European art Many of the questions raised by authors of these papers would not have been raised, or would have been raised at a much deeper level, if they had been familiar with the thought and writings of art historians in Europe and America. Art historians often owe a very great debt to anthropologists. But anthropologists still have a great deal to learn from art historians.

J. B. DONNE

London

BRAVMANN, RENE A. *Islam and tribal art in west Africa* (Afr. Stud. Ser. 11) xiv, 190 pp, illus., bibliogr. Cambridge Univ. Press, 1974. £5

This book falls naturally into two parts. In the first Dr Bravmann attacks the assumption, so widely held by ethnographers and historians alike, that the introduction of Islam into west Africa had a negative and even destructive effect on traditional representational art. In the second, he takes three examples of masking cults on the Ghana-Ivory Coast border in the region of Bondoukou, and examines the extent of Muslim participation in the light of fieldwork carried out in 1966–8.

The first four chapters are largely concerned with a reconsideration of the literature on Muslim attitudes to representational art and on the dissemination of Islam in west Africa by the Mande-speaking peoples, particularly Dyula traders. Evidence is adduced that intrusive Muslim Mande-speaking groups not only countenanced but supported and even adopted traditional cults, with the Do cult of the Bobo and Dafing, and the Simma cult (here described in print for the first time) of the Vagala and Tampolense, given as examples.

The last four chapters deal with three masking traditions—Bedu, Gbain and Do—which are still active today in and around Bondoukou, and in particular the extent to which they are attended and even controlled by Muslims. This is a fragmentation area where Kwa, Gur and Mande languages are all to be found, together with a variety of tribes, among whom

the best known are probably the Brong, Kulango and Nafana Senufo. Islamised Ligbi traders appear to have entered the area in the fifteenth century and settled in the former commercial centre of Begho, just inside the frontier of modern Ghana.

Bravmann found that the Bedu cult, which employs pairs of huge face-masks and whose function appears to be to induce prosperity and avert disaster, was controlled by animists, but that its ceremonies were attended by all members of the community, including Muslims. The Gbain cult searches out witches, and for this purpose it employs masks of the helmet-type with monstrous jaw and backward-sweeping horns. Not only is this cult to be found among both Muslims and animists, but the latter claim that they have adopted it in recent times from their Muslim neighbours. Finally, the Do cult, whose masks resemble the *gpelihe* face masks of the Senufo (indeed two of the illustrations show typical *gpelihe* masks that look like 'tourist' pieces), is completely Muslim The masks perform at public Muslim festivals, especially the end of Ramadhan, and therefore have no significance for animists.

Bravmann maintains that the Bedu cult is of recent origin, and is certainly correct in suggesting that its masks are descended from those of the so-called Sakrabundi cult which was suppressed by the British at the turn of the century (The cult survived, but its masking tradition was destroyed.) As to the Gbain, in 1891 Marcel Monnier, who was a member of the Second Binger Mission, witnessed a witch-hunt in Sapia, a dozen miles from Bondoukou, which appears to have been carried out by a Gbain society. His description of a mask and costume is almost identical with that of another mask he had seen earlier at Yakassé, today over 100 miles south of Bondoukou by road Here were no Muslims, and if the Gbain cult has been spread by Muslims in recent times, nevertheless its origins should probably be sought in one of the many animist witch-hunting cults in the Ivory Coast whose masks are so similar Finally, an unpublished Do mask in the Musée des Arts Africains et Océaniens in Paris, collected in the 1930's, has a well-authenticated provenance from Dabakala, over 100 miles west of Bondoukou. This provides an obvious link with the Senufo *gpelihe* masks, and here it would seem that the mask has been taken completely out of its animist context of initiation rites and absorbed into the festivities of a Muslim community.

Whether he would accept these hypotheses or not, Bravmann has succeeded in making the point that Islam and animism are often much more closely linked in west Africa than is usually recognised. This is an important book, and it will also long serve as a source of reference for those interested in the masks of an area that had previously been almost unstudied.

J. B. DONNE

London

TAYLOR, J. GARTH. *Netsilik Eskimo material culture: the Roald Amundsen collection from King William Island.* 173 pp., illus., map, tables, bibliogr. Oslo: Universitetsforlaget, 1974. Nkr. 45.00

J. Garth Taylor's catalogue of Amundsen's ethnographic collection from the Netsilik Eskimo is a useful, succinct and carefully written description of a large group of artefacts. The author has described about 1,200 objects by first organising them under standard functional categories and then in formal subclasses His lucid descriptions are enhanced through the use of photographic plates, drawings and tables listing salient features and dimensions.

Netsilik Eskimo material culture has previously been described in depth only once (Birket-Smith, Kaj, Ethnographical Collections from the Northwest Passage; *Report of the Fifth Thule Expedition, 1921–24*; Vol VI, No 2, Copenhagen), Taylor's study is by no means a duplication of this. The Amundsen collection was made about fifteen years earlier than the other and was obtained from different sub-groups who had experienced relatively little contact with Europeans As the author notes, these factors may well explain why he has come to conclusions about the cultural affinities of the Netsilik Eskimos which are contradictory to Birket-Smith's.

In recent years the study of ethnographic Eskimo material culture has received less attention than have investigations of social and archaeological problems In this regard, Taylor's study is most interesting and welcome because, in drawing on the excellent archaeological and ethnographic works from the central Arctic, he has altered and added to our knowledge of cultural differentiation within the Netsilik group

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BERLIN, BRENT, DENNIS E. BREEDLOVE & PETER H. RAVEN. *Principles of Tzeltal plant classification an introduction to the botanical ethnography of a Mayan-speaking people of Highland Chiapas.* xxii, 660 pp., illus., maps, plates, bibliogr. New York, London: Academic Press, 1974 £18·95

The title of this work gives an accurate indication of its nature and scope but gives no idea of the three authors' considerable achieve-

ment. An anthropologist and two botanists have together managed to produce a book that does several things and all of them well. Firstly, it represents a substantial contribution to the ethnography of the Mayan people of Highland Chiapas Secondly, it is an indispensable work of reference for anyone of any subject who has a serious interest in that area. Thirdly, it is a pioneering work in the field of ethnobotany and as such it has set a very high standard for future researchers to emulate. Finally, it is methodologically invaluable because the authors have been so explicit about the way in which they have tackled their research

The volume is divided into two parts In Part I the setting, principles and methods are discussed, and in Part 2 detailed descriptions of the flora are given. The book contains a mass of information and the authors, in order to help the reader find his way around, have included not only three indexes but also four appendices whose contents, such as the alphabetical inventory of Tzeltal plant taxa, provide additional guidance. The work is beautifully produced and lavishly illustrated with photographic plates and line drawings. Unfortunately, because of its specialised nature and its almost inevitable restriction to library shelves, the authors, and the publishers, are unlikely ever to receive the general merit they deserve.

P. G Rivière

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Lambert, Wallace E. *Language, psychology, and culture: essays* . selected and introduced by Anwar S Dil. xvi, 362 pp., tables, bibliogr. Stanford Univ. Press, London Oxford Univ. Press, 1972. £5

In the postscript to this selection of his works, Lambert refers to them as 'a running account of my attempts to understand the psychology of language and culture through family experiences'. He and his French wife became interested in the process of bilingualism in individuals by watching their own children growing up There is indeed no single intellectual 'programme' linking Lambert's successive research projects as is clearly the case with the other sociolinguists featured in this Stanford series of monographs. Lambert's interests range widely and he seems to have addressed himself to the solution of particular problems almost on an *ad hoc* basis. Not that this matters for he is able to sniff out suspect hypotheses, put them to rigorous tests and demonstrate in remarkably clear language their defects.

As with other books in this series, the contributions are chronologically listed In this case there seems little justification for this, for Lambert does not claim to have developed any particular line over the years. An appropriate non-chronological division would have been between those papers which deal with bilingualism and those which are concerned with more general issues. The former make up the majority and often overlap but together deal with assessing methods for measuring which of a bilingual's languages are 'dominant', finding out how bilingualism affects learning, intelligence, and specific cognitive responses, and the development of prejudices and stereotypes with reference to members of one or other of the two language/ethnic groups. Much of this is of direct interest to psychologists interested in language but Lambert does give insights into conventional anthropological work, as for instance in his analysis of the personality and identity conflicts and pressures to which children of mixed parentage (English/French) are subject. And in a number of other essays, he incidentally outlines the social implications of bilingualism for mixed ethnic situations: an implication worth following up would be to ask in what respects does bilingualism offer the possibility of greater manipulation of roles between the two groups, and paradoxically to what extent is dual ethnic membership a constraining rather than liberating factor?

Chapters 4, 9, 11, 14, 15, and 21 are not specifically concerned with bilingualism. The first of these is a superb and very anthropologically relevant analysis of the content of propaganda in different French newspapers spanning the political spectrum from left to right. The focus is on their reports of a 'domestic' murder in 1949 which involves characters whose ethnic origins, political affiliations, and life-styles are sufficiently varied and ambiguous for either right- or left-wing sympathies to be expressed for them at the ensuing trial. Lambert uses Pareto's residue-derivation classification, i e. how latent sentiments can be articulated under situations of opposition, and it would be interesting to see whether other methods of content analysis could achieve the same results Chapter fourteen on the use of *tu* and *vous* forms of address in French Canada takes off from the famous Brown and Gilman paper and suggests divergences from the situation in France where the rules for pronominal address are more complex For its sheer interest, it is also worth reading chapter eleven outlining psychological approaches to the study of language. This is by no means a familiar post-Whorfian eulogy on psycholinguistics as the answer to problems of interpersonal language use. It soberly considers aptitudinal skill and neurophysiological fac-

tors behind language learning abilities with reference to the ideas of Skinner and others.

Lambert's clarity of thought and expression enables the non-psychologist to follow his views which have long been incorporated or considered by those sociolinguists who are mainly interested in straight correlations between sociological and linguistic features. Lambert's material does seem hard to fit into the revived anthropological interest in language and thought. But, because it rests on 'hard' attitudinal and sociodemographic facts acquired by systematic surveying, it may have value as an empirical restraint on outright fantasy.

DAVID PARKIN

SOAS, London

COLE, MICHAEL *et al. The cultural context of learning and thinking: an exploration in experimental anthropology.* xx, 304 pp., illus., maps, tables, bibliogr. London: Tavistock, in association with Methuen, 1974. £1·50 (paperback)

This book is a reprint of a cross-disciplinary study first published in 1971 by a group of Americans interested in education. It summarises their field research among children of the Kpelle tribes of north-central Liberia, where they sought for evidence of specific relationships between 'culture' and 'cognition'; in particular how widely different social environments and strategies affect learning in American-style schools. Written for the 'interested laymen as well as specialists in different disciplines' much technical material has been relegated to about fifty pages of appendices. A very full bibliography includes reference to anthropologists from Lévi-Strauss to Lévy-Bruhl, linguists, educationists and to a host of experimental psychologists, many of whom can be but names to this reviewer. In a Foreword, George Miller warmly welcomes this attempt to reformulate questions about comparative intelligence levels, so as to open up lines of communication between these disciplines. Until recently, it could fairly be said that psychologists have not asked 'the right questions' about the mental processes of people from non-western or traditional cultures. As a result, their answers have usually endorsed ethno-centric conceptions about the superior reasoning powers of 'western' over so-called 'primitive' man.

The present authors are highly critical of such previous work. They start from the assumption that the people they are working with are most likely to 'behave reasonably' in context; when their behaviour 'appears unreasonable, it is to ourselves, our procedure and our experimental tasks that we should turn for an explanation'. The book is thus an attempt to explain, both the tools used for making comparative measurements of intellectual development for Kpelle children in, and out of, school, and also the cultural reasons for observed disparities in performance.

It is a pity that we are given no background information by which to identify either the four authors, or their four associates, who may well be native Liberians. Nor are we told how long the project took, how many of the team spoke Kpelle, where or how they lived, nor how their experimental work was perceived by the local population. Such an absence of identification and contextualisation of field research is a grave handicap in a book which is aimed at a mixed audience.

The search for measurable categories of thought, in controlled experimental situations, both attracts and repels traditional social anthropologists, is it pursuit of a star of wisdom or a mere will-o-the-wisp of the imagination? The present authors are candid about the relative lack of success in many of their experimental techniques, some too simple, some too sophisticated. They are equally refreshing about the policy lessons to be learned from their attempts. The implications of 'different but equal' are hard to assimilate fully, and are rarely taken seriously by westerners making cross-cultural studies in this field.

Through much technical detail, one message comes through clearly: we can never make simple comparative assessments of 'intelligence' out of social context, even the identification of intellectual learning is very complicated, and much more ethnographic study of how children do learn complicated skills in traditional, non-literate societies is badly needed. Whether this knowledge will prove as useful when applied to improving the American black child's educational chances—as the authors hope—is more open to doubt. But it is still of great theoretical interest.

ROSEMARY FIRTH

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MILGRAM, STANLEY. *Obedience to authority: an experimental view.* xvi, 224 pp., illus., tables, bibliogr. London: Tavistock, 1974. £2·50

This social psychological study of obedience is based upon laboratory data obtained from eighteen variants of a basic experimental design, ostensibly, an experimenter invites two subjects, one designated 'teacher', the other 'learner', to participate in a learning experiment. The 'teacher' is told to administer an electric shock whenever the

'learner' errs and to increase the voltage after each error, by increments, up to 450 volts. In fact, the experimenter and the learner are both actors who give standardised performances and no shocks are administered The object is to determine at what point the naive subject, the 'teacher', will react to the protests of the learner and disobey the experimenter. The eighteen variations investigate such matters as, feed-back, proximity, divided authority, inversion of roles, peer group support, and the effect of venue 636 subjects of diverse backgrounds participated, recruited by press advertisements and a sampling procedure. Whilst most subjects manifested signs of considerable tension, a high proportion was compliant in all save three experiments In replications of the series outside the U S.A , by other researchers, similar results have been obtained.

The high degree of compliance was unanticipated by Milgram and his colleagues He argues that this was because they had ignored the emergent properties of the experimental situation. Subjects become locked into a situation from which, once committed, very few can extricate themselves Arguing from a cybernetic analogue, Milgram maintains that when individuals operate autonomously, internalised norms serve as an inhibiting mechanism to prevent actions which harm others In hierarchies, however, individuals pass into an agentic state in which this inhibiting mechanism is by-passed and the individual cedes responsibility for his actions to an authority In this state, individuals may obey commands which involve harm to others without experiencing guilt or remorse. Given this, and the devastation which can result under modern technology from a single act of obedience, Milgram expresses extreme pessimism for human survival.

Whilst Milgram confronts the general problem of obedience his analysis focuses particularly on voluntary obedience to malevolent authority It is unfortunate that when extrapolating from his laboratory data, he concentrates on obedience to authority in military and prison contexts—aerial bombardment, Nazi concentration camps, the My Lai atrocity, and so forth He is right to emphasise that quite ordinary men and women, given a particular type of authority structure and a particular definition of the situation, may obey commands which involve harming others. But the authority structures he considers are extreme forms. In other contexts, individuals do not experience such encapsulating and unitary authority structures in which there are severe penalties for disobedience. In everyday contexts, individuals are subject to numerous and diverse authorities, the boundaries of legitimacy may be situationally circumscribed and commands may conflict. Obedience then becomes more problematic and the subject of bargaining and negotiation. Milgram's study, eloquent and thought-provoking though it is, tells us very little of such processes.

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TEGENFELDT, HERMAN G *The Kachin Baptist church of Burma. a century of growth.* xxvi, 512 pp , illus , maps, tables, bibliogr. South Pasadena William Carey Library, 1974. $9 95

The author is an American Baptist Missionary who first visited Kachin country very briefly during the hectic days of spring of 1942 Subsequently, 1945–60, when resident in Myitkyina, he had supervisory responsibility for Baptist Mission activity throughout the northern part of the Kachin area He had personal contacts with all the senior members of the Kachin Baptist Church who were alive at that time as well as with all the American survivors of the pre war mission

Tegenfeldt's interests are narrowly evangelical and the bulk of his history is concerned with the vicissitudes of individual missionaries and with the statistics of conversion. He virtually ignores all recent developments of ethnological, sociological or political-historical significance Forty-four pages out of 500 are devoted to an ethnographic summary based on Hanson (c. 1910) and Leach (c. 1954). A comparable thirty page summary covers formal history from the earliest times down to 1972 The only anthropological innovation is that, at pp 44–5, the author introduces a new and unconvincing explanation for the fact that Christian missionaries have always maintained that the Kachins had an indigenous belief in a High God named Karai Kasang. The author's account of what happened during the 1940–45 period, though partly based on personal experience, is largely inaccurate, though no more so than other published records.

The extensive bibliography includes references to missionary magazines and unpublished missionary archives which have not been listed in earlier Kachin bibliographies, but it does not appear that they contain anything of great anthropological significance

I find it astonishing, but also very sad, that the blinkered devotion which inspired the missionary contributors to the *Evangelical Magazine* around 1800 should reappear in entirely unmodified form in the work of an American author writing in 1974, but so it is.

EDMUND LEACH

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BOOKS RECEIVED FOR REVIEW IN *MAN* OCTOBER–DECEMBER 1974

ABARBANEL, JAY S. *The co-operative farmer and the welfare state economic change in an Israeli moshav;* with a foreword by Max Gluckman xvi, 236 pp, illus, tables, bibliogr. Manchester Univ. Press, 1974. £4·50

AGRAWAL, D. P. & A GHOSH (eds). *Radiocarbon and Indian archaeology.* xxiv, 526 pp, illus, plates, maps, tables, bibliogrs. Bombay: Tata Institute of Fundamental Research, 1973 £4

APPELL, G N. *Basic issues in the dilemmas and ethical conflicts in anthropological inquiry* (Module 19). 28 pp, bibliogr. New York MSS Modular Publications, 1974 95c

ARONOFF, MYRON J *Frontiertown. the politics of community building in Israel* xiv, 313 pp, tables, bibliogr Manchester. Univ Press, Jerusalem. Academic Press, 1974. £4·95

BALLONOFF, PAUL A. (ed) *Genealogical mathematics,* proceedings of the MSSB Conference . 1974 . x, 311 pp, illus, tables, bibliogr Paris, The Hague. Mouton, 1974 $15

BASEL MUSEUM FUR VOLKERKUNDE UND SCHWEIZERERISCHES MUSEUM FUR VOLKSKUNDE *Cultura populară română* . Sonderausstellung 1973/74. 62 pp, 34 plates. Basel Museum fur Volkerkunde, 1973

BAUDEZ, CLAUDE F. & PIERRE BECQUELIN. *Archéologie de Los Naranjos, Honduras* (Etudes mésoaméricaines 2) 438 pp, illus, plates, tables, bibliogr. Mexico: Mission Archéologique et Ethnologique Française au Mexique, 1973. $25

BEIDELMAN, T. O. *A bibliography of the writings of E E. Evans-Pritchard,* vi, 26 pp London: Tavistock, 1974 £2 25

BERGER, NAN *Rights a handbook for people under age* 60 pp., illus, Harmondsworth Penguin Education, 1974 60p

BETEILLE, ANDRE *Studies in agrarian social structure* viii, 206 pp. Delhi: Oxford Univ Press, 1974. £2·55

BLACK, CRAIG C. *History and prehistory of the Lubbock Lake site* (Mus. J 15) 160 pp., illus, maps, tables, bibliogr Lubbock, Texas: West Texas Museum Association, Texas Tech. University, 1974

CAHIERS ZAÏROIS D'ÉTUDES POLITIQUES ET SOCIALES Vol. 1. 191 pp., tables Lubumbashi Presses Universitaires de Zaire, 1973. $4

CAMPBELL, JOSEPH. *The masks of God creative mythology.* xx, 730 pp., illus. London Souvenir Press, 1974. £4·50 (cloth), £2·25 (paper)

CAMPBELL, JOSEPH. *The masks of God· occidental mythology* xii, 564 pp., illus., London Souvenir Press, 1974. £4 (cloth), £2 (paper)

CAUVIN, JACQUES. *Religions néolithiques de Syro-Palestine* (Publ Cent Tech Ecol. Préhist 1) 140 pp, illus, map, bibliogr Paris Libraire d' Amérique et d'Orient, Jean Maisonneuve, 1972. F36

CENTRAL STATISTICAL OFFICE. *Facts in focus,* second edition. 245 pp, table Harmondsworth Penguin in association with H M S O., 1974. 60p

CHIVERS, DAVID J. H *The Siamang in Malaya a field study of a primate in a tropical rain forest* (Contrib Primat 4) xiv, 335 pp, illus, plates, maps, tables, bibliogrs. Basel· Karger, 1974. Basel: Karger, 1974 £25

CIPOLLA, CARLO M. *The economic history of world population;* sixth edition. 154 pp, maps, tables, bibliogr Harmondsworth Penguin, 1974. 45p

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Made and printed in Great Britain by William Clowes & Sons, Limited, London, Beccles and Colchester

MAN

THE JOURNAL OF THE ROYAL ANTHROPOLOGICAL INSTITUTE

NEW SERIES, VOL. 10, NO. 2 JUNE 1975

MAN

CONTENTS

INDEX OF BOOK REVIEWS

AFRICA IN PREHISTORY: PERIPHERAL OR PARAMOUNT?*

J. DESMOND CLARK
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Concepts of the evolutionary history of man the tool-maker are now being radically revised by the unique and abundant fossil hominid remains, many of them radiometrically dated, at present being found in association with stone artefacts, food waste and other features on undisturbed living sites in particular at localities in the Rift Valley, together with behavioural studies of the African non-human primates It is now also possible to review and to appreciate better the extent of Africa's contribution, especially that of the tropical savanna lands, to human biological development and cultural and technological progress throughout Quaternary times Increasing numbers of radiocarbon dates now show that, far from Africa's having been the cultural backwater during the later Pleistocene that earlier notions had suggested, certain major technological advances appear to have been initiated there.

I

I have chosen to speak of Africa firstly because this is where my own research interests have long been centred, but also because of the increasing amount of tremendously important evidence it is yielding that bears directly upon our own biological and cultural origins and because of the way in which prehistoric studies are extending, enlightening and dignifying the history of Africa's peoples. There is a great need today to stand back, as it were, and to consider this record of our past in the light of the longterm viewpoint that the radiometric chronology and recent prehistoric studies are now making possible If we do this, we cannot fail to achieve a more realistic and balanced appreciation of the role played by the huge continental land mass of Africa in the history of mankind.

From this long-term standpoint we can see that about the first two-thirds of that record have been found solely in Africa. Indeed, the researches of the last few years have produced a large, and still growing, body of evidence strongly suggesting that, not only was it in Africa that the human race first began, but that just about every significant biological and cultural advance—at least for the first 2½ million years—took place in Africa. As an example, the earliest undoubted evidence for intentional tool-making anywhere in the world comes from East Africa and is dated between 2·6 and 2·0 m.y. ago. Up to now, the earliest unequivocal evidence from *outside* Africa dates to approximately 700,000 years ago from the site of 'Ubeidiya in the Jordan Rift (Isaac 1969; Isaac & Curtis 1974).

The sediments of the East African Rift system have preserved, relatively undisturbed, a unique record of the living places, artefacts and associated remains, including also the hominids, that now make it possible to reconstruct the *behaviour*

*The Huxley Memorial Lecture 1974, delivered on 7 November at the London School of Economics & Political Science.

Man (N.S.) 10, 175–198.

of our earliest ancestors. The radiometric *time-scale* now gives us the age of much of this material and makes possible meaningful correlation between sites and understanding of how long it took for each biological and cultural development to come about. Now also it is possible to see something of the innovative role of Africa during the later Pleistocene. In addition, many of the methods and techniques of recovery and recording employed in the discipline of Pleistocene prehistory today stem directly from pioneer work carried out in Africa. Studies of the African primates have made a major contribution to knowledge of early hominid evolution and behaviour; pioneer work in ethno-archaeology in widely separate parts of the continent shows the potential for yet another approach whereby life ways and socio-economic patterns can be recognised and reconstructed, at least for the more recent past.

That such evidence should come from Africa is perhaps shocking to those who still retain the belief, with which many of us grew up, that the prehistory of Europe is emphatically the prehistory of humanity. When prehistory as a discipline first began it was in western Europe at a time when there were no techniques for dating any of the finds other than geologically and relatively, nor was it possible to make reliable long-range correlations between Europe and the other continents. To a large extent this explains why, after the first prehistorians had succeeded in convincing the scientific world of the antiquity of man the tool-maker in Europe, and when Acheulian handaxes and cleavers then began to be found in South Africa in surprising abundance, these same prehistorians decided they could not possibly be as old as those from the river terraces of Europe—besides there were too many of them!

Darwin's belief that man might have originated in the African tropics did not immediately stimulate anthropologists to investigate there; partly perhaps, due to the emotional impact on nineteenth-century Europe of his doctrine of natural selection and evolutionary change and also, perhaps, due to the traditional conviction—still not quite dead—that better fifty years of Europe than a cycle of Cathay! Undoubtedly also the sheer difficulties of field research in tropical Africa, which even today have not been entirely overcome, must have had a daunting effect on would-be African investigators. Darwin pointed out, as did Huxley, that the continent of Africa contained apes with a particularly close relationship to man. As Huxley (1863: 143–4) said, 'whatever system of organs be studied, the comparison of their modification in the ape series leads to one and the same result, that the structural differences which separate Man from the gorilla and the chimpanzee are not so great as those which separate the gorilla from the lower apes'. Recent studies of free-ranging chimpanzee and gorilla groups, when assessed in conjunction with the evidence from the prehistoric living floors, are providing a major source of insight into the behaviour and possible relationships of early hominid species (Schaller 1965; Goodall 1971; Fossey 1972; 1974).

For example, the way in which the feeding habits of chimpanzees and gorillas are complementary—the former feeding mostly in trees off fruits, the latter chiefly on the ground using the plant foods of the forest floor—shows how different uses made of the resources could have permitted, and even favoured, a symbiotic relationship in the savanna between different early hominid forms. Moreover, although the territories of chimpanzee and gorilla groups may overlap and each

may make use of the same localities, they do not come into conflict since they use these at different seasons or at different times of the day (Jones & Sabater Pi 1971). Such a relationship seems most likely for the gracile and robust Australopithecines, or for the latter and *Homo*, the fossil remains of which occur together in the same environmental niches until early man's tool-making abilities—permitting extended use of resources—eliminated less efficient competition like that of the robust Australopithecine.

Studies of the social behaviour and organisation of chimpanzee groups are beginning to provide a most valuable model for understanding early hominid groupings and this has infinitely more meaning than the earlier limited conception of the 'armed aggressor'. Chimpanzee groups, though organised in a flexible manner, are now seen to be more tightly structured than earlier studies suggested and dominance plays a significant role. Of particular interest is the mother-infant dependence and the interaction of individual males and females that revolves round this and which has its parallel in the home bases of the early hominids (Goodall 1965; Washburn 1973). In this connexion also, it is of interest that human infants, unlike the young of carnivores, cannot go for many hours without food while their mothers hunt, but are nursed on the pattern of vegetarians (Washburn, personal communication).

The group co-operation of chimpanzee males for hunting small mammals shows how the much more efficient hunting and meat-procuring behaviour of the Plio-Pleistocene hominids might have arisen; the increasing amount of evidence for sharing of meat shows how important—indeed essential—this must have been in the hominid pattern (Bygott 1972). While chimpanzees successfully tear small animal carcases apart, it is probably impossible for large carcases to be dismembered without a sharp tool which draws attention to the inter-relationship for early hominids of hunting, food-sharing and stone-tool-making. In particular, the chimpanzee's tool-using and tool-making behaviour shows a range of circumstances in which tools are employed, providing a base for estimating the increased variability and range of early hominid tool-use and manufacture (Goodall 1964; McGrew & Tutin 1973; Sabater Pi 1974; Kortlandt & Kooij 1963).

In the same way, studies of the behaviour of the large carnivores in the East African savanna—lion, leopard, cheetah, hyaena and wild dog—also make it possible to suggest the niche that a diurnal early hominid predator may have occupied, as well as what the pattern of group movements might have been, and the ways in which communication could have been maintained for protection, as also for successful hunting and scavenging (Schaller 1972; Van Lawick-Goodall, H. & J. 1970; Kruuk 1972).

A most significant contribution to better understanding of the behavioural adaptations undergone by the proto- and earlier hominids in the evolutionary process is also being made by studies of the anatomy and molecular biology of the African apes. The new evidence from molecular biology, DNA and immunological distance all bear out Huxley's observation that there is indeed a very close relationship between man and the chimpanzee (Washburn 1973). Using the new data, Sarich and Wilson have suggested that the separation of the hominid and pongid lineages took place between 5 and 3 m.y. ago (Sarich 1968; 1971). However, at the present time, the oldest dated Australopithecine fossil is the jaw frag-

ment from the beginning of the Pliocene at Lothagam, Lake Rudolf, in the East African Rift. On the known age of the associated faunal assemblage, this is dated to between 5 and 5½ m.y. ago (Patterson *et al.* 1970), so that it would appear that the Wilson-Sarich clock is a little fast. But, whatever we may think about this suggested new time scale or whether we prefer the more traditional long scale advocated by the palaeontologists (Pilbeam 1972), it is very clear that the separation took place a considerably shorter time ago than did that between the chimpanzee and the Old World monkeys. There can, then, be no doubt that the Hominidae and the African apes are both closely related, that the separation took place at no very remote time in the geological past and that our anatomical and behavioural similarities are due to common ancestry and not to parallel evolution. We may thus—perhaps not ungratefully—conclude with Beaumarchais that 'All there is to distinguish us from the other animals is drinking when we are not thirsty and making love at all seasons.'

It was in 1924 that the search for fossil remains of man's early ancestors really began in Africa when the first man-ape skull was found at Taung in the northern Cape and assigned to a new genus—*Australopithecus*—by Professor Raymond Dart (1925). The record of this and later discoveries in both south and east Africa is well known. Now, in South Africa, several hundred specimens of each of the two species of *Australopithecus*—the gracile *africanus* and *robustus*—have been recovered from five sites and the number and range of skeletal parts is being added to each year (Tobias 1973). Although it was not until 1959 that Louis and Mary Leakey found the first significant East African hominid fossil in the Olduvai Gorge, the richness of the subsequent discoveries at this unique site, at sites in the Lake Rudolf and Omo basins and elsewhere in East Africa, continues By 1971, Olduvai Gorge alone had produced remains of 34 fossil hominids (M. D. Leakey 1971*a*; 225–34), while, up to the present, 84 localities in the Omo have produced some 211 hominid fossils (Howell 1972 and personal communication) and the sites in East Rudolf have yielded over 100 specimens, some in a remarkable state of preservation and completeness (R. E. F. Leakey 1974). Today there is sufficient cranial and post-cranial material to permit many comparative studies of body parts and motor patterns, to give indications of the extent of variability that can be expected within a population, to estimate the dependency period of juveniles on the adults (Mann 1975) and even the gestation period or the size of the young Australopithecine at birth (Frazer 1973; Leutenegger 1972).

II

Why, it may be asked, does this evidence appear to have been confined to Africa? Within the last few years, the several new methods of radio- and isometric dating, the palaeo-magnetic reversal time-scale and the refinement of other dating methods have shown that the hominid lineage derives from a tropical African ancestor as far back as the early Miocene, some 18–20 m.y. ago (Bishop 1972; Brown 1972; Curtis & Hay 1972; Fitch *et al.* 1974). It would appear that at this period, when Africa, Europe and Asia were all connected, certain land mammals, including hominids, were present throughout the area and that about 12·0 m.y. ago, a major change in climate confined these hominids to what are now

the tropical zones (Van Couvering 1972; Cooke 1972). At first sight then, it would appear that evidence similar to that from East Africa should also be found in Asia, since the Dryopithecine apes were present there as well as the controversial *Ramapithecus*, whose hominid status still remains in doubt pending the recovery of more complete cranial and post-cranial fossil material (Pilbeam 1972*b*: 91–9; Washburn 1967). It could be argued that accident of discovery has over-emphasised the part played by the African tropics but if, as is generally believed, the genetic changes that led to *Homo* took place only once, then the evidence as we know it is very compelling that these changes came about in Africa. If they were not confined to Africa, then it is necessary to suppose that interchange was able to take place and that the gene pool extended over both continents at this early time. If this was so, it would also imply that the Pliocene hominids were already adapted to living in a wide range of environmental habitats. But, if the Mediterranean filled about

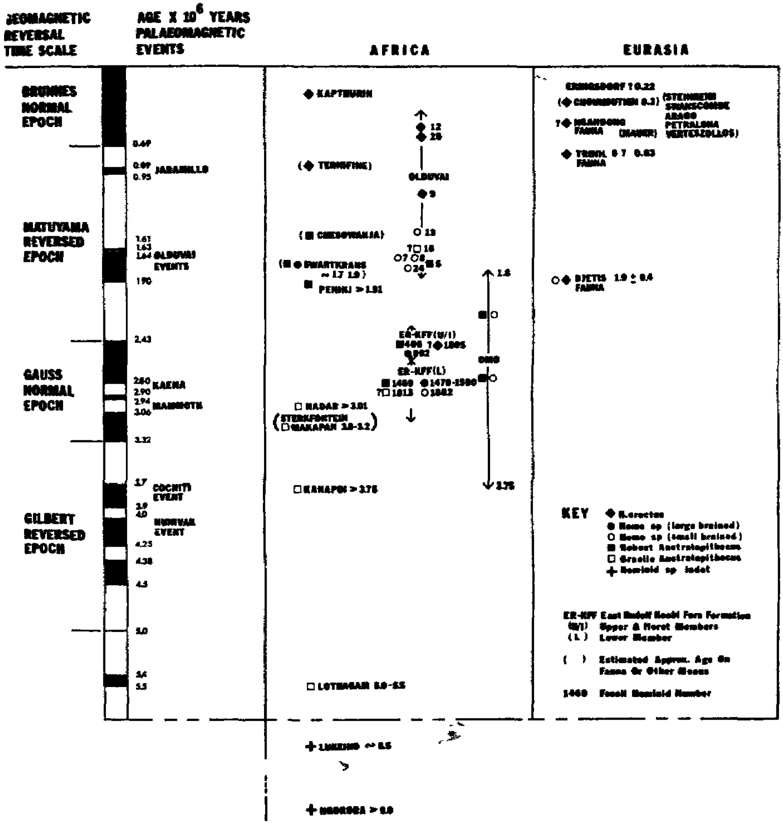


FIGURE 1. Radiometric and palaeo-magnetic chronology for Fossil Hominids in the Plio-Pleistocene time range in Africa and Eurasia. Only the more significant fossils have been shown from the East African sites. Partly based on Appendix 1A Chart illustrating ranges of uncertainty regarding the age of selected hominid fossils ~2.0 to ·01 million years (Butzer & Isaac (eds) in press).

5·5 m.y. ago following the late Miocene desiccation (Hsü *et al.* 1973) and the land bridge at the lower end of the Red Sea was cut at this time also, as recent work indicates (Gurdler & Styles 1974), then communication between Africa and Eurasia would have been significantly reduced and, without it, therefore, the likelihood of such an extended gene pool at that time also diminishes. Again, also, a dual or a Eurasian origin for the hominid lineage seems less likely in view of the wealth of cultural evidence associated with the earliest tool-makers in Africa and the apparent absence of any such artefacts in Eurasia.

At this point—(see fig. 4)—I would like to emphasise the significance of the overall size of the continent and the immense ecological variety to be found in Africa. The vast expanse of the relatively open savanna country—some 8,000 miles from Senegal to the Cape—with its comparatively favourable climate, could clearly have supported a much larger hominid population than Europe, even during the less harsh climatic episodes During a Pleistocene glacial advance more than half of the world, where man could live, was in what is now Africa, and Europe in fact, for most of the Pleistocene, must have been a small, cold, peripheral area. By contrast, the African tropics constituted one of the richest biomes in the world, and the great variety in the plant and animal communities, particularly in the forest/savanna fringes, with the many opportunities these gave for experimentation and the choice of a wide range of behavioural patterns, would have provided a most important stimulus to the evolution of the hominid lineage. The increased use made of meat protein by the savanna groups can be seen as a crucial component in a changing adaptive complex which included tool-making, food-sharing and bipedal locomotion. The picture will become clearer when more fossil evidence becomes available from late Miocene and Pliocene sediments and already teeth showing the hominid pattern have been recovered by W. W. Bishop and his team from the Ngorora Formation (12–9 m.y. old) (Bishop 1972: 227; Bishop & Chapman 1970) and from the Lukeino Beds (*c.* 6·5 m.y. old) (Bishop, personal communication) in Baringo, northern Kenya, while another international team led by M. Taieb and D. Johannson, have recently announced the discovery of further more complete and highly significant finds of gracile and robust hominids in the 3–4 m.y. time range from the Afar Rift in Ethiopia (Press release by the International Afar Research Expedition, November 1974 Addis Ababa).

Of fundamental importance is the ability to place all this material in chronological order—(see figs. 1 and 2). The chronology that has been developed by various radiometric/isometric methods (Bishop 1972: 219–46, Brown 1972; Curtis & Hay 1972; Fitch *et al.* 1974) checked against the palaeo-magnetic reversal time-scale (Cooke 1973; Brock & Isaac 1974; Isaac & Curtis 1974; Grommé & Hay 1971) and applied so successfully to the East African record, is of the utmost significance, since it shows for the first time, not only the period when the major evolutionary developments took place, but the tremendously long stretch at the beginning of tool-making—approximately 1·5 m.y.—during which only very, very gradual changes are apparent in the industries and behaviour patterns.

This time-scale shows that if *Ramapithecus*—dated at 14–12 m.y. ago (or ~10 m.y. on the Indian evidence)—is, indeed, a hominid, then something like from 9–5 m.y. were necessary for the evolution of *Australopithecus*. It also shows that the Pliocene period lasted only about 3·0 m.y.—not very much longer than the

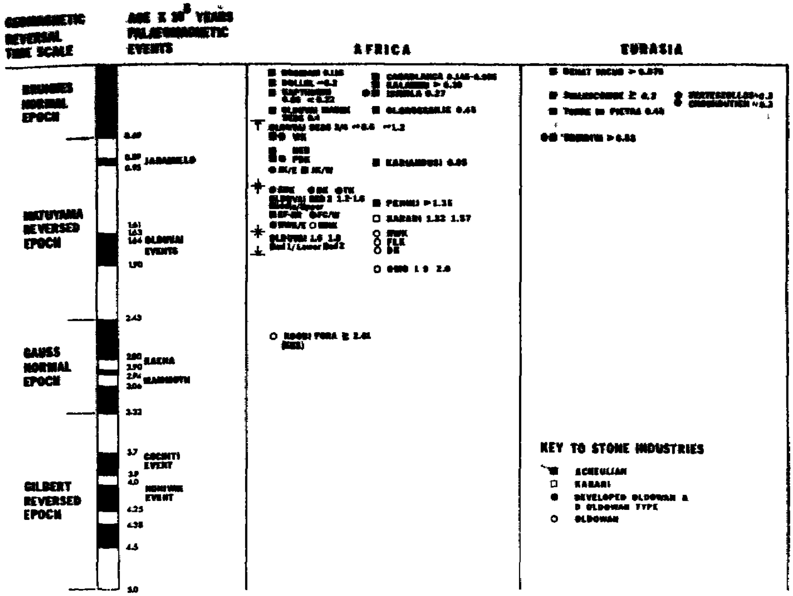


FIGURE 2. Radiometric and palaeo-magnetic chronology for stone industries in the earlier Pleistocene time range in Africa and Eurasia Only the main assemblages from Olduvai have been listed. Partly based on Appendix IB Chart illustrating range of uncertainty regarding the ages of selected archaeological sites and industries ~2·0–01 million years (Butzer & Isaac in press).

Pleistocene.[1] It can now be seen that gracile and robust forms of Australopithecine were both present around 3·0 m y. ago in East Africa (Howell 1972, R. E. F. Leakey 1971; 1974) as in South Africa, if the faunas are correlated correctly (Tobias 1973*b*; Maglio 1972; 1973; Vrba 1974). And, if the K/Ar dating for the lower member of the Koobi Fora Formation in East Africa is confirmed, a large-brained *Homo* form was already present at this time also. If, however, as faunal calibration with the assemblage sequence at Omo and other East African localities may suggest, the age is nearer 2·0 m.y., then this slender, larger-brained hominid—typified by the now well-known KNM-ER 1470 cranium and femurs (R. E. F. Leakey 1973)—falls into the time bracket of the smaller-brained *Homo habilis* forms from Bed I at the Olduvai Gorge (L. S. B. Leakey *et al.* 1964; M. D. Leakey *et al.* 1971). Whether, therefore, the evolution that produced the small- and large-brained *Homo* forms took place between 3·0 and 2·0 m.y. ago or before that, still remains to be determined, as also does the relationship of these forms to the slender Australopithecine.

III

Intentionally flaked stone tools make a relatively sudden appearance in the record at 2·0–2·6 m.y. ago and it cannot be doubted that the significance of this for increasing experimental behaviour and thereby renewed radiation and biological

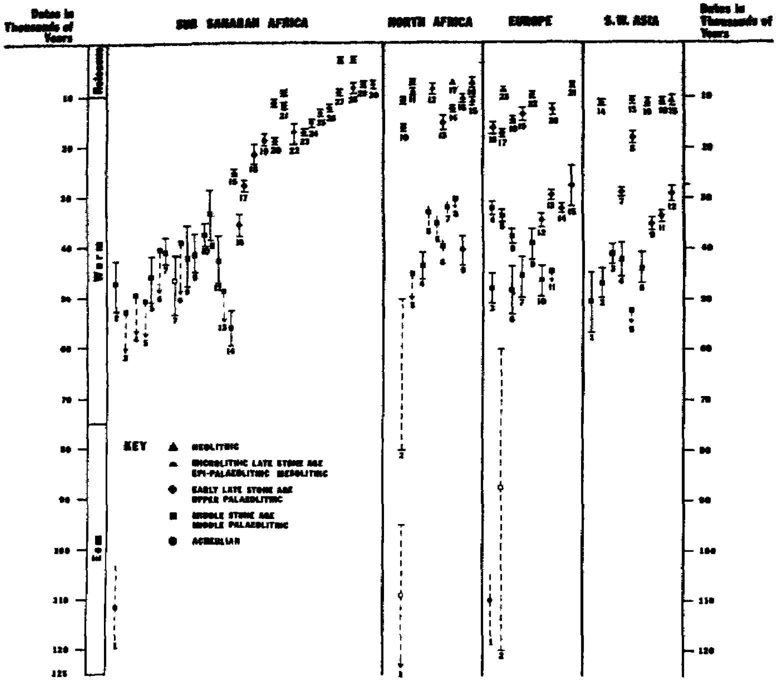


FIGURE 3. Radiometric dates for comparable Later Pleistocene Techno-complexes in Africa and Eurasia. Assemblages selected to show the time range of each complex.

Site Lists, dates and Industrial Associations

A. Sub-Saharan Africa

Acheulian

1. Rooidam, Cape. 115,000±10,000 B.P.

'*Middle Stone Age*'

2. Hout Bay, Cape. 47,100, +2,800, −2,100 B P.
3. Bushman's Rock Shelter, Transvaal >53,000 B.P. (Pietersburg).
4. Florisbad, O F.S. >48,900 B P. (Hagenstad).
5. Montagu Cave, Cape 45,900±2,100; >50,800 B P. (Howieson's Poort).
6. Mufo, Angola. >40,000 B.P. (2 samples) (Lupemban).
7. Kalambo Falls, Zambia. 46,100+3,500, −2,400 (Lupemban).
8. Klassies River Mouth, Cape. >38,000 (?Mossel Bay).
9. Red Cliff, Rhodesia. 41,800±3,000; 40,780±1,800 (Bambata).
10 Zombepata, Rhodesia. 37,290±1,140 (Bambata).
11. Witkrans, Cape. 33,150±2,500 (?Pietersburg).
12. Pomongwe, Rhodesia. 42,200±2,300 (Bambata).
13. Border Cave, Ingwavuma, Natal. >48,250 (Pietersburg).
14. Ndutu Beds, Olduvai Gorge, Tanzania. 56,000±3,500.

Early 'Later Stone Age'

15. Heuningsneskrans Shelter, Natal. 24,640±300.
16. Border Cave, Ingwavuma, Natal. 35,700±1,100.
17. Rose Cottage Cave, Ladybrand, O.F.S. 29,430±520 ('Pre-Wilton').
18. Leopard's Hill Cave, Zambia. 21,550±950 ('Proto-Later Stone Age').
19. Sahonghong, Lesotho. 20,900±270 ('Howieson's Poort').
20. Nelson Bay Cave, Cape. 18,660±110 (Robberg Ind.).
21. Nelson Bay Cave, Cape. 11,950±150 (Albany Ind.).

Late Stone Age (microlithic)

22. Naisiusiu Beds, Olduvai Gorge, Tanzania. 17,550 ± 1,000 ('Kenya Capsian').
23. Leopard's Hill Cave, Zambia 16,715 ± 95 (Nachikufu I).
24. Zombepata Cave, Rhodesia. 15,120 ± 170 ('Wilton').
25. Kamasongolwa, Zambia. 13,300 ± 250 ('Wilton')
26. Lake Nakuru; transgressive phase to ~60m Kenya. 12,140 ± 206 (Blade Ind.).
27. Nelson Bay Cave, Cape 9,080 ± 180; 2,660 ± 150 (Wilton).
28. Wilton Rock Shelter, Cape. 8,260 ± 720 (Wilton).
29. Melkhoutboom, Cape. 7,300 ± 80. (Wilton).
30. Matjes River Rock Shelter, Cape. 7,750 ± 300 (Wilton).

All the above are radiocarbon dates except (14) which is an amino-acid racemisation age (Bada & Protsch 1973).

B North Africa and the Sahara

Acheulian and 'Pre-Mousterian'

1. Harounian beach, Casablanca, Morocco ~145,000–~125,000 (Moroccan Acheulian, Stage 8).
2. Haua Fteah, Cyrenaica. ~80,000–~50,000 (extrapolation dates) ('Pre-Aurignacian').

Middle Palaeolithic (Mousterian/Aterian)

3. Bir Sahara, Egypt > 44,680 (Aterian).
4. Haua Fteah, Cyrenaica, Libya 43,400 ± 1,300 (Levalloiso-Mousterian).
5. Taforalt, Morocco ± 34,550
± 32,350 (*Mousterian/Aterian*)
6. Bou Hadid, Algeria ± 39,900 (Aterian)
7. Berard, Algeria. ± 31,800 (Aterian).
8. Dar-es-Soltan, Morocco > 30,000 (Aterian)

Upper Palaeolithic· Epi-Palaeolithic

9. Hagfet ed Dabba, Cyrenaica, Libya. 40,500 ± 1,600 (Dabban).
10. Haua Fteah, Cyrenaica, Libya. 16,070 ± 100; 10,600 ± 300 (Eastern Oranian).
11. Haua Fteah, Cyrenaica, Libya. 8,400 ± 150, 7000 ± 110 (Libyco-Capsian).
12. El Mekta, Tunisia. 8,400 ± 400 (Capsian).
13. Gebel Silsila, Egypt. 15,200 ± 700; 16,000 ± 800 (Sebekian).
14. Wadi Halfa, Sudan 14,000 ± 280 (Ballanan)
15. Taforalt, Morocco, 12,070 ± 400, 10,800 ± 400 (Ibero-Maurusian).
16. Khanguet el Mouhaad, Tunisia. 7,200 ± 120 (Upper Capsian).
17. Columnata, Algeria. 7,750 ± 300 (Capsian)

All radiocarbon dates except no (1) which is an Th^{230}/U^{234} age.

C. Europe

Acheulian

1. Grotte du Lazaret, France. 110,000 ± 10,000.

Middle Palaeolithic

2. Weimer-Ehringsdorf, Germany. 60,000–120,000 (Mousterian)
3. Gorham's Cave, Gibraltar 47,700 ± 1,500 (Mousterian).
4. Calombo Cave, Italy. 32,000 ± 680 (Mousterian).
5. Velika Pécina, Yugoslavia. 33,850 ± 520 (Mousterian)
6. Grotte aux Ours, France 48,300 ± 230 (Mousterian).
7. Regourdoux, France. 45,500 ± 1,800 (Mousterian).
8. Les Cottes, France. 37,600 ± 700 (Mousterian/Lower Perigordian).
9. Combe Grenal, France 39,000 ± 1,500 (Mousterian)
10. Broion Cave, Italy 46,400 ± 1,500 (Mousterian)
11. Moldova, Ukraine, U.S.S.R. > 44,000 (Mousterian).

Upper Palaeolithic

12. Abri Pataud, France 33,300 ± 760? 34,250 ± 675 (Basal Aurignacian).
13. Abri Pataud, France. 29,300 ± 450, 32,800 ± 450 (Aurignacian)
14. Willendorf, Austria 32,060 ± 250 (Aurignacian).
15. Abri Facteur, Dordogne, France. 27,890 ± 2,000 (Aurignacian).
16. Puits de l'Homme, Lascaux Cave, France. 16,100 ± 500 (?Magdelenian).
17. Arka, Hungary 17,050 ± 350 (Eastern Gravettian).
18. Angles sur l'Anglin, France. 14,160 ± 80 (Magdelenian).
19. Cueva Reclau, Spain. 13,200 ± 600 (Solutrean).

Mesolithic

20. Grotte di Ortucchio, Italy. 12,619± 410
21. Shippea Hill. 7,610± 150 (Tardenoisian).
22. Starr Carr. 9,557±210 (Maglemosian).

Neolithic

23. Nea Nikamedia, Greece. 6,100± 420.

All are radiocarbon dates except (1) dated by Th^{230}/U^{234} and (2) dated by Th^{230}/U. and Pa^{231}.

D. South-West Asia

Middle Palaeolithic

1. Shanidar Cave, Iraq. 50,600± 3,000 (Mousterian).
2. Shanidar Cave, Iraq. 46,900± 1,500 (Mousterian)
3. Tabun Cave, Mt. Carmel, Israel 40,900± 1,000 (Mousterian).
4. Geulah Cave, Mt Carmel, Israel. 42,000± 1,700 (Mousterian).
5. Ras el Kelb, Lebanon > 52,000 (Mousterian).
6. Ksar Akil, Lebanon 43,750± 1,500 (Mousterian).

Upper Palaeolithic

7 Ksar Akil, Lebanon 28,840± 380 (Aurignacian)
8. Ein Ager, Negev 17,510± 290 (Final Upper Palaeolithic).
9. Rasaket. ± 34,600 (Aurignacian).
11. Shanidar, Iraq. 33,300± 1,000 (Baradostian).
12. Shanidar, Iraq. 28,700± 700 (Zarzian).

Mesolithic/early Neolithic

10. Zawi Chemi, Shanidar, Iraq 10,800± 300 (Pre-Pottery Neolithic).
13. Shanidar, Iraq 10,600± 300 (Mesolithic)
14. Jericho, Israel 11,116± 107 (Natufian).
15 Jericho, Israel 10,300± 500 (Pre-Pottery Neolithic).
16. Ganj-i-Dareh, Iran 10,400± 150 (early Neolithic).

variability, was profound, The unique evidence of the associated occupation debris preserved at the earliest living sites and recovered by Mary Leakey at Olduvai (M. D Leakey 1971*a*), Glynn Isaac at East Rudolf (Isaac 1969) and the American and French teams at Omo (Chavaillon 1970; Merrick *et al.* 1973), shows that the oldest stone tools appear at the same time that meat—on the evidence of the food bones—becomes a highly significant part of the diet of early man (Isaac 1971). This Oldowan stone-tool kit exhibits from the beginning a clear understanding of the principles of flake manufacture and of the advantages of selecting different raw materials; but, on the whole, it also shows remarkably little evolution over about 2·0 m.y., through a Developed Oldowan stage, up to the close of the Middle Pleistocene. However, on the evidence of morphology, attributes and the implications of edge damage for providing evidence of intended use, it is possible to detect a minimal number of functionally distinct artefact sets in these Oldowan/Developed Oldowan industries; namely, artefacts with edges designed for chopping, cutting, scraping and piercing as well as bashing equipment—polyhedrons/spheroids—and unmodified stones, or manuports, that were intentionally carried onto the sites. All these tools, unlike those characteristic of the later Acheulian Industrial Complex, are made of raw material no larger than cobble size (M. D. Leakey in press.)

Oldowan sites were always located near water which, apart from the obvious need of the water itself, indicates also a certain reliance on plant foods, the availability of game meat and perhaps shade—suggested by the concentration of the debris (Isaac 1972*a*; Hay 1973). Preferred locations for the home base are close to the mouths of meandering streams draining to lakes, out on the lake plain itself and, in one instance, in a deltaic situation. The way in which certain of the stone tools and

debris from their manufacture are interspersed with the bone waste on these living sites leaves no other conclusion than that these tools are functionally directly associated with the preparation of meat, as also with the manufacture of other tools of wood, horn or other material that have now disappeared, but which played a significant part in obtaining and processing food resources (e.g. M. D. Leakey 1971*a*: figs. 7, 24, 32, 47). The fragmented nature of the animal-bone waste at these living sites, the number of individuals present, the size range—from very small to large animals, including elephants—and the way in which individuals are usually represented by only a limited number of body parts, have interesting behavioural implications. They establish the sites as home bases where the dependent young could be cared for and learn the behaviour necessary for successful adult life and they point to the significance of hunting and so of group organisation and food-sharing—the basis of all human society. The collection of plant foods, which primate behaviour and ethnographic evidence suggest formed the greater part of the diet of tropical hominids, implies that, if these foods were carried into the home base, some form of natural container would have been essential—such as a skin, a piece of bark or a tortoise shell. Also, the stone manuports, although they show no modification, were clearly intended for some purpose, and it would seem probable that they were the weapons with which these 'naturally defenceless' hominids protected themselves and secured a proportion of their meat.

The technically more advanced Acheulian tool-kit makes an apparently sudden appearance in East Africa at dates now known to range from *c.* 1·5–1·4 m.y. ago at the Olduvai Gorge; 1·35 at Peninj in the Natron Basin; and between 1·5 and 1·3 m.y. ago for the related Karari Industry from East Rudolf (Isaac 1972*b*; Isaac & Curtis 1974; Fitch *et al.* 1974; Isaac *et al.* in press). The time-range of this great Industrial or Techno-Complex in East Africa thus stretches from *c.* 1·5 m.y. to *c.* 115,000 years ago for an evolved form in the Cape Province of South Africa (Butzer 1974: 20). The Lower Acheulian in Africa is directly associated with fossil remains ascribed to *Homo erectus* at the Olduvai Gorge (Tobias 1965; M. D. Leakey 1971*b*; Day 1971) and at Ternifine in Algeria (Arambourg 1963). This larger-brained, more robust hominid appears to have begun to replace the earlier forms of *Homo* in Africa some time around 1·5 m.y. ago and what may be an early, smaller-brained *Homo erectus* fossil has been reported recently from East Rudolf in this time range (R. E. F. Leakey 1974 e g. Skull KNM–ER 1805).

Several different kinds of Acheulian site can be recognised and these are contemporaneous with others with tool-kits made in the Developed Oldowan pattern. They occur in varying combinations of artefacts, separately or mixed together on the same site (Clark in press). While the Oldowan pattern appears, as we have seen, to be closely connected with meat preparation (Clark & Haynes 1969; Isaac 1971), the Acheulian tool-kit can, I believe, best be viewed as representing a *new* range of activities, most probably related to new and more efficient ways of exploiting the resources, perhaps associated more with plant products. Settlement patterning now ranges between large, discrete concentrations on multi-purpose sites, to single purpose or special activity contexts with one type of tool or activity set predominating (Howell & Clark 1963; Isaac in press). The sites are found in topographic contexts similar to those of the Oldowan with, in addition, sites in littoral marine localities (Biberson 1961; Clark 1966: 26–35; Gatehouse 1954;

Sampson 1962), round springs (Caton-Thompson 1952; Deacon 1970) and in caves (Mason 1962:159-69; Keller 1973:42-69). Technological efficiency and social interaction would also have been considerably improved and the range of habitats extended by the controlled use, and perhaps the manufacture of fire, attested more than 200,000 years ago on the Acheulian living floors at the Kalambo Falls.[2]

Although throughout the 1·5 m.y. when the Acheulian tool-kits were being made, several significant technological advances can be seen—for example, in the skills involved in the manufacture of bifaces—and although the assemblages show considerable variety—quantitatively, stylistically and idiosyncratically—the overall variability between functionally distinct artefact forms still operates only within a narrow, comparatively restricted range and it is possible to identify on attributes no more than seven or eight different kinds of tool. The Acheulian, and the associated Developed Oldowan, Techno-Complex exhibit, therefore, an overall 'sameness' that continued, with some evolving refinement, throughout the whole of this period and over all the occupied part of the world. This very slow rate of technological development may also reflect an equally slow genetic evolution during the Lower and Middle Pleistocene and, while the group organisation of *Homo erectus* shows him to have been a reasonably efficient hunter and food-gatherer, he was still, it appears, using one general pattern for exploiting only to a very superficial degree, a wide range of food resources in a variety of different habitats (Clark in press).

IV

The radiometric chronology now provides the first reliable means of comparing cultural and biological evidence both within Africa and between that continent and Eurasia. Although the dating evidence from Eurasia is still very incomplete, it nevertheless now appears most probable that it was not before about the beginning of the Middle Pleistocene (now considered to start around the end of the last major palaeo-magnetic reversal period *c.* 700,000 years ago)[3] that hominids—with industries in the Developed Oldowan and Lower Acheulian traditions—make their first appearance in Eurasia (Horowitz *et al.* 1973; Šiedner & Horowitz 1974; Evernden & Curtis 1965; Cherdyntsev *et al.* 1965; Stearns & Thurber 1965). By this time, therefore, man was sufficiently advanced, culturally and biologically, to be able to adapt to a wide range of more open environments and climates from the hot tropics into the temperate and cold peri-glacial regions of the north. Figures 1 and 2 show in greater detail than is possible here the chronometric evidence for the age of the hominid fossils and cultural units on which the dating of this radiation into Eurasia is based.

Two pieces of evidence, however, suggest the possibility of an earlier movement out of Africa, perhaps as early as 2·0 m.y. ago, but both that from Vallonet on the Riviera (Lumley-Woodyear 1969) and the hominid fossils from the Djetis Beds in Java (Jacob 1973) are ambivalent and need to be confirmed by further stratigraphic control and chronometric evidence and, in the case of the Java specimens, by controlled excavation also.

By the close of the Middle Pleistocene, now placed at about 125,000 years ago, at the beginning of the Last Interglacial, biological and cultural developments had

reached yet another threshhold and *Homo erectus* gives place to Neanderthal man and other early forms of *Homo sapiens*. The tool-kits are correspondingly more complex and now, clearly, begin to exhibit variability on a regional basis also.

The evidence from Eurasia shows that, some time between 40 and 35,000 years ago, modern man with his prismatic blade technology was replacing the Neanderthaloid maker of the Mousterian industries. In Africa, however, the basic techniques found with the Mousterian appeared, from earlier work, to have been retained and there were some who thought—by implication—that the genotype persisted also. In the light of the new radiocarbon and other dating evidence, together with that of the new fossil material, however, it now becomes necessary radically to revise this conception of Africa as a region of cultural stagnation and genetic isolation during the Upper Palaeolithic. This period in Africa south of the Sahara is known as the 'Middle Stone Age' and the new radiometric dating which we owe to Dr J. C. Vogel's Pretoria Laboratory (Vogel & Beaumont 1972)—substantiated by an increasing number of results from other laboratories (Klein 1970), now shows that the earliest 'Middle Stone Age' assemblages must be from 80 to 100,000 years old and belong to the early part of the Upper Pleistocene (Butzer 1973). It shows also that, between 30 and 20,000 years ago, the 'Middle Stone Age' tradition was replaced by 'Later Stone Age' industries. Up to a decade ago, the comparatively few radiocarbon results that suggested this earlier date for the 'Middle Stone Age' were considered to be anomalous or contaminated in some way and this view was reinforced by the, apparently, late age of the African 'Later Stone Age' small blade technology which was, therefore, placed in the Holocene on a parity with the Eurasian Mesolithic.

In fig. 3 are set out some of the more important sites and assemblages with their dates. Summarising this evidence, it can be seen that the evolved Acheulian in South Africa, which, in not a few respects, resembles the Mousterian of Acheulian tradition, is at one South African site now dated to *c.* 115,000 years ago[4] while, in North Africa, the final Acheulian stage, again showing similarities with the Mousterian of Acheulian tradition, is dated to between 125 and 95,000 years B.P. (Biberson 1971: 57), and, again, a late Acheulian industry with well-developed Levallois and blade technology from Kapthurin in northern Kenya dates to about or before 200,000 B.P. (Isaac 1972*b*: 417). It would seem, therefore, that by about 100,000 years ago the Acheulian tradition was superseded by that of the 'Middle Stone Age'/Middle Palaeolithic.

Some of the best evidence for the first appearance of the 'Middle Stone Age'/Middle Palaeolithic comes from the caves on the south coast of South Africa (Butzer 1973) and, in the north, from the Mediterranean coast where these industries follow immediately upon or belong to the time when the sea had receded from the 6–8 metre marine transgression beach of the Last Interglacial (McBurney 1967: 45–134; Camps 1974: 23–35). Dates from inland sites also confirm the greater antiquity of the 'Middle Stone Age' and a number of occurrences in caves and from open sites are now shown to have radiocarbon ages ranging between more than 50,000 to about 40,000 years B.P. (Sampson 1974: 175–7, 206–9, 242–4). Other chronometric dating methods confirm this evidence and it is thus now apparent that the 'Middle Stone Age' in sub-Saharan Africa and the Middle Palaeolithic in North Africa and Eurasia are broadly contemporaneous techno-complexes.

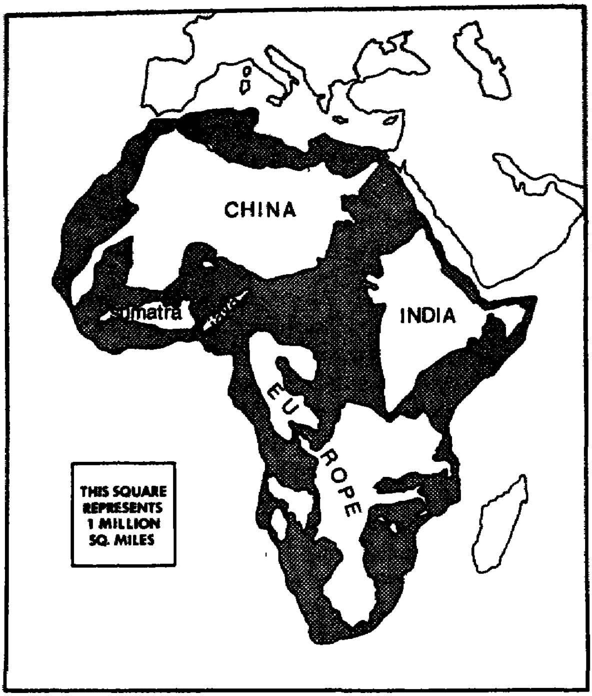


FIGURE 4. Africa to show comparative size. Based on *The Daily Telegraph* Map of Africa. Geographica Ltd, London, n.d.

Moreover, this revised chronology shows that the conventional view of a 'Middle Stone Age' flake tool tradition demonstrating increasing complexity and refinement, culminating in the admixture of forms such as backed blades and burins reminiscent of the Eurasian Upper Palaeolithic, can no longer be justified. Some increase in technical complexity and in stylistic refinement is certainly recognisable in some sequences as, for example, in the Cave of Hearths or at the Kalambo Falls, but these are of secondary significance by comparison with the variability that the chronology indicates was present, both within a single geographical region and between regions, by about 50,000 years ago. Occurrences with a low incidence of retouch (Mossel Bay and earlier Pietersburg traditions) characteristic of the earlier 'Middle Stone Age' are contemporary with others in which bifacially and unifacially retouched forms are common (later Pietersburg and Bambatan traditions). Again there are other occurrences (Howieson's Poort tradition) that constitute another complex and make use of both flake and small blade technologies to produce from the last various tools that, in Europe, would be considered as early Upper Palaeolithic.

At one time—perhaps still today—such technological complexes would be considered the products of successive or contemporary but distinct populations. But it would now seem to be more meaningful to regard them as the residual

components of toolkits that are the visible expression of locally selective exploitation patterns each laying emphasis on using a different set or combination of resources, e.g. large or small game animals, plant foods or marine or freshwater resources. The way in which these several 'Middle Stone Age' traditions or complexes replace each other is not too dissimilar to the manner in which the different Mousterian traditions are seen to interact in Europe. I would suggest that these cultural changes are directly related to ecological changes, not so much those that stem from long-term climatic fluctuations but from shorter term changes that arise from local climatic or biotic factors including those that result from man's own activities. These can, in a comparatively few years, so change the local plant and animal communities that the nature and abundance of the resources exploited from a particular site can change significantly and, with them also, the equipment used to process these resources. For example, what has been termed the 'elephant cycle' can reduce forest to grassland and back again in some 300 years (Buechner & Dawkins 1961; also Dr Harold F. Heady, personal communication) but at the time depth with which we are concerned, differences of 1,000 years, let alone shorter periods, cannot, as yet, be measured with any precision. Yet it is essential to be able to do so since such local ecological changes could very well lie behind and so be the explanation for the inter-relationship of the different 'Middle Stone Age' or Mousterian tool-kits.

Population movements clearly must have taken place but they need to be documented and not lightly assumed. In any case, the new studies of hunting/gathering peoples show that, outside the peri-glacial area, or unless into empty regions, movement is much more likely to have been on an individual, perhaps on a family basis, similar to the anucleated Kalahari Bushman pattern, for example, rather than the hypothetical, large-scale movements many of us were brought up to believe in (Yellen & Harpending 1972).

Finds of new human fossils and new estimates of the age of some well known examples also support the greater antiquity of the cultural evidence in the later Pleistocene. Thus, there is now good reason to associate Rhodesian Man from Broken Hill with an Upper Acheulian/Sangoan industrial stage so that he is a near contemporary of the conspecific Saldanha cranium from the Cape, associated with an Upper Acheulian industry and a Middle Pleistocene fauna (Singer & Wymer 1968; Klein 1973). More recently, amino-acid racemisation dates for some of the Broken Hill human and faunal material show that the human fossils have an age of *c.* 110,000 years (Bada *et al.* 1974). These Rhodesioid fossils—which also show some characteristics in common with Hominid 13 from the Olduvai Gorge—can now, therefore, be seen as representative of the human populations at the close of the Middle and beginning of the Upper Pleistocene.

Although fossil evidence for the Upper Pleistocene populations is still very incomplete and depends often upon a single fragmentary specimen, it is clearly evident that considerable variability, both within a single population and between regional populations, existed in Africa, as in Eurasia, during the later Pleistocene. In both Africa and Eurasia contemporaneous 'Neanderthaloid' populations were being replaced approximately 40,000 years ago by modern man. Three fossils from Member I of the Upper Pleistocene Kibish Formation in the Omo, however, show that an early form of modern man was already present in East Africa

well before 40,000 years ago (Day 1969). The variability shown by the Omo I and II crania is comparable to, though greater and probably of an earlier age than, that in the Skhul population from Mount Carmel (McCown & Keith 1939) and gives some indication of the extent of genetic variation that can be expected in an early population in transition to fully sapient modern man. They show also that north-east Africa cannot be excluded from the region where this genetic change was first manifest. The dates for the Florisbad (Klein 1970: 130, 133) and Border Cave (Beaumont 1973) fossils, if confirmed, also show that modern man had begun to replace earlier forms of *Homo sapiens* around 40,000 years ago in South Africa, at much the same time, perhaps, as he appears in Europe. Again, the orthodox view that Africa lagged behind Eurasia in early evolutionary development has to be revised and, by the beginning of the Holocene, some 10,000 years ago, fossil evidence from each of the main geographical regions of the continent confirms the presence there of the ancestors of the indigenous African peoples, stocks that can now be more clearly seen to have evolved there by selective adaptation during the later Pleistocene (Clark 1970: 117–47)[5].

V

It is not easy to document convincingly from archaeological researches of societies remote in time man's growing self-awareness, developing intellectual ability and technological skills but, when artefacts are studied in the full context of their associations, some significant indications emerge, a few of which I would like to mention since they concern Africa. For example, the earliest structure built by man comes from the base of the Olduvai sequence and dates to *c.* 1·8 m.y. ago (M. D. Leakey 1971*a*: 24; fig 7); the earliest appreciation of colour is perhaps to be seen in the red ochre fragment (L. S. B. Leakey 1958: 1100) and green lava cobbles (Hay 1971: 17) carried onto certain of the occupation floors preserved in Beds I and II at the Olduvai Gorge. The symmetry and refinement of some of the earlier Acheulian handaxes, which surely go beyond the basic utilitarian need, may reflect the first appearance of an aesthetic appreciation of form. Again, although as yet not radiometrically dated, other evidence suggests that the Levallois method was developed in tropical Africa for making the large formalised Acheulian bifaces from boulders and blocks of hard rock (Balout 1967: 730–1; Lowe 1945). Musical appreciation can, perhaps, be recognised on the basis of the bone flute from the basal industry at Haua Fteah, Cyrenaica, as far back as the Last Interglacial (McBurney 1967: 90, pl. IV, 4). The use of sea foods also goes back to this time as indicated by the large quantities of mollusca with the same Cyrenaican industry (McBurney 1967: 99) while an increasingly more intensive use of marine resources is attested from the south end of the continent after the close of the 'Middle Stone Age' about 30,000 years ago (Klein 1974: 269).

The Middle Palaeolithic/Aterian technique of hafting by means of a tang is now seen to date back more than 45,000 years ago and is the oldest indisputable evidence from anywhere in the world that stone artefacts had begun to be mounted in handles for greater efficiency (Haynes & Haas 1974)[6]. Again, there is strong reason to suggest that the long and slender, bifacial lanceolates of the Aterian in the Sahara and the Lupemban in what is now equatorial forest, besides being some of the finest known examples of Palaeolithic stone work, were mounted as the cutting

and piercing parts of projectile weapons (Clark 1968: 130–2). The early blade tool assemblages from the Last Interglacial in the eastern Mediterranean have their counterpart on the coast of Cyrenaica (McBurney 1967: 75–101; Garrod 1961), while some of the earliest 'Middle Stone Age' assemblages from southern Africa that date to this time, also show high percentages of both large and small blades (Sampson 1974: 157–77).

The early appearance of backed blade forms and burins in the 'Middle Stone Age' in sub-Saharan Africa—well before 35,000 years ago—appears to anticipate, rather than follow from the appearance of the Upper Palaeolithic in Europe (Vogel & Beaumont 1972). It carries with it also the implication, reinforced by the almost 20,000 year duration of the Dabban Industry in Cyrenaica (McBurney 1967: 135–84), that tool-kits reflect the traditional preferences and adaptive behaviour of their makers, undergoing change when the population was presented with and took the opportunity to adopt more efficient life styles but not otherwise.

Important technological changes that mark the beginning of the 'Later Stone Age' and the introduction of new exploitation strategies can now be seen to make their first appearance some 20,000 and more years ago in southern Africa and show that, far from being, as it were, the poor relation of prehistory, Africa continued to make its contribution, with the hunter/gatherer populations of Eurasia, to the growth of the more efficient life-styles prevailing by the close of the Pleistocene. Fully developed microlithic industries with backed bladelets and lunates—the hallmarks of the composite tool and, perhaps, by implication, the invention of the bow and arrow—make their appearance in central Africa between 16 and 12,000 years ago (M. D. Leakey *et al.* 1972; Van Noten 1971; Miller 1971; Deacon 1966). Handstones for grinding were present more than 27,000 years ago;[7] bored stones or digging stick weights by 16,000 B.P., if not before (Clark 1974: 225–6; Miller 1971); edge ground axes by 10,000 B.P. (Miller 1969; Clark 1974: 141) while the earliest dated rock art in Africa—already evolved paintings on flat slabs—comes from a deep, stratified succession in a South West African rock-shelter and is more than 12,000 years old (Wendt 1972: 20).

Studies in the valley of the Nile, initiated by the building of the Aswan High Dam, show that here as in southwest Asia, an equally long period (starting 15,000 years B.C.) of adaptation, experiment with local plants and animals and readjustment by the more closely structured populations, lies behind the appearance of agriculture in Egypt in the 5th millennium B.C. or earlier (Clark 1971). In other parts of the continent also, the early Holocene is characterised by a series of such adaptations to changing environments and other pressures. An increasing number of radiocarbon dates leaves little reason to doubt that the 6th millennium saw an independent development of pottery-making on the Upper Nile and in the Sahara, where also the wild cattle of North Africa were apparently domesticated by 4,000 B.C. (Camps 1974: 223–4; Hays 1974). The wide distribution of this pottery tradition and other traits shows that the desert environment was no deterrent to inter-communication and the maintenance of common life-styles over much of northern Africa at this time. Similarly, the trade in Amazonite, like the obsidian trade in southwest Asia, is further confirmation of cultural interchange over great distances and, with it, the spread of knowledge concerning economic and technological innovations (Mauny 1956).

The eventual adoption by the peoples of North Africa of the southwest Asian domesticates undoubtedly reflects the greater genetic advantages of these forms over most indigenous species with which the prehistoric Egyptians experimented. But, in view of the demonstrated long period of pre-adaptation, this also suggests that it was not until the economic returns were seen to outweigh the loss of independence attendant upon the farming way of life, that these new domesticates were adopted. It should be possible to test such hypotheses by relevant archaeological investigation and there is a need to do so and also to determine the extent to which the tremendously rich biomes south of the Sahara may have contributed to an even longer delay in the development there of local domesticates.

VI

The construction of models for interpreting discrete units of archaeological data is made possible by the identification of processes, perhaps universals, established by programmes in ethnoarchaeology. In Africa, the eco-systems below the Sahara have not suffered the same disruption by man and beast as have those in the Middle East or Europe and it is still possible to find and study there ecological habitats similar to those that archaeology shows to have pertained in the 3rd or 4th millennium when indigenous African food plants began to come under intensive modification (Harlan & Stemler in press). Similarly, it is still possible to study social and economic systems based upon herding and the collection of wild plant foods, mostly grasses; on fishing and collecting; hunting and herding; mixed farming and pre-industrial urban centres. The models suggested by the ways in which these societies have adjusted to the environmental and demographic pressures and stresses of their habitat can provide the archaeologist with working hypotheses against which to test the excavated record of the change from food-gathering to food-production and are equally as relevant for regions outside as within the continent. The value of such evidence to archaeologists cannot be over-estimated, though it has, as yet barely begun to be used by them. Just as the geologist and palaeontologist study the present in order to interpret the past, so can study of transition in Africa today help to make explicit the social and economic forces that may have acted on human societies making such changes in earlier times. What greater opportunity could there be for the prehistorian who was, as it were, born at the wrong end of time and has to live backwards from in front?

The new techniques developed by the Leakeys and other African archaeologists since the 1940's for the recovery of information from the primary context living sites of early man have added, as it were, a new dimension to the study of Palaeolithic populations. No longer are the artefacts alone the most important part of the cultural record. It is the pattern of associations between these artefacts and all the other related evidence and features of an occupation area, that provides the greatest potential for reconstructing the behaviour of the occupants. This necessitates the collaboration of experts from many different disciplines working as a team. A series of such interdisciplinary teams have been at work in East Africa over the past ten years and the results obtained have brought about a revolution in our knowledge of Pleistocene prehistory. At the same time, interest in human origins has so transcended nationalistic attitudes that these teams are also now international

in outlook and composition and work in the closest collaboration. This is, indeed, a far cry from the days when prehistorians often worked in secrecy, and these African discoveries have assisted in no small measure in breaking down old restrictions and dog-in-the-manger attitudes. The prehistorian is now seen as only one part, albeit a most important part, of the interdisciplinary team closely collaborating in field studies and it seems to me that a new level of prehistoric research could be attained if palaeo-anthropological studies could be regrouped along the lines suggested by Sir Julian Huxley in 1963: 'In place of special subjects, each with its own assumptions, methodology and technical jargon, we must envisage networks of co-operative investigation, with common methods and terminology, all eventually linked up in a comprehensive process of enquiry'. Africa, where so much of the pioneer work has already been carried out, would seem to be the ideal place to initiate such studies on a permanent basis.

VII

In this necessarily brief survey of the significance of Africa in prehistory, I have, I hope, made clear that not only does the available evidence indicate that it was in that continent that those developments which gave us our humanity had their origins, but also that it now appears that some of the technological innovations of the Pleistocene and earlier Holocene were first developed there or, at least, were of equal antiquity inside as outside the continent. Brought up as most of us were with an idea of culture similar to that of the Greeks in which other societies could claim to be civilised only in so far as they resembled our own, it is probably not surprising that our view of Africa should have been similar to that of Strabo when he said, 'In general the extremities of the inhabited world which lie alongside the part of the earth that is not temperate and habitable, because of heat or cold, must needs be defective and inferior to the temperate part, and this is clear from the modes of life of the inhabitants and from their lack of human necessities (Jones 1932: 8, 141–2). Only a century and a half ago Hegel was able to declare 'Africa is no historical part of the world; it has no movement or development to exhibit'. Even in our own time—no more than a few years ago—an otherwise eminent historian permitted himself to write that Africa's past reveals 'only the unrewarding gyrations of barbarous tribes in picturesque but irrelevant corners of the globe' (Wilkes 1971: 7).

It is probably the liberation of the former colonial peoples and their efforts to preserve their own life style and traditions coupled with the many attempts by social anthropologists and ethnographers to understand and explain the history of human societies different from our own, that have resulted in a genuine reaching out to try to understand these and to appreciate them for their own worth and no longer to measure them against our own and find them wanting. There is, however, still a need outside Africa for palaeo-anthropologists—archaeologists, if you prefer it—to re-evaluate their traditional conception of the position of Africa in prehistory. As a colleague of mine at Berkeley has put it: 'Archaeology has too long and honourable a tradition to be surrendered without a protest to fantasies which require us to start with our conclusions and use them to deform the evidence' (Rowe 1966), and I hope that I have been able to demonstrate—even if not

to convince!—that, far from being on the periphery of the discipline, Africa's contribution is such that, without it, there would, *on the evidence available today*, not only be no prehistoric studies as we now know them, and no civilisation—but no human race!

NOTES

I feel very deeply the honour of having been invited to give the 1974 Huxley Lecture. It is nearly twenty years since I last spoke at the Royal Anthropological Institute and I thank you most sincerely and for giving me this opportunity to review some of the new discoveries, methodology and changing concepts in African prehistory that are making this field such an exciting one today. But before doing so I wish to pay tribute to those prehistorians—many of them from Britain, in particular from the Schools of Cambridge and London—whose earlier research and excavations have provided the basis for the successes of the present decade. An overview of the evidence that I shall put before you would not have been possible if it had not been for their work and our debt is a very considerable one to men and women like Tony Arkell, Miles Burkitt, Gertrude Caton-Thompson, Bernard Fagg, John Goodwin, Louis and Mary Leakey, Kenneth Oakley, E. J. Wayland and many others, as also to their counterparts from France and Belgium. They laid the foundations and their work made possible the new techniques and strategies used by many of the active researchers—friends and colleagues, black and white—in the field today whose contributions now make this reassessment both possible and necessary.

I would also like particularly to express my personal thanks to my colleagues at Berkeley—Elizabeth Colson, Glynn Isaac and Sherry Washburn—for being kind enough to read through the draft of this lecture and for their several suggestions that have been incorporated. In addition I am grateful to the L. S. B. Leakey Foundation for having given me my fare to London and, as often, I would also like to thank my wife.

[1] The epochal boundary between the Miocene/Pliocene followed here is that suggested by van Couvering (1972: 251–2) and others on microfaunal evidence from the Mediterranean, and placed at ~5·5 m.y. The boundary between the Pliocene and Pleistocene has been taken as ~2·5 m.y. pending any consensus ruling on this difficult question.

[2] Amino-acid racemisation age of ~190,000 years B.P. obtained on wood from Acheulian horizons at Kalambo Falls (Dr Jeffrey Bada, personal communication).

[3] Recommendation of a conference on 'Stratigraphy and patterns of cultural change in the Middle Pleistocene' held at Burg Wartenstein, Austria, July 2–11, 1973, and sponsored by the Wenner-Gren Foundations for Anthropological Research, New York.

[4] Rooidam, Cape Province (see Butzer 1974).

[5] The rapid cultural diversification associated with the spread of modern man during the later Pleistocene cannot have been possible except through the medium of speech and it now seems most probable that it was the possession of language proper that lay behind the phenomenally rapid success of *Homo sapiens sapiens*. Using linguistic evidence Foster (in press) suggests that the history of language covers a period of some 50,000 years and she shows how this primordial language might have been structured. Foster's hypothesis finds support in the work of Cavalli-Sforza who suggests that 'the earliest fission in human racial divergence (leading to the two most widely different groups—African and Eastern populations . . .) has been dated between 25,000 and 100,000 years of age. Increased knowledge available today may permit us to refine this estimate and reduce its wide range of error. It is interesting that this estimate is not inconsistent with the notion that modern man appeared on the world scene as judged from skeletal remains, at least 50,000 years ago. The extension of modern man to the Old World (mostly Asia and Africa) may have been fairly rapid and the differentiation into major races may have started soon thereafter.' (Cavalli-Sforza 1973: 17–18).

[6] Bir Sahara series, SMU–79>44,680 and SMU–81>41,450.

[7] Present in Rubble 1 (c), Mbwilo member of the Kalambo Falls Formation, Zambia, and associated with the Sisya industry of the Lupemban Industrial Complex (27,500±2,300 B.P.); also at other sites e.g. with the Pietersburg Industry at the Cave of Hearths and Kalkbank, Transvaal.

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MILITARY ORGANISATION AND THE PRE-COLONIAL POLITY OF THE BEMBA OF ZAMBIA

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In the fifty years or so that preceded the establishment of *pax britannica* in what is now Zambia, the Bemba fought a series of wars with their neighbours that resulted in their gaining hegemony over most of the Great Plateau. Their remarkable military success is commonly attributed to their acquisition of fire-arms from Arab traders, but this view does not appear to stand up to critical scrutiny. Attention here is focused on indigenous modes of military organisation. The further analysis attempts to trace out some of the implications these had for other aspects of the pre-colonial Bemba social system. This emphasis on military power leads to a reappraisal of certain aspects of their political system as it has been presented in the ethnographic literature, and suggests some problems for further comparative research.

War has always provided a rich field of speculation for the theorists of society, but at least among anthropologists there has been a singular neglect of warfare itself—the analysis of the waging and conduct of war in different societies and its possible bearing on other aspects of social life. The point is by no means a novel one; Davie was clearly struck by it when, in introducing his study of the evolution of war, he was led to comment (1929: 9) that it is so much easier and more alluring to speculate as to the how and why of war than to grub for the facts. Turney-High's book *Primitive warfare*, first published in 1949, and Andrzejewski's study *Military organisation and society* (1954) were important attempts to draw attention to the problems and stimulate further enquiry. Since then there have been a number of studies of warfare in particular societies (e.g. Vayda 1960; Chagnon 1968), but the glaring fact remains that compared with other domains of social life the ethnography of war is still remarkably thin (cf. Fried *et al.* 1968: xiii; Hallpike 1973: 451). In these circumstances some account, however manifestly inadequate, of Bemba warfare and military organisation would seem justified. But beyond the purely descriptive task I also hope to show that consideration of these issues may illuminate some other aspects of pre-colonial Bemba polity and society.

I

The expansion of the Bemba

The Bemba today are numerically the largest and one of the most important ethnic groups in Zambia. During the 1930's when they were studied by Audrey Richards they numbered around 120,000 and their territory covered an area about the size of modern Ireland.

It was not always so: the evidence is quite clear that Bemba territorial expansion was a fairly recent historical development. The real heart of Bembaland is Lubemba

Man (N.S.) 10, 199–217.

in the Kasama District. It is here that Chitimukulu, the Paramount Chief, has his capital and the hereditary councillors known as *Bakabilo* have their villages. There are a number of other areas—Ituna, Miti, Ichinga—which are regarded as Bemba country proper: these are ruled by chiefs of the royal Crocodile clan, the *Bena Ngandu*. Reaching out in all directions beyond the boundaries of these areas are other *fyalo*, or provinces, also ruled by Bemba chiefs. Some of these are *Bena Ngandu* too (e.g. Chikwanda, Shimumbi) but most belong to the category referred to as *bana ba mfumu*, sons of the Chief or King. Since the Bemba follow the rule of matrilineal succession, such sons are not of the royal clan and cannot succeed to the major titles. All the areas now ruled by the *bana ba mfumu* were once the territories of other tribal groups, governed by their own chiefs and leaders. But from somewhere around 1830 the Bemba became increasingly involved in wars of conquest with their neighbours until within the space of about fifty years the whole of the Great Plateau came under Bemba hegemony, representing at least a fourfold increase on what they had previously claimed as their own.

How are we to account for this remarkable development? One answer has recently been suggested by Langworthy (1972: 42–4). Bemba expansion, he says, was a response to environmental conditions. Bemba country lay within the tsetse-fly belt in which it was difficult to raise cattle. Moreover, the leached and infertile soils of Bembaland yielded a bare subsistence on grain crops; regular periods of hunger were a feature of local life. 'Because of the poverty of their area, the Bemba found it difficult to trade for what they needed . . . poverty of environment and economy encouraged the Bemba to expand by warfare at the expense of their relatively richer neighbours'.

At first glance this seems quite an attractive view. Further reflection, however, suggests that it is hardly adequate as it stands. It appears a fair assumption that natural environmental conditions had been relatively stable for some time. Why then should expansion have taken place at this time and not throughout the previous 150 or so years that the Bemba appear to have been in possession of their homeland? If environment is a factor, it must be because of more recent changes of another kind. Two possibilities need to be considered: (1) population increase and (2) trade.

On the first point, if there were growing pressure of population, the killing of the enemy and the conquest and settling of his lands would certainly have helped to ease the problem. At first sight this might seem unlikely given the fact that the area always appears to have been sparsely populated: Richards (1939: 18) reports that density in the 30's did not exceed 3·9 per square mile. On the other hand, given the low carrying capacity of *citimene* systems and other ecological factors characteristic of the region (see Allan 1965, esp. ch. 9), it is possible that even a marginal increase of population could have set up land pressure. But all this must remain speculative; for the moment there are too many unknowns in the equation to allow of any reliable judgement in the matter. On the second point, we are on rather stronger ground and Roberts (1974) has recently presented a very careful and persuasive analysis of changing patterns of trade within the region. The incursion of Arab traders in the period we are presently considering had profound ramifications. It appears, for example, to have contributed to the decline of the Lunda Empire of Kazembe to the west, thus creating a power vacuum. No less important, however, were the new opportunities created for trade, at first primarily for ivory, but then

increasingly for slaves too, which provided the Bemba with an incentive to seek to establish control of the trade routes which lay beyond their traditional frontiers.

Such motives may help to explain the why of Bemba expansion. They cannot of course explain the how of it. In particular, if their neighbours were richer than the Bemba, they had much more to lose. Why then were they so easily conquered? Or, putting the question in another way, why were the Bemba so militarily successful?

II

Weapons: the arms issue

Civilians, Turney-High points out, tend to seek an answer to such questions in terms of possession of superior weapons. He has, in fact, little difficulty in demonstrating that superior weapons are no guarantee of success: the clue to military victory lies elsewhere. Nevertheless, each case has to be examined on its own merits. This is particularly so in the present instance, since possession of guns or muskets is mentioned so often in the literature as a factor in Bemba success. Thus Sheane (1911: 24), for example, has described (on the basis of an informant's account) Bemba preparations for war: 'a King of "angry soul" would call together the younger warriors and give them guns and powder and fine clothes inside the gates of the harem'. The argument has been restated most recently by Langworthy (1972: 74): 'The Bemba were united under powerful leadership and had guns. The peoples who were defeated were usually not well united and frequently did not have guns'.[1]

Happily, this issue has been most carefully reviewed in a paper by Roberts (1971) and it is unnecessary to retrace the ground in detail Roberts suggests that the defeat of the Tabwa Chief, Nsama, in 1867, gave rise to an exaggerated faith in the foreign traders' new weapons, but notes how this soon yielded to a rational assessment of their military value and economic costs as compared with the other weapons available. Of these he refers especially to the spear, commenting that the heyday of Bemba conquest seems to have coincided with an increasing dependence on spears. Given Wills's statement (see note 1), of particular interest is Roberts's (1974: 202) comment that it is quite possible that throughout the conquests and conflicts of Chileshye's reign (when the Ngoni arrived) the Bemba not only had virtually no guns but remained ignorant of their proper use. Roberts's general conclusion, restated in his book (1974: 205), is that it is doubtful whether firearms were an important factor in the growth of Bemba power as they were, for example, in the case of Msiri, founder of the Yeke Empire. This judgement coincides with my own, which is based not on a study of the documentary evidence, but on what I learned from my Bemba informants. I shall return to the point a little later. The immediate conclusion to which we are led is that for an explanation of how the Bemba expanded so rapidly we need to examine their modes of military organisation.

III

Military organisation

Roberts (1974: 169) remarks that Bemba society was strongly characterized by military ambition and achievements, but that it could hardly be said to have been

militarised in the same sense that this was true of the Zulu or the Ngoni. At different points he refers in passing to a number of aspects of Bemba military organisation which would appear to substantiate this claim, and I will refer to these again shortly. For present purposes the point that needs to be made is that the fundamental criterion for evaluating forms of military organisation must be effectiveness. Effectiveness, however, is not easily measured in abstract terms; military organisation needs to be examined within the context of aims and objectives, which are military and political, as well as the conditions, social and environmental, under which warfare is to be waged. Successful warfare implies the co-ordination of a number of activities towards a particular end, and that in turn involves the solution of a number of recurring or 'type' problems. From this point of view we can describe the military organisation of a particular people in terms of a limited number of variables: command structure, tactical operations, planning and logistics, and the like.

A preliminary word needs to be included here about the nature of the material on which the analysis is based. The literary sources for a study of Bemba warfare are limited in the extreme. Of the few reports that are available, none shows evidence of being based on direct observation, some, such as those of Father Labrecque (1968) and Father Oger (n.d.), present what has clearly become a fairly standardised folk narrative: we are told, for example, of wars and battles and the chiefs who joined to fight them, but nothing of the strength and disposition of the forces they led. My own data too are drawn from informants, none of whom of course had direct experience of the events they were describing, and, although I sought to check most points with different people, there are still unfortunately many uncertainties and lacunae in the material. A further difficulty is that we are dealing with a situation that was basically unstable. One of the problems I am attempting to tackle is essentially a problem of change, but unfortunately there are few fixed points available by which to calibrate this process. We shall meet these difficulties at every point in the following discussion, but they emerge most crucially at the very outset when we seek to examine the command structure.

IV

Command structure

Accordingly to Gouldsbury and Sheane (1969: 23), all the more serious affairs of state, including the making of wars and the declaration of peace, were settled by the *Bakabilo* in council presided over by the King, Chitimukulu. When acting as a council of war, the council was known as *cilye che lamfya* after the *ilamfya* or war-horn. The horn, containing various substances which had been prepared by ritual specialists, served as an instrument of divination, but it also accompanied the troops when they set out on the warpath in much the same fashion, one supposes, that the ancient Israelites marched into battle bearing aloft the Ark of the Covenant. The *ilamfya*, it should be noted, was peculiarly associated with the King. It was on his instruction that a new *ilamfya* was prepared; on the completion of the task he was required to carry out a special sacrifice, and thereafter, save when removed to accompany a war party, it rested in the inner room of the quarters where the King slept.

But how far does all this imply a centralisation of military command? How far did the decisions reached in the *ilamfya* council embrace all Bemba areas and their chiefs? So far as the *ilamfya* itself is concerned, it is plain that Chitimukulu did not have the sole monopoly. Sheane, an early official of the British South African Company, was presented with a horn by Shimumbi which had been assigned to him by Mwamba. Shimumbi was a *mwina ngandu* (i.e. of the royal Crocodile clan). Lesser chiefs, however, were unlikely to have had the ritual specialists in their villages to prepare the horns and were thus placed in a position of dependence on their senior clansmen or their royal fathers.

Many features of the pre-colonial administrative system still remain obscure. Given the extent of the area to be governed, particularly in later days, and the prevailing modes of communication—since much of the country was woodland, movement was mainly through well-worn paths through the bush—one would expect a decentralised system with a high degree of local autonomy. Gouldsbury and Sheane's account may represent therefore an over-systematised model as it might appear to an early administrator in the area. They refer to a number of different levels of administration:

1. *Wasimupelo*. These were the Lords of the Marches, direct appointees of Chitimukulu, who controlled the remote provinces and the lines of boundary villages. They occupied stockaded villages across a main trade route or controlling an important ferry: such villages were flanked by a line of other minor villages along the frontier.

An important task of the *wasimupelo* was control of trade. Everyone crossing the frontier for purposes of trade was required to pay a toll-fee. *Wasimupelo* were also collectors of tribute from among the subject peoples within their areas. But the Bemba were also conscious of the military importance of these areas which were known in the vernacular as *fipata fya calo*, the gates of the country. They had a keen sense for the strategic position of these 'gates'. Luwingu, for example, seems to have drawn its name from the maxim *luwingu lwa nsase uwatumpa lwamocha* (he who does not take care of the fire gets his fingers burned), a reference to the fact that the outpost was located between two hills with a commanding view of the surrounding countryside.

2. *Balashi*. Next in importance were the *balashi* or district officers. Each was responsible for an area with definite hill and river boundaries, and had various duties including providing a contingent for war.

3. *Bashicalo*. Finally, there were the village headmen, responsible to the *balashi*.

Gouldsbury and Sheane make the point that the *wasimupelo* were not independent petty chieftains, but it is not clear what precisely this means. All the areas of Bemba country, whether long established or more recently acquired by conquest, were defined territorial units called *calo* (pl. *fyalo*) under the rule of the chiefs, *mfumu* (pl. *bashamfumu*). Among these were the *bana ba mfumu*, sons of the Bena Ngandu who, in the later period of Bemba expansion, came to rule very large areas, such as Munkonge or Mporokoso. All of these acquired their titles in the way just described for *wasimupelo*, but what is not clear is whether we have here alternative terms for the one category or two distinct social and political categories.[2] If the latter, we would need to know more about the relationship between them.

What does seem fairly plain is that at least the Bena Ngandu chiefs (Chitimukulu,

Mwamba, Nkolemfumu, Nkula and some others) and perhaps too the lesser chiefs had the power to organise independent raids. What also appears likely is that as the Bemba war machine became increasingly proficient and successful, so the command became increasingly centralised. This view appears to have Roberts's support for he notes (1974: 181) that in the time of Chitimukulu Chitapankwa (whom Livingstone visited in 1867) Bemba chiefs on the east and north-east borders seem not to have conducted wars of invasion and conquest independently of him. The processes by which this took place, and the reasons for it, I will discuss later. What must be noted here is that a stable political system with power unequivocally established at the centre was never achieved, with consequences for the Bemba that have lasted down to the present day.

Thus far I have been looking at the command structure in global and essentially administrative terms. It is also important to look at in a more purely military perspective, to take what one might call a headquarters' view of the matter.

Roberts (1974: 168) states that the Bemba had no standing army. For the Bemba to mount an expedition of more than 200 men, he says, it was necessary to conscript villagers outside the village Each campaign moreover was a somewhat *ad hoc* affair. Now it is true that the Bemba had no traditional system of age-regiments of the kind familiar among many African pastoral peoples, a principle of organisation which Chaka, for example, was able to adapt in creating the Zulu armies. However, if we examine the structure and composition of the *musumba*, the village of a chief, and especially the *musumba* of Chitimukulu, we may be led to some modification of Roberts's view.

The Bemba capital at the zenith of their power appears to have covered a large area. According to Livingstone (1874: I, 184), Chitapankwa's village had a triple stockade, the inner one defended by a deep broad ditch; it was about 200 yards broad and 500 long. Within this inner stockade was *musumba* proper or *isano*, the royal compound where the king stayed with his wives and where he also had his private quarters previously mentioned. The fence of the *isano* had a number of gates. Astride these entrances, and reaching towards the village, were *fisaka*, long houses or dormitories of which at one time, according to one of my informants, there were fifteen to twenty. The *fisaka* were the quarters of a category of persons known as *milubamfwa*, literally those who ignore death, 'the fearless ones'. They were young, unmarried males, who were servants of the King but who when the Bemba sent out a party also had a special role to which I will refer later. Some of these youngsters were sons of the *bena musumba* (the residents of the capital) but *milubamfwa* were recruited from all the villages within the area. It appears that the Bemba were alive to the problems of recruitment, and if the King's attention was drawn to the presence of some likely lad messengers were sent to bring him to the capital. No doubt, too, as the system developed, fathers anxious to seek preferment for their sons, or mother's brothers for their nephews, would send them to serve the King.

Sometimes in talking about the *milubamfwa*, and to sharpen the image, my informants would point to some young man nearby in his early or mid-twenties and remark that in the old days such a man would still be serving *ku isano*. If I remarked that the person in question was already married, I was quickly reminded that in the past things were different: *milubamfwa* did not marry until they had the King's per-

mission. One *kabilo* explained on another occasion that in earlier times the *bena musumba* did not bother about marriage payments; they received their wives by way of gift from the King. Such gifts were usually conferred only after one had proved himself in battle by bringing back the head of the enemy he had slain. (These heads were prominently displayed around the main stockade of the capital.)

Control of marriage in this way is found among many African people with a martial tradition, and it was of course a notable feature of Chaka's military organisation; it is an important aspect of command and discipline. *Milubamfwa* worked in the King's gardens, but I have no information how far, if at all, they received any kind of military training. What is clear however, is that at least the later kings were at pains to inculcate a warlike spirit among the young. Sheane (1911: 31) reports that youths who had not yet been to war were ordered to go out to meet the soldiers returning from a raid and were then required to carry the enemy heads taken in battle. Those who did this without blenching were rewarded with special privileges ordinarily allowed only to older men.

I have referred to the *cilye che ilamfya* or war council which was made up of the King and his hereditary councillors or *Bakabilo*. There were something of the order of forty Bakabilo, holders of hereditary titled offices. The *Bakabilo* did not live in *musumba*, each had his own village within Lubemba, located in a particular position in relation to the capital. However, when their presence was required there, they gathered at their own special quarters which are referred to as *nsaka*. There were five of these *nsaka*: the first was that of *nchenje* to which belonged the most senior of the *Bakabilo* with important ritual and executive responsibilities. Then there were the *nsaka* of the *Bena Katongo;* of *Bamushika;* of the *Shifingo;* and of another for which I did not discover the name. Each of the five divisions of the *Bakabilo* had its own clearly prescribed tasks and duties, often of a priestly or ritual kind, relating to some aspect of the kingship (cf. Richards 1968). Of particular interest in the present context is the *nsaka* of *Bamushika*, whose members were especially concerned with matters of war and its planning, though when actual warfare was contemplated consultations took place with the other *nsaka*, particularly of *nchenje*.

The term *mushika* refers to a commander of the King's army, but it also refers in a more general sense to the stewards of the King. The majority of people in the *musumba* lived in village sections or *fitente*, each of which was under the control of a *mushika* assisted by a body of elders, the *bafilolo* These section headmen, *bamushika*, enjoyed no hereditary rank; they might be ordinary commoners, even slaves, who were appointed by the King because of their prowess and experience in warfare. The *mushika* might have a number of kinsmen living with him in the section, but he was not a village headman, *mwine mushi*, in the usual sense, for the section's residents were not brought together on the basis of kinship as in the case of an ordinary village. Rather they were composed of social categories carrying such names as *mipamba nkondo*, *misoka*, *mpashi*, *kapinda* and the like, all of these having some association with warfare or soldierly qualities. Thus *mupamba* is the term for an evil omen; it also means, perhaps by extension, a wicked man. In the present context it refers to mature men who were seasoned soldiers. *Misoka* is the ordinary term for murder; here it refers to older men whose task it was, on the march or in battle, to follow behind the younger men both to encourage them and to make sure that they did not retreat. *Mpashi* is the word for soldier-ants, noted for the

characteristic way in which they march in columns and the extreme difficulty of deflecting them from their course. The association here is self-evident. *Kapinda* refers to young warriors who had been given permission to marry.[3] The sections to which these different categories were attached had no permanence; they were known simply by the name of the *mushika* in charge.

I think it is worth mentioning that one of my informants, without any prompting on my part, told me that the *citente* system had been introduced by Chitapankwa, and that it was in his reign that the *musumba* reached its largest extent. Under his successor, Sampa (1884–96), it had already declined. What the total population of *musumba* was at the time of Chitapankwa I am unable to say. Some *Bakabilo* said that there were so many sections, they could not remember them all. One undated report by Père Guilleme in the Kasama district notebook mentions thirty-three divisions, while Labrecque (1968: 301) speaks of thirty or forty. The *musumba* of Mwamba replicated in many of its main features that of Chitimukulu and I was told that the capital of Mubanga Chipoya, Mwamba the Great (ruled 1884–98), had perhaps 15–20 sections and I was given the names of a dozen section heads.[4]

Thus it does not seem to me to do violence to the evidence to suggest that the *musumba*, at least in Chitapankwa's day, had many of the features of a garrison or military base. Certainly, as my informants expressed it, the army was always gathered there. But the point also came up spontaneously in quite different contexts and in a way very revealing about Bemba thought on the matter. Once, for example, I was discussing traditional methods of dealing with disputes and offences. My informant outlined the general procedure and then took a hypothetical case involving a man and a woman living at the capital by way of illustration. Then, my informant went on, if the *bafilolo* found the woman was at fault, the chief would upbraid her, asking her why she was destroying his soldier (*cita*) and then sell her into slavery.

The *musumba* was in the first place a unit of defence. It was surrounded by a trench and concealed spears buried in the ground provided a trap for the stranger or the unwary. All who approached the stockade were required to carry their weapons pointing towards the ground, and none could carry an axe on his shoulder without being taken for an enemy. In the event of attack, the beating of the *imangu* drum sounded the alert and called the men to their stations.

But it was much more than an arrangement for defence. Under Chitapankwa at least it seems to have been more a springboard for attack. Apart from the permanent presence of the soldiery, the capital was at the centre of an elaborate network of intelligence provided by messengers and spies (*nengu*) reporting from the villages of the area and the more outlying districts. It was from the capital, too, that the command went out when, in the event of a major expedition or war, it was thought necessary to bring in the other chiefs and their forces. Finally, it was at the *musumba* that the complex rites and invocation of the ancestral spirits were carried out as a necessary prelude to a successful campaign, the topic to which I now turn.

V

Tactical operations

One of our earliest accounts of the Bemba is provided by the Portuguese traveller Gamitto, who visited the court of Mwata Kazembe in 1831 and later passed through

Bisa country shortly after it had been conquered and occupied by the Bemba. Of their methods of waging war he reports that 'the Bemba always procure a surprise attack, but this is put in without discipline and without any particular tactics' (Cunnison 1960: 2: 159). A similar view of Bemba military organisation appears to underlie the comment by Roberts (1974: 169) that apart from the fact that the *basole* [?= *milubamfwa*] of Mwamba fought together as a vanguard, Bemba appeared to have no regular patterns of assembly, line of march or deployment in which leaders and followers had fixed roles and positions. On the other hand, Roberts does observe in a later context (p. 205) that in their protracted struggle with the Ngoni, who had succeeded in occupying part of Mpanda, an area ruled by Makasa, and whose threat the Bemba had the greatest difficulty in containing, final success was 'due to the resourceful tactics of the Bemba and the massive, if temporary, alliance of chiefs from all over Bembaland'. It may not be irrelevant here to recall the observation of Turney-High (1971: 126–7) that the lack of a tactical sense has so often been attributed to 'tribal' peoples that it has become almost an *idée fixe* in the civilian anthropologist and civilised military mind. The reason for this is fairly simple: what the earlier observers of these societies saw or heard they interpreted in terms of the assumptions about the way battles should be fought prevailing at the time in their own cultures. As Turney-High puts it in his characteristically forthright way, 'because the American Indian failed to array a dress parade, he is accused of having no battle method'.

Bemba planning for war included decisions not only on what detachments should set out, but also which sections should be held back to defend the capital and country against attack. The war party or army as a whole was called *mulalo*, but its size depended on the nature of the operation. The party itself was made up of units known as *mpuka*, whose numbers varied between twenty and sixty men. Each *mpuka* was under the command of a *mushika* and there was a second intermediate officer or lieutenant, but my informant could not recall the proper term for him When the occasion demanded, *mpuka* were grouped into larger formations known as *nyimbi*. *Nyimbi* ordinarily refers to rapidly passing clouds and as such were taken in the past as a sign that warfare was going on somewhere. In a military context, however, the term was applied to a unit of about 200 men.

On the march the Bemba adopted the basic military formation of the column, a number of columns marching in parallel. In the van was the standard bearer, the carrier of *ilamfya*; behind were the *milumbamfwa*, *kapinda* and *mipamba* in the fashion I have already described. When the chief himself accompanied the war party, he brought up the rear with his own party which included his staff officers, the senior *bamushika*, and a body of protecting troops who guarded the flanks. Those who had already killed in battle marched carrying only their weapons, their loads and food for the expedition being carried by groups of porters.

It will be appreciated that on many of these occasions considerable distances were involved. The party would march during the day and would seek to avoid contact with the enemy. Towards evening a halt would be called and the party would strike camp. For the King or chief a special stockade was put up which provided the *nkambi* or staff quarters. Other ranks put up temporary shelters called *nsakwe*. Meanwhile, scouts (*nengu*) were reconnoitring in advance of the party and messengers moved to and fro between the groups.

I have referred to Gamitto's emphasis on the element of surprise in Bemba attacks, and this is confirmed in later reports as well as in the accounts I myself was given. The aim was to approach the enemy village without being detected. At this point the force was split: some detachments were sent forward as close to the stockade as possible. Their task was to seek out the weakest parts of the fence. Meanwhile their fellows waited further back and in positions around the village until just before dawn when the attack was launched. In Sheane's words (1911: 27):

> When the word was given, a rush was made for the weakest part of the fence. Some dug out the foundations of the stoutest poles, whilst others, climbing to the top, hacked with their axes at the withies that bound them, and dragged the stakes of the palisade to the ground. The besiegers poured through the breach thus made, and on entry the keeper of the *ilamfya* fired the first shot as a signal for a general fusillade

During the reign of Chitapankwa muskets had become plentiful, but it is plain from all the accounts I received that they can have played little part in the actual fighting. For once the Bemba had entered the village they could not fire them for fear of hitting their own fellows. If anything, the noise of the firing served rather to promote panic and confusion among the enemy who, caught unawares and dashing hither and thither in the half-light, were ready victims of the Bemba spears and axes.

But victory was not always instantaneous. A resourceful enemy might be prepared to resist. The Bemba would then withdraw to their camp and their bowmen would prepare strips of bark-cloth dipped in oil which they would light and fire into the village in the hope of setting it ablaze. If this tactic worked, the people would be slain as they fled to escape the flames. Alternatively, the attackers might settle down to a siege, hoping to starve the villagers into surrender. One such episode was described to me. The Bemba had made war on the Mambwe chief Chitongwa.[5] But Chitongwa's was a large village and not easily to be taken. The Bemba surrounded it, cutting off access to the gardens and streams. The Bemba themselves of course were able to live off the land. After a month the Mambwe had been reduced to starvation; they were forced to live on rats, dogs and even the bark-cloth in which the babies were carried. At length they sent forth an emissary to arrange the surrender. The Mambwe were instructed to file out of the village through a single gate. As they came through the men were seized and slain and the women taken into captivity. Chitongwa himself was taken, his body cut up and the pieces put in a pot and boiled.

Sheane (1911: 30) remarks that the Bemba avoided pitched battles whenever this was possible. This is very likely to have been the case. However, there is also more than a suggestion here that the Bemba lacked the tactical skills and courage for more open and more manly forms of battle. But this is sheer ethnocentrism, if not romanticism. The aim of warfare is victory and, whether they conformed or not to the canons of other cultures, Bemba methods were singularly well adapted to the conditions of terrain and social organisation then obtaining in the region, as their success shows. At the same time, it is important to recognise that they were not tactically limited to the surprise attack at dawn, but were alive to other possibilities when the circumstances required it. Thus in one unsuccessful encounter with the Mambwe two Bemba chiefs, Chilangwa and Mpangamina, were killed. Some time later Chitapankwa raised a large army and set off for Maswepa country to

seek vengeance. On this occasion the Bemba approached under the eyes of the Mambwe. Leading the advance guard were the young men, the *milumbamfwa*, with the main body well in the rear. To the Mambwe the troops appeared as mere youngsters and easy prey, and they came out of the village to advance to the attack. The *milumbamfwa* retired slowly according to plan, luring the Mambwe forward, and then as they closed to attack, the *mipamba* and *misoka*, held back in position in the rear, fell on them from the flanks and utterly routed them. It is also worth recalling here Sheane's own observation (1911: 30) that the most important engagement in which the Bemba were ever involved, Chitapankwa's last battle with the Ngoni, which ended with the final withdrawal of the Ngoni from Bemba country, though it began with the usual attack on the village, finished up as a pitched battle.

Bemba tactics were adapted to local conditions, and it would seem that it was only in response to the challenge of the Ngoni that they found it necessary to mobilise vast forces. For some twenty years the Bemba had been under pressure from the invaders, an offshoot of the martial Zulu, and when at length in one attack on the village of the Bemba chief Makasa the Ngoni killed Mukukamfumu, the sister of Chitimukulu, it required, as Roberts says (1974: 205), 'the massive, if temporary, alliance of chiefs from all over Bembaland' to enable Chitapankwa to exact vengeance and secure the final victory. In the later campaigns against the Mambwe Chitapankwa was supported by Mwamba, Nkula and other chiefs, but for the most part, and especially after the withdrawal of the Ngoni, the Bemba were confronted with enemies whose defence was organised on a village basis, against whom large-scale concerted action would have been militarily wasteful and pointless.[6] This situation may well have had an important bearing on the later development of the Bemba political system, a matter to which I will return later.

VI

Military objectives

At various points in his discussion Turney-High draws attention to the contrast between the warrior and the soldier. The warrior is a representative of a society that lacks what he calls the 'military horizon', that is a society that may possess adequate weapons, but is weak on teamwork, organisation working along certain simple principles (1971: 23). Soldiers, on the other hand, belong to societies that have crossed this threshold. The warrior is essentially an individualist for whom deeds of personal valour in warfare bring honour or glory. As among the American Indians, to quote Turney-High again, the warrior almost always yields to the temptation to accomplish useless small victories, the slaughter of one man or the crushing of one small party so that often no real advantage is acquired by the victor nor permanent injury done to the defeated. The soldier too is expected to display personal valour, and may be rewarded and honoured for acts of bravery, what sets him apart from the warrior, however, is that he is subject to discipline—he must learn to subordinate his personal inclinations to tactical considerations and the interests of the group as defined by his commanders.

In this sense the Bemba were soldiers. Martial values were certainly a part of Bemba culture before Chitapankwa: Gamitto (Cunnison 1960: 2: 160) speaks of their favourite diversions as drumming, war dances and also songs with which they

celebrated victories won in war. On the other hand, all my informants were in no doubt that it was Chitapankwa who really developed their war-like character to the point where the reputation they gained then still distinguishes them among the peoples of Zambia. I have already mentioned the measures by which military virtues were inculcated in the very young. Under Chitapankwa bravery became a prime social value; acts of valour in war were rewarded by gifts from the King and respect among the people. The man who returned with wounds in his chest was acclaimed; the man who was wounded in the back was punished. Cowardice was not tolerated.

What turns warriors into soldiers is discipline, a development which is related to changes in military objectives. Turney-High (1971: 105) illustrates the point with reference to the rise of the Zulu. Before 1820, he points out, conquerors and conquered intermingled, and there was no memory of real bloodiness. They thought they had performed feats of great valour if they killed two or three of the enemy. The arrival of the Zulu changed all that. The Zulu message was: spare no one except perhaps female captives. In consequence the Zulu terrorised the region and were thus safe from attack themselves. The Bemba appear to have been no less ferocious. Sheane (1911: 23) reports an episode from the wars against the Bisa when the Bemba 'wreaked their vengeance upon Matipa and his people until they were tired of slaughter, and long after the Bisa had acknowledged themselves the slaves of Mwamba'. In similar vein I have referred earlier to the engagement with the Mambwe which also finished up with systematic slaughter despite the fact that the Mambwe had surrendered. Warfare conducted in this fashion was no longer a matter of petty raiding, to pillage or to take booty in the form of cattle. No doubt slaves, who could be sold to the Arab traders, were an important prize of war, but warfare was now tied to more clearly political aims—the conquest, control and settlement of new areas.

VII

Warfare and the Bemba polity

I observed at the outset that warfare as a subject of enquiry in its own right has been much neglected by social scientists It is hardly surprising then, as Andrzejewski (1954: 1) points out, that, with certain notable exceptions, they have ignored the importance of military factors in the shaping of society. Students of the societies and cultures of south central Africa have provided no exception to the general rule (though see Wilson 1939). The omission is particularly striking in regard to a people like the Bemba; all the ethnographic accounts attest to the strong military traditions of the tribe but curiously, Sheane's article apart, they tell us little of their modes of warfare, and even less of the possible relevance of warfare to an understanding of their polity. It is true that Audrey Richards, our leading authority on the Bemba, does point out (1940: 106) that command over the army and over the supply of guns lay in the chief's hands and that the greatness of the *Bena Ngandu* rested to a great extent on fear. However, only a few pages later she tells us that there was no general military organisation within the tribe, though attached to each big court were one or two captains whom she refers to as *bashika*. She then goes on to conclude, it seems to me in complete disregard of her own evidence, that the inte-

gration of the tribe depended chiefly upon the sentiments of tribal cohesion and loyalty to the Paramount, sentiments which rested in turn on the 'dogmas of kinship'. In subsequent analyses of the Bemba political system, Richards (1960; 1968) was more concerned with the 'ideology of divine kingship' and stresses rather the mystical power of the paramountcy and the means by which it was maintained. However, towards the end of her Henry Myers lecture, she does point to changes in the bases of the King's authority, and the increase in his material power, following the arrival of the Arab traders in the latter half of the nineteenth century. 'The paradox of the benign man-god and the king with his cruel executioners may have become more and more evident in these years' (Richards 1968: 34).

Stated in rather different terms, the Bemba Divine King(s) had also become army commanders, with considerable powers of physical coercion at their disposal. It seems worthwhile to try and examine the implications of this situation even if, given the paucity and inadequacy of the data, any such analysis must include a good deal of speculation. My general argument amounts to little more than the development of an observation by Andrzejewski (1954. 29) that success in war, more than in any other human activity, depends on co-ordination of individual actions, and that the larger a group, the more necessary is the co-ordination, and the more complex the hierarchy required. Under certain circumstances, military objectives change. Where this involves a stepping up of military activity, particularly of operations of offence as against defence, this is likely to set in motion a process that gradually generates a momentum of its own. The more successful the effort, the more difficult the task of consolidating that success and extending the range of conflict without an increase in the scale of military organisation. Such an increase in scale contributes in turn towards an increasing hierarchy and concentration of power at the centre However, there may also be countervailing forces at work which set limits to the process and even set it in reverse. It remains now to show these processes at work in the Bemba context.

Despite Roberts's admirable pioneering efforts, there is much in the Bemba historical record that remains obscure, particularly for the earlier periods. What does seem clear, however, is that Chileshye Cepela's assumption of the title of Chitimukulu somewhere around 1820 marked a turning point in that history. His accession in fact appears to have coincided with the incursion into the region for the first time of Arab and Swahili traders moving into the interior from the East Coast; they reached the capital of Mwata Kazembe in 1831. Although they seem not to have entered Bemba country at this time, it appears not unlikely, as suggested earlier, that the situation thus created, and the opportunities it provided for trade, stimulated the Bemba to seek control of the trade-routes which lay beyond their traditional boundaries.

Some of my informants spoke of Chileshye as a man of gentle disposition, not a ferocious, war-like character like Chitapankwa. For all that Chileshye was involved in a good deal of fighting: against the Lungu, for example, which resulted in part of the area known as Ituna coming under Bemba control. But in other campaigns he was not conspicuously successful. In one excursion against the Ngonde (of contemporary Malawi) the Bemba were repulsed and many of their men were captured and taken into slavery (Wilson 1939: 27, 62), while he experienced continuing difficulty in coping with the invading Ngoni. As we have seen, Gamitto

refers to the Bemba conquest of Bisa country, but Chikwanda, whom Chileshye had sent to govern there, was not able to maintain the earlier gains. It was not in fact until Chitapankwa's reign that the Bemba fully established their hegemony in the Mpika District and were also finally able to drive out the Ngoni. Such facts may be taken as pointing to Chitapankwa's greater skill as a military leader and/or as evidence of the more effective machine he had been able to build up. Even so, it is important to recognise that Chitapankwa was only carrying forward a 'programme' that had earlier been initiated by Chileshye. That political rather than more purely 'individual' considerations are primary here is, I think, supported by the fact that Chileshye's successor, Bwembya, was quickly removed by the *Bakabilo*, and Chitapankwa installed in his stead: Bwembya was regarded as a fool—i.e. a weak man; still faced with the Ngoni threat, the *Bakabilo* presumably wanted someone who would provide strong leadership and build on the foundations that Chileshye had laid.

There is no evidence that Chileshye was an innovator so far as military matters were concerned, and I think it is unlikely. But insofar as some of his successes led to the taking over of new territory he set events upon a new train, generating tendencies both centripetal and centrifugal. The gradual expansion of Bemba power was not simply the creation of a new political entity with vastly extended boundaries; the process by which this was achieved fed back on and considerably influenced the character of Bemba society.

To begin with, the acquisition of territory by conquest poses a number of problems for the conquerors. How should the new areas be administered? A related question, bound up with the first, concerns the policy to be adopted towards the defeated enemy. The first extension of territory beyond the traditional 'homeland' actually occurred in the reign of Chiliamafwa (? c 1790–1820) when the chieftainship of Makasa was established in what had hitherto been Mambwe country. Makasa was a son of Chitimukulu. He appointed the defeated Mambwe leader as one of his councillors and left parts of the conquered area under the control of a Mambwe headman whose sister he married (Roberts 1974: 100). By contrast, the first Bemba ruler in Bisa country was of the royal lineage, the first chief Chikwanda, but in the major period of expansion following the defeat of the Ngoni by Chitapankwa, the new chiefly titles were allocated to the sons of Chitimukulu and Mwamba. This may have been in part a response to the lessons learned among the Bisa to which I shall refer again shortly, but there is also no doubt that the appointment of sons within the framework of a matrilineal system was an important factor in consolidating the power of the great chiefs. The *bana ba mfumu* were of course excluded from the succession to the more senior titles and, although they enjoyed a certain measure of autonomy in their areas, they remained in important respects dependent upon their royal fathers. This is seen very clearly for example in relation to access to trade goods. As power shifted to the centre, Chitimukulu was increasingly able to insist that all dealings with the Arab traders were with himself, a prerogative which by all accounts Chitapankwa guarded jealously, and one which presumably was also congenial to the traders themselves. It was thus to the advantage of the new chiefs to ensure a regular supply of tribute to the capital, a movement which further enriched the King and strengthened his control.

The problem, however, was not simply who was to administer the new areas, but how they should be administered. The allegiance of the defeated to their conquerors is unlikely to be strong; they are likely to be unreliable so that such areas become centres of disaffection, prone to rise at the first opportunity. This was certainly so in Bisa country where Chikwanda ran into difficulties and in the end fell victim to a rebellion by his Bisa subjects. It is my guess that the ferocity and ruthlessness of the Bemba, notably absent in their earlier treatment of the Mambwe, but increasingly evident in the days of Chileshye and his brother Mutale (who was the first chief of Ituna to take the title Mwamba), and reaching its peak under Chitapankwa, was developed as a deliberate policy in response to this situation. After the taking of a village, the male captives were either sold into slavery or simply butchered. Women too were sold as slaves, but many were spared to be given as wives to the Bemba settlers. Although most of the peoples concerned followed the rule of matrilineal descent and succession, what happened in fact was that the offspring of these unions grew to identify themselves with their more prestigious and conquering fathers. In this way very large areas were very rapidly and effectively Bembaized. Roberts's claim (1971: 21) that, unlike the Lunda of Kazembe, the Bemba polity was not a conquest state can only be true in the limited sense that the Lunda Kingdom was characterised by the ethnic and cultural diversity of its people who were united only in their acknowledgement of Kazembe as their King; among the Bemba the conquered were successfully merged in a common Bemba identity.

One way in which conquest has often been noted to affect social structure is in regard to stratification. Rank always appears to have been an important aspect of Bemba social organisation, and this almost certainly acquired a greater importance with increasing military success. Major Wissmann (quoted in Gouldsbury & Sheane 1969: 22) states that among the Bemba there existed, in 1887–8, a perfectly developed rank system, determined by the number of heads of the enemy secured in battle by each warrior. This is probably a gross exaggeration, but it is likely to contain a kernel of truth in that military values provided a framework in terms of which all sections of the population could measure their relative social position. At the top of course was the King, the epitome of those values and the lynch-pin of the whole system, followed by the aristocracy from among whom he sought his confidantes and advisers. The position of Bemba commoners was itself elevated by the presence of the newly subject peoples and slaves. However, unlike certain situations elsewhere in Africa, Bemba conquest did not give rise to a system of castes based on the dichotomy of conquerors and conquered. On the contrary, Bemba society was in certain respects a very open one in which military prowess and distinction offered considerable possibilities for social advancement. As I have just mentioned, Bemba policy favoured the emergence of a common identity and, as previously noted too, even a slave could rise to be a *mushika*. Such possibilities for upward mobility served to perpetuate the military ethos and of course contributed at the same time to consolidating the position of the King as the fount of all patronage.

I am inclined to believe that the development of the military ethos also had an impact on the Bemba kinship system. In a well-known paper Richards (1934) describes the balance of rights and obligations as between matri- and patrikin; but

she does not consider how far this pattern may have developed in response to the factors I have been describing here. As we have just seen, military prowess and the way this enabled a man to gain the attention of a chief counted no less, and possibly a great deal more, than descent in determining one's position in society, and this would have been particularly the case in those areas that had recently come under Bemba control. Richards (1934: 270) records that in tracing pedigrees a man would remember on his father's side fewer or more relatives according to the latter's rank. It may also be worth mentioning here that African research assistants of the former Rhodes-Livingstone Institute who had been conducting surveys in the Northern Province once told me how struck they were by the minimal role of descent groups in Bemba social life compared with neighbouring areas in which they had worked. Such findings are entirely consistent with the material and argument I have presented.

VIII

I have been arguing then that Bemba warfare passed through a number of phases. The Bemba may always have fought with or raided their neighbours. But once they started to acquire new territories warfare took on a new dimension which had profound implications for their political and other institutions. Military success breeds success. Morale among the troops is high and as the opportunity for booty and prizes increases they are increasingly tied to their leaders. A Bemba chief is ideally a bountiful chief, and a chief who is also a successful military leader is in a particularly happy position to fulfil this expectation; he gives as gifts what his men have captured or rewards them with women or the goods supplied by the traders. In this way power becomes increasingly concentrated in fewer hands

There is much here that is reminiscent of the rise of the Zulu state, though there are also important differences Walter (1969: 259) points out that having changed from a country of herders and peasants to a primitive garrison state, Zululand was subdivided into command areas, in which military *nduna* shared authority with territorial chiefs, who were often royal kinsmen. These *nduna*, it should be emphasised, were commoners who owed their appointments and their futures exclusively to the King, without independent access to power, wealth or honour. The Bemba were unable to achieve this separation of chiefly, administrative and military functions, and in developing their political system they were required to make much more use of kinship bonds. But this worked rather differently from the way Richards seems to have envisaged it. Thus Roberts (1974: 124–5) has drawn attention to the importance of Chileshye's achievement in extending the influence of his own lineage at the expense of other branches of the royal clan. He also indicates how this development was carried even further by Chitapankwa, so that at the end of the latter's reign the Miti lineage had gained control over almost all the older chieftainships. Werbner (1967) has discussed the complex pattern of relationships between various Bemba lords to which the new situation had given rise, but the point I would like to stress here is the way in which growing command of physical force, based on military success, enabled Chitapankwa to pursue a 'career of upsetting most traditions of succession' (Brelsford 1944; cited in Werbner 1967: 30). Such power clearly also underlay the fear that was a marked feature of Bemba

attitudes towards their chiefs to which Richards has repeatedly referred, and which was still evident on occasion at the time of my own fieldwork.

At the same time continued military success also brings fresh problems in its wake. Apart from the administrative issues discussed earlier, it may generate internal stresses or exacerbate existing tensions. The richer the prizes the more intense the struggles to become favourite of the chief, and the greater the opportunities for intrigue around the court. The war machine thus acquires a momentum of its own, and I would conjecture that by the time of Chitapankwa, and after the expulsion of the Ngoni, continued Bemba expansion was at least in part a way of solving the problems of internal friction; sons who might otherwise prove troublesome around the court were given scope for their energies and ambitions by being sent out to govern the newly acquired territories. All of this in turn required a more elaborate and more sophisticated command structure.

The most evident aspect of the increased scale of military organisation, I have suggested, lay in the changing character of the *musumba*. Under Chitapankwa it appears to have taken on something of the character of a garrison community. But under local conditions of subsistence, to supply the needs of such a large population must have been a very considerable problem indeed. No wonder that when taxed with the question by early visitors, Bemba would point in the direction of Mambwe country and say 'Our gardens are over there'—a reference to the raids carried out on the Mambwe in order to seize their cattle. But raiding apart, which can hardly have satisfied the demand, the growth of the *musumba* must have intensified the need for tribute which would have generated demand for a much more systematic form of administration to ensure a regular food supply to the people of the capital. Gouldsbury and Sheane's claim (1911: 21) that the system of administration was extremely well organised and was upheld with the utmost rigour clearly harks back to the reign of Chitapankwa.

Yet when full allowance has been made for Chitapankwa's considerable achievements in binding so many disparate groups into a single polity, the significant fact that remains is that he did not succeed in creating the machinery of a unitary state that he could bequeath to his successors Indeed, within a short time of his death the fabric of the kingdom was being rent by serious internal divisions. The reasons for this are complex; in part they relate to the particularities of Bemba politics of the day (see Roberts 1974: 215), but they also point to a more general problem that might repay comparative analysis. A full discussion of these issues would go far beyond my present purpose but two points may be worth mentioning in so far as they arise directly out of the analysis I have sought to develop in this paper. First, I would like to suggest the possibility that, paradoxically, political failure owed much to the fact that militarily the Bemba had in a way been too successful. Once the Ngoni threat had been removed, there was no enemy to confront them that required the mobilisation of the entire people; to that extent there was increasing scope for local autonomy and independence, and the play of centrifugal forces. This links directly to my second point. For the relations between Bemba chiefs were also likely to have been affected by changing trade routes. As the East Africans consolidated their position in the north west, Ituna was likely to develop as a power base at the expense of Lubemba.[7] Again, the obvious areas for expansion in later days were to the south and west, reaching out beyond the borders of Ituna and

more amenable to the influence of Mwamba. Perhaps it was some perception of these possibilities that led Mwambe II to decline the Chitimukuluship in favour of his younger brother Chitapankwa. But whether this is so or not, it would seem in retrospect that such a development must have had profound implications for the balance of power. As Roberts points out (1974: 216), well before 1883, when Chitapankwa died, Bemba political ties had polarised around the two leading chieftainships of Chitimukulu and Mwamba. On the death of Chitapankwa, followed only a little later by that of his nephew Mubanga Chipoya (Mwamba III), the way was open for a bitter struggle between their successors. When towards the end of the century the British moved into the area, although the Bemba were still engaged in waging war, the signs of internal division were all too evident. Unlike the Ngoni, whom they had earlier defeated, the Bemba yielded to the incoming whites almost without a struggle.

NOTES

Much of the material presented here was collected in the course of two very short visits to Bemba country in 1953–4, when I was on the staff of the Rhodes-Livingstone Institute The information was gathered more by chance than original design, emerging out of discussions with Bemba Councillors and notables in the areas of Chitimukulu, Munkonge and Nkula. I have no doubt that had I been more alive to the issues at that time, a great deal more in the way of relevant and valuable data could have been obtained. A first draft of the paper was given to an African History Seminar at the University of Sussex, and I am grateful to Richard Brown and the members of the seminar for their comments. I am indebted to Dr Andrew Roberts, SOAS, on a number of counts· for coming down to Sussex specially for the seminar, for letting me have detailed comments on a later draft, and for making available to me a number of papers of whose existence I had not known. Needless to say, his recent book *A history of the Bemba* has been indispensable in the preparation of this article. I am also grateful for comments by Dr Audrey Richards. I would like to take this opportunity to pay tribute to Dr Richards who personally, through her teaching, as well as through her writings, gave me my introduction to the Bemba.

[1] Cf. Wills (1973 81) who refers to the advance of the 'gun frontier' altering the balance of power, giving dominance to those tribes supplied with firearms; he adds that the Bemba could not have withstood the Ngoni onslaught in 1856 without weapons supplied by the Arabs.

[2] I have no record of having encountered the terms *Wasimupelo* or *Balashi* in the field, which may have passed out of current usage. Nor did I find the term *bashicalo* used for ordinary village headmen. Dr Richards advises me that *bashicalo* refers rather to a category of headmen who have special ritual duties as spiritual owners of the land.

[3] Unfortunately I omitted to explore the meaning of this term further. However, the White Fathers' Bemba dictionary (1954) notes that *kupinde linga*, which ordinarily means to build a fence, also has the special meaning 'to besiege a village'.

[4] On the whole question of the size of *musumba*, cf. Roberts 1974: 166.

[5] A number of Mambwe chiefs had allied themselves with the Ngoni. With the latter out of the way, the Bemba took the opportunity to settle some old scores. For an account of the Mambwe campaigns, including the episode described here, see Labrecque (1968 314–20) The taking of Chitongwa's (Mpande V) village is also mentioned by Roberts (1974 146)

[6] I have come across only one reference in the literature to the size of force the Bemba committed to a particular campaign. In an expedition into Lungu country under Chitimukulu, Sampa and Makasa in 1893, the Bemba encountered von Wissmann, the Imperial Commissioner for German East Africa The Germans estimated the Bemba force at around 5,000 (see Roberts 1974: 234). This was long past the heyday of Bemba expansion with which I am principally concerned in this article

[7] It is interesting from this point of view that when Tippu Tip, perhaps the most important of these traders, refers to his friends, chiefs of the Bemba, he does so in the order Mwamba, Kitimkaro [Chitimukulu] and Shanza [Nkula] (see Roberts 1974: 195).

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RASTOGIS AND SECRETOR STATUS

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ABH secretion studied in a sample of Rastogis living in Lucknow shows secretor frequencies similar to those previously published for Bengali and Jat but quite different from other caste groups of Uttar Pradesh.

In India, the Hindu caste system takes its origin from the system of four Varna, 'Chatur Varna Vyavastha' (Prabhu 1954). Almost every caste (Jati) traces itself from one of these Varnas. Though initially the Jatis were clearly attached to Varnas, due to intermixture among castes many new Jatis have emerged, and indeed are still emerging. There are a few Jatis which are not clearly assignable to any Varna; their status, in the Varna framework, has been differently defined by different workers. Examples of such unresolved cases are Kayasthas and Rastogis.

Conflicting claims have been made regarding the origin of the Rastogis and their place in Chatur Varna Vyavastha. Most informants believe that the Rastogis belong to Suryavanshi, a division of Kshattriya Varna (Todd 1925). Their name is thought to be derived from 'Rohtas' ('Rohit' meaning sun, 'aas' meaning Vansha). Rohatagi (1953) however, claims them to be descendants of the mythical king 'Harishchandra', who had a son named Rohit from whom the name Rastogi is derived, and the caste is also sometimes known as Rohatagis. Still another view (Crook 1896) is that they are a subgroup of Vaishya Varna, a view probably based on Nesfield's (1885) theory of the occupational basis of the caste system; Rastogis are traditional money lenders and business men. Of these the second appears to be the most tenable theory, that the Rastogis are the descendants of the mythical king Harishchandra. Some eleven centuries ago they established their rule in three places, one to the south of Kashi on the banks of the river Som, a second in the Punjab to the west of Indraprastha, and a third at Kannauj. Of these, the first extended their kingdom further to the southeast and retained the name of their ancestral prince Rohit in the name of their capital Rohtasgarh. The second who migrated to the Punjab named their kingdom Rohtak also after the prince Rohit, while the people who migrated to Kannauj and ruled up to the fifth century A.D. later became known as Rothari Rajputs.

Today the Rastogis are endogamous, and gene flow into their genepool from outside is restricted. Internally marriage among them is regulated by 'gotra' exogamy. In view of the conflicting claims as to their origin, it is important to establish their caste ranking. For this purpose the gene frequencies of Rastogis, compared with those of the other groups of Kshattriya and Vaishya Varna with whom affinity has been claimed, may be of relevance. The present study examines the ABH secretor status, a well-established genetic character controlled by a single

Man (N.S.) 10, 218–220

pair of alleles, in Rastogis of Lucknow and compares the results with those from other populations, to see whether useful indications of affinity emerge.

On 200 unrelated Rastogis, 100 males and 100 females, living in different localities of Lucknow, ABH secretor status was tested, using the inhibition technique of Dunsford and Bowley (1955).

RESULTS

The results (table 1) show that the majority of Rastogis are secretors. There is no significant difference between the male and female sample ($\chi^2 = 0{\cdot}61$). The results by ABO blood group are set out in table 2, and as expected there is no significant difference between blood groups according to secretor status.

Data from other relevant caste samples are set out for comparison in Table 3.

TABLE 1. Secretor status among the Rastogi series.

Group	*Total No*	*Number observed*		*Gene frequency*	
		Secretor	*Non-secretor*	*Se*	*Se*
Male	100	69	31	·443	557
Female	100	74	26	490	·510
Total	200	143	57	·457	543

TABLE 2. Secretor status of males and females with reference to blood groups in Rastogi series.

Groups		*Blood groups %*			
		O	A	B	AB
Secretor	Male	16 0	15·0	37·0	1·0
	Female	11 0	17·0	39·0	7·0
	Total	13·0	16·0	38·0	4·0
Non-secretor	Male	7 0	7·0	16 0	1·0
	Female	7 0	7·0	16·0	1·0
	Total	7 0	5·5	14 0	2·0

TABLE 3 Comparison of secretor and non-secretor status of Rastogis with the other castes of U. P.

Population sample	*Number tested*	*Secretors no %*	*Non-secretors no. %*	*Diff from Rastogis* χ^2	*Probability*	*Author*
Bengalis	414	277 (66 91)	137 (33 09)	1·328	NS	Buchi 1954
Banias	580	476 (82·07)	104 (17 93)	9·64	·01 > P > ·001	Anand 1960
Sindhis	160	137 (85·62)	23 (14·38)	10·59	·01 > P > ·001	Anand 1961
Jats of Delhi	105	76 (72·3)	29 (27·7)	·0266*	NS	Chattopadhaya 1963
Khattri	2629	2156 (82·01)	473 (17·99)	12 80	·001 > P	Seth 1968
Aroras	2596	2175 (83·78)	421 (16·22)	18 91	> 001 > P	*Idem*
Rastogis of Lucknow	200	143 (71·50)	57 (28·50)			Author

* ·0009

Including the present Rastogi sample, overall there is highly significant heterogeneity ($\chi^2 = 88 \cdot 4$, 6 degrees of freedom). Comparing the observed number of secretors and non-secretors in each in turn with those currently observed in the Rastogi sample gives the values of χ^2 set out in the table. There is no significant difference of the Rastogi frequencies from those in the Bengali sample of Buchi (1954) or from the Jat sample of Chattopadhayay (1963). Highly significant differences emerge from the Bania, Sindhi, Khattri and Arora sample. The similarity of Rastogi and Jat may perhaps be interpreted as indicating biological affinity, but the differences from the other Kshattria groups are then more difficult to explain and so is the similarity to the Bengali sample. It is clearly necessary to take into simultaneous consideration a large number of genetical traits in tracing the affinity of this and other populations.

NOTE

The authors are grateful to Dr K. S. Mathur, Head of the Anthropology Department, Lucknow University, for providing facilities and to Dr B R. K. Shukla for his valuable suggestions.

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SPIRITUAL RELATIONSHIPS AND SELECTING A GODPARENT

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Founded upon church rites, the ties of the *compadrazgo* differ from other relationships in that their irreducible 'substance' is spiritual affinity. As the bonds are woven into a social system, however, they assume secular functions. Although these practical tasks of the complex vary by society, they should be comprehended in part as a manifestation of the sacred relationship, for the spiritual backing legitimates, transforms, and confers a special meaning upon their secular nature, just as they provide one realisation of the spiritual This interplay of the sacred and the social in the *compadrazgo* can be seen in the rules for choosing godparents. One norm, a prohibition upon parents sponsoring their children, is now a church rule, but it seems to have arisen from folk practices A second, the prohibition on choice reversal, appears to be a folk deduction from church theology. Both selection patterns bear a similarity to those of asymmetric alliance.

The *compadrazgo*, considered in its most general form cross-culturally, consists of three personages—a parent, a child, and a godparent—linked in several relationships. Parent and child are kin, while the godparent is tied to the child and the parent by bonds of spiritual affinity. In a previous paper I described the theological underpinning of the complex, its ideological relation to the family, and the variations which the core structure of the *compadrazgo* undergoes (Gudeman 1972). Here I extend the analysis by considering two further problems. The first concerns the nature of the godparent and co-parent ties. What is distinctive about these bonds and in what way do they differ from kinship relations? Are the ties merely an 'idiom' for other relationships, or do they have an irreducible element of their own? I shall argue that the *compadrazgo* is at once a sacred and a social set of relationships having a distinctive character not reducible to other kinds of bonds

The second problem I consider concerns cross-cultural regularities in parental selection of the godparent or co-parent—the initial step in the formation of the *compadrazgo*. Viewed cross-culturally, the choice patterns appear to exhibit great variation. In some cultures a parent is not permitted to select his own child as co-parent, in others he may. Parents in some societies must choose the same person as godparent for all their children, for all their same sex children, or for all the religious rites through which any one of their children passes; in others they must select a new godparent for each child and each ceremony. The sex and number of godparents may or may not be prescribed; and in addition to these and other folk differences, the church regulations themselves have varied over the course of two millennia. Despite this diversity, I shall suggest that at least two patterns of godparent selection characterise the *compadrazgo* in contemporary cultures; one is an inflexible rule; the other is a series of variations that seem to be correlated with, though not

entirely caused by, the broader social context in which they are found. These two patterns are:

1. Parents universally are prohibited from choosing themselves as godparents.

2. Within a culture, parents are constrained by one of three selection forms: choice reversal may be prescribed, permitted or prohibited. The first rule provides for a symmetrical relationship between parent and godparent while the latter can lead to an asymmetric bond.

In analysing why parents are prohibited from choosing themselves, I shall ask whether the rule is an integral part of church dogma or whether it has important social effects. To arrive at a better understanding of the norm I examine its evolution in early church law, for though baptism is said to have been instituted by Christ, it was not until the ninth century that the church explicitly prohibited parents from sponsoring their own children. The second choice pattern is examined in the context of current cultures. Here, the problem is somewhat different. Choice reversal from godparent to parent seems to be linked to a symmetrical relationship between the two. Conversely, the prohibition upon choice reversal, or the non-reciprocated form of the co-parent relationship, is frequently associated with an asymmetric bond between parent and godparent, and when parent and godparent occupy different status positions, the latter normally holds the higher. Furthermore, both the overall choice patterns appear to be structurally similar to certain rules of marriage alliance: on the one hand, the prohibition upon parents sponsoring their own child seems much like a rule of exogamy; on the other, reciprocal and non-reciprocal selection bear a resemblance respectively to 'bilateral' and 'matrilateral cross-cousin' marriage. Hammel (1968), in fact, has already drawn attention to the similarity between asymmetric alliance and the non-reciprocated form of the godparenthood complex, as it is found in the Balkans. One purpose of this article, then, is to ask how useful the 'alliance model' is for the *compadrazgo*; what does it explain about the choice patterns and the asymmetry of the relationships?

It is at this point, however, that the second problem concerning choice is joined to the first concerning the nature of the bonds. I shall suggest that the godparent choice regulations reflect both the sacred and the social sides of the complex. Let me put the argument in the form of a paradox. The first choice rule is a church norm, yet the norm itself has a social origin; the second set of rules are purely folk regulations, but some of the folk regulations are logically deducible from ecclesiastical ideas. Situated between the church and society, the fundamental selection rules of the godparenthood complex reflect their dual origin.

I

To reach an understanding of the nature of godparenthood bonds, I describe in the first part of this section their structural origin, different forms, and relation to kinship and affinity. Then, I turn to their functions and content. Since the worldwide variegated forms of the complex are united by their common tie to the church, my discussion focuses primarily upon church ideas.

Theologically, a distinction is made by the church between bonds which are natural and those which are spiritual. Natural bonds originate in carnal intercourse

and are of two types, consanguinity and affinity. Spiritual relationships, by contrast, have their origin in ecclesiastical law. By current canon law spiritual relationships are defined as those introduced at the reception of baptism and confirmation.

Through the ages the spiritual relationships recognised by the church have varied, but the central relationship has been spiritual paternity. It is divided into a direct and indirect form. The bond between the minister and baptised (or confirmed) and/or the tie between godparent and baptised (or confirmed) is known as direct paternity. (Today these are the only relationships recognised by the Roman Catholic Church.) Indirect paternity designates the tie between the spouse of the minister or sponsor and the baptised. By contrast, direct co-paternity refers to the link between, for the one part, the minister or sponsor and, for the other, the parents of the baptised. The relationship between the spouse of the minister or the sponsor and the parents of the baptised is known as indirect co-paternity. Finally, spiritual fraternity designates the relationship between the carnal children of the minister or sponsor and the baptised. At various historical times this bond was extended to all the children of the two co-parents, or at least to those born after the sacred ceremony.

For purposes here it is important to recognise that spiritual paternity and spiritual co-paternity are different relationships. Historically, direct paternity preceded direct co-paternity. But though the two bonds are ideologically distinct and though historical church laws may have applied to one but not the other, the two relationships are not independent. Ecclesiastical laws about co-paternity may have direct implications for or be used to define the nature of spiritual paternity.

Spiritual relationships are not linked intrinsically to sacramental events, and in this regard an interesting comparison may be drawn between spiritual bonds and baptism, on the one hand, and kinship and 'biological facts', on the other. Theologically it is normally argued that baptism is a regeneration and a rebirth to the spiritual life, just as birth marks the beginning of the natural life. Therefore, just as two adults contract a natural tie with those who are born to them, so also they form a spiritual bond with those who are reborn to them, or with those at whose baptism or confirmation they assist. God is the author of the tie. In the current law, for the valid and licit appointment of a sponsor, certain conditions must be met. The godparent himself must be baptised, know the rudiments of the faith, be at least fourteen years of age, and not be in a religious order or excommunicated. The spiritual relationship is not contracted if the sponsor does not have the requisite qualifications, even if the sacramental ceremony is validly performed. In 'Fortesian' language we might say that the godparents must have the 'proper credentials' for the tie of spiritual paternity to be recognised. Yet, this observation points to the fact that although God may be the author of the bond, spiritual relationships are a creation of church law. They are no more natural or given than kinship ties, even less so. For example, in the early centuries confirmation was not distinct from baptism and therefore did not give rise to spiritual relationships. Even baptism at first did not give rise to a bond between baptised and sponsor, while the evidence is contestable that a spiritual relationship, as now understood, existed between minister and baptised in Apostolic times (I St. Peter V, 13; II Tim., II, 1; Philem. 10). Eventually, however, spiritual ties became recognised not only over confirmation but penitence and the catechism, while Don Alfonso the Wise in his *Siete Partidas* (1931: 426) has a delightful passage explaining the obligations which new

knights bear towards their godfathers who ungird their swords.[1] By contrast, the church also recognises baptism of desire and baptism of blood, in addition to baptism of water, and 'metaphorical baptisms' such as the blessing of bells and ships have long been practised, but none of these baptismal forms has given rise to spiritual relationships. And, this is not to consider all the folk forms of metaphorical baptism which do give rise to spiritual relationships in the eyes of the people if not in the view of the church. In other words, baptism is not linked of necessity in a one-to-one relation with spiritual paternity any more than the physical facts of intercourse, conception, pregnancy and birth naturally and automatically give rise to natural filiation. Spiritual relationships may originate with God but they must be recognised by the church, and what the church has chosen to recognise as the starting point for the bonds has been variable.

Marital impediments are not themselves a defining feature of godparenthood bonds, but the two are closely linked, and a consideration of such prohibitions helps uncover the nature of the ties. Church impediments are legally of two types. A prohibitory or impedient impediment is a grave prohibition imposed between two persons who wish to marry; it does not, however, render such a marriage invalid. A diriment impediment both prohibits a marriage between two persons and renders it null and void. Where a diriment impediment exists no marriage may be contracted validly. The impediments which pertain to cognation, to affinity, and to the godparenthood complex are all of the diriment variety. Since an impediment prohibits the establishment of a marriage, it is, in a sense, the converse of affinity. An impediment is a rule which prohibits the imposition of new affinal relationships upon already existing bonds. It is a 'taboo' which marks the boundaries of a category and prohibits the confounding of incompatible relationships. Thus, both consanguinity and affinity give rise, in differing degrees, to marital impediments; new affinal relationships are not allowed to confuse the already existing bonds. Similarly, some but not all spiritual relationships give rise to marital impediments; through the agency of these impediments affinity is prohibited from intruding into the domain of spiritual ties. I shall return shortly to the many ways in which the recognition of impediments has been used historically to distinguish spiritual relationships from affinity.

Since some marital impediments separate affinal relationships from those of cognation and of spirit, it may be asked how in turn consanguinity and spiritual ties are kept apart, for it would appear that the imposition of marital impediments on consanguinity, affinity or spiritual bonds would have little effect upon prohibiting cognation from being transformed into spiritual relationships and vice-versa. In fact, the reverse is true. The recognition of impediments to marriage has been used in quite spectacular and complex ways to prevent spiritual relationships from being reduced to consanguinity, although other means also have been employed.

A consideration of the functions of godparenthood bonds brings us even closer to an understanding of their essential nature. Both cross-culturally and historically the functions of the complex have varied widely. From the church's standpoint the withering away of the tasks of godparenthood has been a source of some concern. In 1925 the Sacred Congregation of the Sacraments prepared a special reply to a petition and questions submitted by the Archbishop of Utrecht in which they

restressed that a sponsor must see to the religious education of his spiritual children and must regard them as being in his perpetual care. These duties were said to derive from the nature and origin of spiritual parenthood itself. The Sacred Conregation deplored the fact that 'Today when faith is growing cold, this sacred sponsorship established of old by the Church is despised or made little of,' but they also took note of similar observations made by prior synods, councils, and even the Council of Trent (1545–63): 'This office is so carelessly regarded in the Church today that nothing is left of it but the bare name, and men seem not even to suspect that there is anything sacred about it' (Bouscaren 1934: 344, 342). Anthropologists, by contrast, have long emphasised that the godparenthood complex may be used to fulfil a variety of functions, such as socialising children, providing emotional satisfaction, linking together people of the same or different social classes, providing aid in times of need, giving labour service, offering ceremonial assistance, providing housing in distant areas, and so forth.[2] Godparenthood has long been put to social uses, but it would appear that at times these functions have threatened to engulf its essential nature.

Why, then, can the over-exercise of secular duties or the under-exercise of religious functions threaten the *compadrazgo*? Of what is the complex composed? Is it an 'idiom' for other relationships or does it have an irreducible 'content' of its own, and if so what is it?

To grasp the nature of the godparenthood and co-parenthood bonds it is perhaps best to consider first the ecclesiastical notions Created by the church and ultimately by God, the ties are said to be enduring sacred bonds. This sacred backing gives the ties of godparenthood their compulsive, moral quality. Once entered they cannot be escaped, and the duties attached to them ought at least to be observed, though in fact they may not be. Analytically, then, godparenthood ties are spiritual relationships Two persons are linked, through their souls, by God. Yet, the concept of spiritual relationship is an abstraction, unrelated to a social context. To be felt and used by persons, godparenthood bonds must be bodied forth or expressed socially; and this embedding of spiritual affinity in a social context (whether as religious education for the godchildren within the church or as political patronage for the co-parent in the society) gives it content. Rather than viewing the 'idiom' as separate from the 'content' of godparenthood ties, I argue that the bonds have a distinctive spiritual quality which becomes attached to and provides a special meaning for certain social relationships. This cultural meaning must not be abstracted from the content.

Let me put it this way. For the sake of description we may distinguish two kinds of spiritual 'content'. On the one hand, nearly all godparenthood bonds seem to be marked by some form of ritual etiquette, such as distance or deference behaviour, exaggerated respect, forms of greetings, sexual avoidance, prohibitions upon drinking, and so forth. These 'immaterial' obligations are expressions of the underlying moral relationships; they are outer signs of the spiritual bonds. But anthropologists also have observed that the ties of the godparenthood complex may be used for explicitly economic or other purposes (see, for example, Foster 1961; 1963; Mintz & Wolf 1950). At this level, however, we must be careful to keep the complex in proper perspective and not subordinate its special nature to other relationship systems or functions. At times, spiritual relationships are deployed in

the economic domain, but when they are, the economic is to some degree 'spiritualised'. Let me illustrate with two brief examples from my own fieldwork in rural Panama. Two *compadres*, who were non-kinsmen, happened one year to be working adjacent plots. A single path, used by no-one else, led to both their fields. Ordinarily those who use the same path collaborate in cutting down the long grasses which grow up in the roadway during the wet season. As the agricultural year progressed, the parent-*compadre* dropped a few hints to his godparent-*compadre* that it was time to trim the path, but the latter did not respond. Time passed, the grasses grew and the father became angrier; on the one hand, he did not want to trim the pathway himself since it was not only his work neighbour but his *compadre* who shared the path with him; on the other hand, he was reluctant to force the issue precisely because his neighbour was a *compadre*. Eventually, the parent-*compadre* became so disgruntled that he stopped visiting his co-parent's house. We may, then, view this case relationship from two perspectives. From the standpoint of the economic domain, what was an ordinary utilitarian task became morally binding due to the spiritual legitimation; the parent-*compadre* was upset not because his neighbour refused to help—an all too common occurrence—but because his *compadre* refused. From the standpoint of the spiritual domain the *compadre-compadre* relationship was extended into the economic, but this intrusion began to threaten the bond itself.

My second example concerns the selection of friends as *compadres*. When friends are chosen, their relationship is said to become more formal and respectful; they must now avoid some of the mutual joking and drinking in which they might have engaged previously. Two friends also must cease using the informal 'you' between themselves and replace it with the formal form. The bond of friendship is not abrogated by the imposition of spiritual affinity, but it is changed; two friends who also are *compadres* will act differently from two who are not. Both these examples are adventitious in that in the first the *compadres* happened to be work neighbours in one agricultural year while in the second there is no requirement that *compadres* first be personal friends. In many societies, however, the spiritual bonds are mapped on to social relationships in a more determinate and permanent fashion, and what we need always to ask is what are the unique effects of the spiritual legitimation upon the social ties? How are economic or political relations transformed by the *compadrazgo*? More generally, why is a spiritual legitimation attached to some social functions and not to others, and why is a sacred as opposed to a genealogical, affinal or other relationship clothed with a particular social task? In each case we need information about the local cultural ideas which define and refine the essentially spiritual relationships and give meaning to the observed social bonds.

In short, the ties of the godparenthood complex are enduring spiritual bonds but in themselves they are 'intangible'. To become 'real', to become more than an idea, they must be put to use. Utilising the bonds, however, not only transforms them but the social relationships upon which they intrude. Inevitably the ties of the *compadrazgo* pertain to both the spiritual and social domains.

II

With this perspective, let us turn to the first selection rule, that which prohibits parents from choosing themselves as godparents. This prohibition is fundamental,

for it ensures that parent and sponsor are distinct—though meaningful only in contrast to one another—and that spiritual links are established with others from without the nuclear family. But why are parents prohibited from sponsoring their own child?

A current doctrinal justification might be as follows (see, for example, Abbo & Hannan 1960: 750). Man, descended from Adam, is born into sin. To be saved he must be reborn to Christ. Since the natural parents are tainted with original sin, others must serve as spiritual parents, hence the prohibition upon parents sponsoring their own children.

On purely logical grounds, however, I do not find this argument convincing. First, baptism and other church ceremonies are not intrinsically linked to spiritual relationships; conceivably a person could be reborn to the church in general and not to specific spiritual parents. (Indeed, in earliest times spiritual paternity was not even recognised between the baptised and his sponsor.) Moreover, parents and godparents are alike in that both have been born in sin and reborn to the spiritual life. Godparents are not intrinsically purer than parents, and it is conceivable that parents might sponsor their own children; in fact, under certain conditions they are allowed to to do so. Thus, while it is possible to justify the separation of natural and spiritual paternity on doctrinal grounds, it is quite different to argue that the same ideas caused or produced the prohibition on parents choosing themselves as godparents. The same ideas could also have caused the child to be reborn solely to the minister, to a saint or saints, to the church in general, to Christ alone, or to the natural parents

If, for elucidation, we turn to the early history of the prohibition an interesting fact appears: church practices exhibit a complete *volte-face*. In earliest times parents, almost without exception, did sponsor their own children; seven centuries later, however, they were explicitly prohibited from doing so. A consideration of this historical change may bring to light some of the reasons for the prohibition itself. The early history of sacred sponsorship, however, is somewhat obscure and an examination of the ecclesiastical regulations raises several historical problems. Was baptism in fact practised in the first centuries, and if so were children being baptised? What was the relation of the developing theology about baptism to the practice of having sponsors, and who was actually sponsoring children?

Some of these questions received consideration in a recent discussion between two biblical scholars. Jeremias (1962) held that not only baptism but infant baptism could be traced to Apostolic times In a direct attack on this thesis, Aland (1963) proclaimed that the first unambiguous evidence suggesting that *infants* were being baptised emerges only in the first half of the third century. The change in practice, he said, was linked to a change in theology. Tertullian in the early third century had argued that the newborn child was in a state of purity; later in the same century, however, both Cyprian and Origen had held that even a baby was caught in the sinfulness of all mankind. Also, about the same period there was a decline in the eschatological belief that the end was near. Thus, argued Aland, 'from the moment that the taint of original sin was believed to apply to the newborn child, its baptism became a necessity under the new presuppositions, for it could no longer be assumed that the Last Day would come in the lifetime of these children' (1963: 106). In a sharply argued rejoinder Jeremias (1963) reiterated his

claim that infant baptism was held in the Apostolic age. With respect to the doctrinal change, he argued that Aland had turned Origen's argument around. Origen had not argued from the theology to the practice; rather from the practice of infant baptism he had inferred that newborn infants must be tainted with sin. Concludes Jeremias (1963: 74):

> It was infant baptism which came first. It was the *lex orandi* which led the way; from it was deduced the *lex credendi*, the doctrine of the sinfulness of new born infants, not the other way around! It was not infant baptism which was the innovation, but the doctrine of original sin which was substantiated by it This doctrine supplied the *ex post facto* justification for the existing custom of infant baptism.

A similar view concerning the relation between practice and doctrine is stated by Williams (1927).[3]

Both Jeremias and Aland agree that baptism itself was held in earliest times and that, in fact, children were baptised. Their disagreement concerns whether infants also were baptised. Moreover, neither disputes the fact that parents at first sponsored their own children, and in this regard the earliest sources all seem to be in agreement. One is Hippolytus in the *Apostolic Tradition* (about A D. 215): 'And they shall baptize the little children first. And if they can answer for themselves, let them answer. But if they cannot, let their parents answer or someone from their family' (1968: 33). *The Testament of Our Lord*, which dates from the following century, has a similar reference: 'Let all the boys who can answer in baptism make the responses and answer after the priest. But if they cannot, let their parents make the responses for them, or some one of their households' (Cooper & Maclean 1902: 125). The first explicit mention of sponsors (*sponsores*) occurs in Tertullian (Evans 1964: 39) writing in the third century But the function of sponsors for adults was probably different from their task for children. Pagan adults needed guarantors of their good character and someone to help teach them, young children needed someone to make the responses for them, that is, to answer in their names.

Some of the clearest evidence that parents ordinarily did sponsor their own children comes from Augustine, and it is this evidence which brings us to the crux of the problem concerning parents sponsoring their own children. In a letter to a Bishop Boniface, written about A.D. 408, Augustine (1953: 134–5) says:

> I do not want you to make the mistake of thinking that the bond of guilt derived from Adam cannot be loosed in any other way than by the parents presenting their children to receive the grace of Christ, for you write in these words· 'As their parents were responsible for their punishment, so let the faith of their parents bring them likewise to be justified.' But you know that many are not presented by their parents, but by persons who are not even related to them, as young slaves are sometimes brought for baptism by their masters. And, not infrequently, when the parents are dead, children are brought to be baptized by those who have the opportunity of doing this work of mercy. Sometimes, even children who have been exposed by their parents are rescued and fed by others, often enough by the consecrated virgins, and then they are brought to baptism by them.

This letter is of great interest, for several reasons. First, from Augustine's examples it is clear that the ordinary practice was for parents to sponsor their own children; the cases he mentions when parents do not sponsor are exceptional: slaves, orphans and deserted children. Second, it is equally clear that a convincing argument could

be made that it is parents who should sponsor their children; having stained their progeny with the sin of Adam, they are the ones to loosen this very sin. Augustine does not provide a theological argument against this notion, he only points out that in some (exceptional) cases parents do not sponsor; therefore, the practice must be justified.

This is paradoxical. Current doctrine prohibiting parents from acting as godparents to their own children is based on the theological opposition of natural and spiritual paternity, a supposition which in turn is founded on the notion of baptism as a cleansing of original sin. It was Augustine, however, who most developed the idea of original sin, particularly in his controversy with the Pelegians; yet Augustine himself clearly did not think it was necessary for parents to be disqualified from sponsoring their own children. Quite the contrary! Thus, it must be concluded that the developing thesis of original sin did not itself directly lead to or account for the prohibition upon parents acting as godparents to their own children.

If church theology does not provide the answer for the prohibition, what explanation remains? The anthropologist turns to social causes, and here he has an established tradition to call upon from Mauss to Malinowski, and Tylor to Lévi-Strauss. The godparenthood complex, I would argue, has social value, in addition to its religious import, for by means of it parents can establish links with others from outside their immediate families. The negative prohibition upon parents sponsoring their children has its positive counterpart in that others must be found to perform the task. The prohibition introduces a social dynamic into baptism and leads to the establishment of relationships between the child and others, and the parents and others. I do not wish to introduce an unverifiable functionalism into the argument, but it may fairly be said that the godparenthood complex creates solidary relationships, like kinship, affinity, friendship, and blood covenant, which may be put to diverse social uses. At the same time the godparenthood complex is distinct from such other bonds in that it emanates from the spiritual realm of society. Similar in form to the nuclear family, the *compadrazgo* is complementary to it in that the one consists of spiritual bonds, the other natural ones, and the former cannot be superimposed on the latter. This neatly balanced and socially useful structure is created only when parents in fact are prohibited from becoming godparents to their own children.

I do not think this emphasis upon the social importance of the godparenthood complex contradicts ecclesiastical reasoning, but let me put the arguments into perspective. On the one hand, it is possible to stay close to the theology and to view the separation of natural and spiritual parenthood as a religious necessity deriving solely from the concept of the natural and spiritual person. On the other, it seems plausible to argue that a distinction between the two forms of parenthood is drawn only when parents share their child with others for essentially extra-religious reasons. The concept that a child must be reborn to the spiritual life only provides a plan for the division of paternal rights; it does not specify that they must be separated. Entrusting a child to others spiritually creates a distinction between natural and spiritual filiation as much as the notions of natural and spiritual paternity cause parents to share their child with others.

This interpretation, I further argue, allows for a fuller understanding of the logic

which lies behind the sequence of early church laws. Ecclesiastical laws about sponsors were not created *ex nihilo*; rather, ideas codified in the regulations originated in social custom. As baptismal sponsorship developed, parents began to entrust their children spiritually to others. This practice raised a series of problems which began to be reflected in the law until finally the church itself prohibited parents from sponsoring their progeny. Thus, the turn about in early church regulations is not to be accounted for by a change in church ideas but by social practices. The seeds of the later laws can be found in Augustinian notions, but it was social usage which led the church to elaborate the ideas and norms. As has been argued in the case of infant baptism, liturgical practice preceded dogma.

Let us look at the relevant laws. In earliest times, it is usually held, spiritual paternity was recognised between the minister of the sacrament and the baptised; however, a spiritual relationship did not obtain between the sponsor and baptised. Adult baptism in the very first centuries was probably at least as common as that of children, if not more so. The rite involved a naked immersion and for the sake of decency sponsors were of the same sex as the baptised adult. Thus, there was no need for a conjugal prohibition to be imposed between sponsor and baptised. In the case of children, as seen, at least through the age of Augustine (early fifth century) parents ordinarily sponsored their own offspring. The first official sign that parents did not always sponsor their own children came in 530 when the civil statute of Justinian forbid marriage between a sponsor and his godchild.[4] The rule was justified by the idea that a bond linked together the souls of the two, that is, a spiritual relationship existed between them. This was the first time that a spiritual bond between baptised and sponsor was recognised With respect to this law, however, one authoritative source terms the marital impediment 'insensible' and adds that 'good judges thought that it was not necessary to consecrate a custom' (*Dictionnaire de Droit Canonique* 1937: 161–2). The Justinian edict apparently was a response to social practices, but was the marriage prohibition truly 'insensible'? What was the logic of the Justinian rule? Two interpretations seem possible On the one hand, the marital impediment introduced between baptised and sponsor can be viewed as a direct consequence of the spiritual relationship; since a spiritual relationship was created between child and sponsor a marital prohibition had to be imposed. Spiritual affinity preceded the marital impediment. By contrast, I argue that this is to reverse the sense of the law and to implant modern ideas on an historical custom. The focus of the law is upon the marriage prohibition and not the spiritual relationship, which is only used to justify the impediment. The context of the law tends to support this argument, for the rule is given almost as an addendum to a series of statements about foster-daughter marriage. The edict reads: '. . . of course, by all means that person being prohibited from coming to marriage whom (whether or not she should be a foster-daughter) anyone has received from sacrosanct baptism, when nothing else can so induce paternal affection and just prohibition of marriage than a joining of this sort, through which by God's mediation their souls are united' (Coleman-Norton 1966: 1067–8). The marriage prohibition was 'prime' and the notion of a spiritual relationship was used to justify the impediment. But if this is a proper interpretation, what sense does it make? Although there was no explicit rule, parent and sponsor in the time of Justinian must frequently have been distinct persons. Parents were not sponsoring

their own children but were having others undertake the task. Therefore, a child could have married its sponsor, since the sponsor was not a parent. But if child and sponsor married this would lead to a collapsing of marriage on sponsorship. The marital prohibition, then, kept sponsor and spouse distinct and itself was legitimated by the notion of a spiritual bond. Hence, I argue that the folk custom of parents sharing their child with others preceded the strictly theological recognition of spiritual paternity between child and sponsor. Ecclesiastical ideas about rebirth and spiritual filiation did not in themselves force parents to choose others as sponsors; practice preceded doctrine.

Yet the Justinian law did not seem to deal with all the implications of parents not sponsoring their own children. Thus, in 692 when the Council of Trullo canonised the Justinian law, it added a marital prohibition between a widowed mother and a male sponsor. This was the first time that a connexion between the natural and spiritual parents was explicitly recognised. However, a question arising from this rule is did the concept of a spiritual relationship—spiritual co-paternity—precede or follow the marital prohibition between mother and sponsor? The actual text does not speak directly of spiritual co-paternity between sponsor and mother; rather, it states that since spiritual affinity (spiritual paternity?) is 'higher' or 'superior' to carnal bonds, such marriages are prohibited. Only by implication might it be said that a spiritual relationship linked mother and sponsor. Again, I would argue that the law prohibiting marriage is not a reflection of a pre-conceived idea of spiritual relationship between parent and sponsor; rather, it is a consequence of the fact that in folk custom parents were entrusting their children spiritually to others. If a sponsor could marry a widowed mother, sponsor and stepfather would become one, thus coalescing natural and spiritual paternity and nullifying the original set of relationships In 721 a Synod of Rome extended the prohibition of Trullo by forbidding marriage between a father and female sponsor.[5] Thus, both male and female sponsors were prohibited from marrying mother or father. In the 721 ruling the sponsor herself is referred to as the 'spiritual co-mother', that is a spiritual relationship—spiritual co-paternity—between parent and sponsor apparently was being recognised. Significantly, in light of my preceding argument concerning the decision of the Council of Trullo, one authority states that this was the first time a bond was recognised between parents and godparents (*Dictionnaire de Droit Canonique* 1937: 162). Still this decision, like the others, seems to have been a reaction to folk practice. Finally, the Council of Munich in 813 set forth what was to be the fundamental and enduring rule: parents were prohibited from sponsoring their own children. Thus, the practice was a part of social custom at least from the time of Justinian and probably earlier, but until the Council of Munich church rules themselves did not give full expression to the underlying tradition.

After Munich, of course, the complex underwent a number of elaborations up to the time of the Council of Trent (1545–63). Among other points the Council of Trent specified that parents contracted spiritual co-paternity (and therefore marital impediments) with the sponsor(s) and minister. To repeat, these refinements which dated back to earlier centuries were not necessary to establish the complex—except in cases of remarriage—given that parents were not permitted to sponsor their own children. In this respect it is instructive to look also at the present code which first became effective in 1918. One provision of canon 765 is that neither a father nor a

mother may sponsor their own child. Again, this law dating back to the Council of Munich is all that is needed to construct the system. Therefore, it is quite relevant that by canon 768 spiritual relationships are said to be established between the child, and minister and sponsor, but the parents themselves do not contract spiritual bonds with minister and sponsor, and have no marital impediment with them (canon 1079). Thus, the second rule of Trullo (prohibiting marriage between mother and male sponsor) and the prohibition of the Synod of Rome (forbidding marriage between father and female sponsor) are no longer recognised by the church, although they are still folk prohibitions.[6] Given church codification of the fundamental norm other reflections of it are not necessary. This interpretation is re-enforced by the current rules concerning confirmation. One provision of canon 795 is that neither father nor mother may confirm his own child. And by canon 796 a confirmation sponsor cannot be the same person as the baptismal sponsor, except in unusual circumstances. However, unlike the case with baptismal sponsors, a child may marry his confirmation godparent with whom he has a spiritual relationship.[7] This regulation, then, offers an interesting contrast to the Justinian civil statute and the first rule of the Council of Trullo. Both these laws, prohibiting a child from marrying his sponsor, implicitly recognised that parent and godparent were distinct persons. Long before 1918, however, this custom had become the rule and, therefore, the epiphenomenal rule prohibiting marriage was no longer necessary, at least in confirmation. A final interesting fact is that although the Latin Code no longer prohibits parents from marrying their children's sponsors, the Oriental Code still does.

In sum, I do not claim that this interpretation of church laws would be verified by the Papal Curia, but at least one way to look at some of the early norms is as expressions of the requirement to share one's child with others. At times the smoke-screen of ecclesiastical rules hid this fundamental social dynamic from view.

Let me also place the argument into a slightly broader framework. At least three types of interpretation can be provided for the changes in the early church laws. First, their specific timing and exact nature may be viewed as resulting from special historical conditions; contextual explanations of the laws do indeed provide some understanding of them. Additionally, the norms could be viewed as simple permutations of one another. For example, the structure set forth by Justinian is a variation of that enunciated by the Council of Trent. From this perspective the relative order of the changes is inconsequential. By contrast to this view, I am arguing that the temporal sequence of some of the early laws also is important. Each church regulation is not simply a variation of a preceding one. Behind the laws was a social dynamic which only became officially recognised by the church in the ninth century. An understanding of each of the early godparenthood structures depends upon knowing what immediately preceded and followed it. Up to the ninth century, at least, the sequence cannot be reversed without undoing the logic underlying it; the historical narrative cannot be sliced up in order to juxtapose at random the separate elements.

III

The second set of regulations specify whether choice may or may not be reciprocated. In church law godparents have never been prohibited from choosing

a parent of their godchild to be godparent to their own child;[8] yet, this is nearly a universal folk injunction. Summarising briefly, the following appears to be the situation:

1. In Átány, Hungary, choice reversal is prescribed. Individuals are reciprocally godparents and parents, and the relationship between them is symmetrical (Fél & Hofer 1969: 163).

2. In San Mateo, Mexico (Diebold 1966: 50), parts of Italy (Anderson 1956: 9, 41, 43, 103), Chimaltenango, Guatemala (Wagley 1949: 18), and Old Montenegro (Hammel 1968: 68) choice reversal is permitted and practised but not prescribed.

3 In a large number of societies choice reversal apparently is prohibited. (See, for example, Gudeman 1972, Hammel 1968. 73, and Ingham 1970.)

Form one provides for a reciprocal relationship between parent and godparent. In form three, the majority of cases, there is a non-reciprocal relationship; parents give away a portion of their child to godparents who in turn choose others to serve as godparents for their own children Finally, in at least four areas (form two) both reciprocal and non-reciprocal choice are practised Yet, even those groups which practise only non-reciprocal selection may not be entirely free of the reciprocal form In the non-reciprocal type it would be expected that the 'godfather-givers' would be distinguished terminologically from the 'godfather-takers'. Ordinarily, however, the reciprocal term 'co-parent' is used between parent and godparent. Both are co-parents, but might we view this as the presence in men's minds of the possibility, indeed the reassertion, of the reciprocal form in the linguistic domain?

Given these possible forms of choice why is the non-reciprocal type so predominantly practised? One possible argument is the following The godparenthood complex is found primarily in the context of bilateral kinship systems. In such systems continuing and discrete groups are never formed on the basis of kinship principles alone. Another element, deriving from locality, the polity or elsewhere must intervene for perpetual groups to be established. In such situations where corporate groups are not based on kinship the reciprocal form would have to be established between conjugal pairs alone, it would not be a continuing structural relation between groups over generations. Conversely, the prohibition upon choice reversal would force parents to engage in more spiritual relationships than the reciprocal form, and this might seem to fit better the fluid structure. Thus, in rural Panama where a bilateral kinship system is found the people are very clear that to reverse choice is to return the debt and nullify all the relationships.

The Balkan data, as reported by Hammel, provide some support for this overall argument. In general, the *kumstvo* (as the complex is known in Yugoslavia) is a relationship between agnatic corporate groups, but it is found in different forms. In southern Serbia the non-reciprocal form seems to be the practice. Reciprocal *kumstvo* is 'strange, almost illegitimate,' sponsor and parent are distinguished terminologically, 'the tendency is for continuity [and] stability' of the *kumstvo* relationships, there is a greater elaboration of respect, ritual, supernatural overtones and avoidance, and the relationships as part of a system are 'more stable' (1968: 73, 77, 81, 77). By contrast, in Old Montenegro choice reversal is 'common', the co-parent term is employed reciprocally, there is a tendency 'toward frequent

changes of relationship [and] complexity' of *kumstvo* ties, the relationships are more frank and practical, and the system shows 'less solidarity' (1968: 68, 77, 81, 77). In other words, reciprocal *kumstvo* is associated with non-continuity of the relationships over time and a less stable system, while non-reciprocal *kumstvo* is associated with perpetuity of relationships and greater solidarity in the total system of relationships. As in bilateral systems the non-reciprocal form forces parents to have more co-parents than the reciprocal form, and this creates greater social cohesion.

But even granted its existence, solidarity is difficult to measure, and the same effect of broadening the relationships can be achieved by other means: different sponsors for each ceremony could be required, distinct sponsors for each child might be a stipulation, the number of ceremonies necessitating sponsorship could be expanded, or the number of sponsors per ceremony could be augmented. It is difficult to formulate an argument about a causal relation between non-reciprocal choice and greater social cohesion.

Linked to the preceding is the problem of the asymmetry of the parent-godparent bond. In situations where choice reversal is prohibited and the complex is contracted between status unequals, the godparent almost invariably occupies the higher social position. Why should this be? Does the godparenthood complex entail an asymmetric prestation between parent and godparent? By contrast to the situation in asymmetric marriage alliance, is it possible to argue from structural principles alone whether the 'giver' or 'receiver' ranks higher? A close look at this situation, however, reveals a complexity. Who the 'giver' is and who the 'receiver' is varies by context depending on which rights and duties are emphasised in the set of relationships. For example, in Yugoslavia the godparent is the receiver; to ask someone to be sponsor is to give (Hammel 1968: 74). A similar notion obtains in Aritama, Colombia (Reichel-Dolmatoff & Reichel-Dolmatoff 1961: 173). By contrast, in Chan Kom, Mexico it is the godfather who confers the favour by sponsoring a couple's child (Redfield & Villa Rojas 1962: 93). As in affinal alliance definition of the prestation varies by society, yet to repeat, the godparent normally occupies the higher status whether he is defined as the giver or as the receiver. Thus, a contextual examination of the prestation does not provide an explanation for the asymmetry.

The answer to both these problems concerning folk usages may be found by turning to church doctrine. Conceptually, spiritual paternity is superior to natural filiation, an idea first explicitly enunciated by the Council of Trullo. A natural parent provides his child only with corporeal substance; a spiritual parent gives him the gift of grace.[9] Given this ideology two social implications follow. First, since the godparent is conceptually higher than the parent, the bond has an inherent or incipient asymmetry which may or may not be socially recognised. At the least, when the complex is contracted between persons in unequal social positions it would be expected that the godfather would occupy the higher. Second, since the spiritual is greater than the natural, the reversal of choice could result in a structurally difficult situation in that two persons would be both natural and spiritual co-parent to one another and each would have a higher and a lower position with respect to the other. The prohibition upon choice reversal preserves the distinction between the natural and spiritual parent, and avoids an ideologically confusing situation.

In terms of the social context, then, the three choice forms are found in the following situations. Form one, reciprocal choice, which has been reported only in Átány, Hungary, is contracted between persons who live in the same village. The relationship cements prior ties of close friendship and apparently occurs between status equals. Each sponsor serves as godparent to all the children of his co-parent, but the relationship itself is not necessarily perpetuated by the children. Form two, reciprocal and non-reciprocal choice, also is found in situations of status equality. Of the four reported instances of form two only in Old Montenegro does it appear that choice reversal is more frequently practised than non-reciprocal choice, but it is in Old Montenegro that the spiritual elements in the relationships are muted, as if, perhaps, to deny the ideological confusion. Only form three, non-reciprocal selection, is contracted between persons of different social positions, although it does not require that father and godfather be of different social strata. I do not wish to imply that the asymmetry of the relationship causes status inequality or instability in the society at large; I only point out the close connexion between the relationship and its social context.

IV

In sum, the godparenthood complex is a distinctively religious phenomenon in that it is based upon church beliefs and practices. At an abstract level it is simply a system of enduring spiritual relationships. At the same time the complex has been widely adapted, permutated, and put to a variety of social uses. The spiritual bonds are realised in a social system. These two facets of the complex, in fact, are closely related. Concerning the two patterns of godparent selection, I have argued that the first—the prohibition upon parents serving as godparents to their own child—is an explicit ecclesiastical norm but was first practised by the folk before being adopted by the church. Conversely, with respect to the second, the church has never prohibited choice reversal nor made pronouncements upon whether the godfather should come from a higher or lower social position than the parent, yet the ecclesiastical idea that the spiritual is higher than the natural seems to have had a profound impact upon folk practices: reciprocal selection is uncommon and when parent and godparent occupy unequal social statuses, the latter normally holds the higher position. In this case folk practices have been influenced by church notions. Thus, the constraints of religious doctrine and the impulse of social practices have both opposed each other and combined to make of the godparenthood complex the distinctive institution known today.

NOTES

This article benefited from a critical reading by Roxane Gudeman

[1] I am indebted to J. A. Pitt-Rivers for this reference.

[2] See Ravicz (1967) for a more extended list.

[3] 'In the practice of Infant Baptism we have a popular development of the earliest use of the sacrament, a development not at first deduced from theological principle or imposed from above by church authority, but arising spontaneously from below, out of the subconscious instincts of the Christian society, eventually forcing its way into official recognition, and generating, at least in Western Christendom, a not inconsiderable body of doctrinal ideas in order to effect its own *ex post facto* justification ... In treatises of systematic theology, the necessity of infant baptism is usually stated as an inference from the doctrine of Original Sin, and in the order of thought this may be so; but in order of time the practice was prior to, and

largely stimulated the growth of, the doctrine, and not *vice versa*. It is not the case that men said "Infants are infected from the womb with a hereditary taint; they must therefore be baptized as soon after birth as possible"; what they did say (after the custom originating, it would seem, in grounds of popular sentiment, had become thoroughly established) was: "The Church actually does baptise infants as soon as possible after birth and we cannot suppose that the Church does anything without good reason, therefore, infants must be infected from the womb with a hereditary taint." .. there is no clearer instance of the control exercised by liturgical or devotional practice over the growth of dogma than that provided by the study of the relations between the custom of Infant Baptism and the doctrine of Original Sin' (Williams 1927: 222–3).

4 Although the notion of a marital prohibition was employed early, the explicit category of an impediment was not elaborated until many centuries later.

5 The law may have been a reflection of the fact that women were now being employed as sponsors for children.

6 From the folk viewpoint the church law has the loophole of not accounting for remarriages, separations and divorces.

7 But it should be added that by canon 796 for the sponsor to be recognised lawfully, he should be of the same sex as the person confirmed, for justifying reasons, however, the minister may permit someone of the opposite sex to serve

8 Prior to the current code the marital impediment between two persons was considered to be multiplied in the case of reciprocal sponsorship (*Dictionnaire de Droit Canonique* 1942: 961).

9 I find Hammel's otherwise excellent presentation of the *kumstvo* deficient in that he omits the dogmatic foundations of the asymmetry. Comparing unilateral sponsorship to unilateral alliance he argues· 'the tendency toward unilaterality of the relationship should have, as a concomitant, some gradient of prestige recognition or other counterprestation Thus, for both structural and motivational reasons, we should expect the sponsor's group to be higher in material wealth and prestige than that of the godchild, and for behavior to show it' (1968: 78)

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ALTERNATIVE LOGIC IN 'PRIMITIVE THOUGHT'

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Primitive thought, especially magico-religious thought, is often said to contain contradictions and logical anomalies. In this article it is argued that *if* such thought is treated as incorporating a certain non-standard logic, the contradictions and anomalies are dissolved. The logic in question is the one devised by Lukasiewicz and subsequently used by Reichenbach to 'dissolve' the anomalies of quantum mechanics The main feature of such logic is its employment of a third truth value, indeterminate, in addition to those of truth and falsity It is argued that such an approach to primitive thought is preferable to various others, including those of Frazer, the early Lévy-Bruhl, and Beattie Reasons are given which suggest that primitive thought actually does incorporate a non-standard logic. Finally some philosophical remarks on the intelligibility of non-standard logics are made.

I

It is a prevalent and long-standing view among cultural anthropologists that the 'intellectualist' interpretation of magico-religious thought must necessarily, and wrongly, render such thought 'child-like', 'silly', and 'illogical'—by, for example, attributing to primitives absurd 'homeopathic' and 'contagious' causal notions. Robin Horton's paper 'African traditional thought and western science' (1970) has therefore been of special interest since he there tries to show that when magico-religious propositions are treated as analogous to certain kinds of propositions within theoretical science they, and their inter-connexions, are rendered eminently intelligible and respectable If I read him right, the 'intellectualist' mistake was in choosing the wrong features of Western science as models for primitive thought, and not in supposing there are such models to find.[1]

My article is written in the spirit of Horton's. Like him I want to regard various magico-religious propositions as analogous to certain ones in theoretical science, and like him I want to say that a number of difficulties in the interpretation of primitive thought are thereby resolved. In particular, my aim is to bring to bear the spirit of this approach upon the question of primitive *logic*. A rough distinction can be drawn between primitive beliefs and the logical relations held to obtain between these in primitive thought. Horton's emphasis was on 'understanding' the former; mine will be on 'understanding' the latter.

It is with logic in a narrower sense than is often encountered in ethnographical literature that I shall be concerned—a sense, though, which is normal among logicians. Some writers use 'logic' to refer to just about any aspect of native thought and beliefs. Lévi-Strauss, for instance, entitles his general discussion of native classification 'The logic of totemic classification' (1966: chapter 2). I find this obfuscating. One tribe may believe that elephant souls fly to heaven at death. Another may believe that, although elephants do not have souls, some illnesses are caused by the ill-will of elephants' souls. Both tribes have peculiar beliefs, but in

Man (N.S.) 10, 238–256.

the second case there is an extra and radically different peculiarity. It would seem reasonable to restrict the term 'logic' to what is at issue in this second peculiarity —the issue of inconsistency. Oddly enough, some anthropologists who have defended primitive thought against charges of illogicality or inconsistency have made their job harder by taking as putative examples of illogic what are not even putative cases of inconsistency in any logician's sense. Beattie, for instance, takes the Nuer's 'Twins are birds' as a putative example of a 'self-contradictory' belief (1966: 68). But, however odd such a belief might be, it is no more a possible candidate, as it stands, for being a contradiction than the proposition 'Chickens are birds'. Inconsistency, since it is a relational property, requires at least two propositions (although these may be structured into one compound proposition). The Nuer proposition would, no doubt, quickly yield a contradiction if linked to other propositions which *we* count as true—but unless we can ascribe these other beliefs to the Nuer themselves, we cannot accuse *them* of inconsistency. The point is not whether this can or cannot be done but that, unless it is done, no issue of logic or illogic is even raised.[2]

Before trying to characterise what would count as genuinely inconsistent or self-contradictory, let me cite two putative instances from primitive thought:

> a Nuer hold, apparently, that very young children do not have souls, which only appear with the second incisor teeth. Yet they also hold that the souls of all twins go 'above' at death. It appears to follow that some children—namely, very young twins—both do and do not have souls.[3]
>
> b . the Eskimos relate that the 'mother of the Caribou', the mythical creator of this species of animal, wore trousers of caribou hide When his attention is drawn to this point, the Eskimo recognizes that there is a contradiction in this myth! How could this clothing have existed before the caribou had come into the world? But this logical difficulty, though recognized, does not impede belief (Cazeneuve 1972 12)

In each case it is clear that the glaring peculiarity derives not from the content of the component beliefs, but from their form The offending premisses of (a), for example, could be displayed as:

i. If somebody is a young child, he has no soul.
ii. If somebody is a twin, he has a soul that goes 'above'.
iii. Some people are both young children and twins.

Now *any* set of premisses having the form of (i)–(iii) is going to yield a contradiction. In abstract shape, they are:

i'. If X is A, then it is not B.
ii'. If X is C, then it is B and D.
iii'. Some X's are both A and C,

from which it follows that some X's are both B and not B.

In calling (i')–(iii') 'abstract', I mean that all expressions other than certain logical operators ('if . then . . . ', 'and', 'not', etc.) are replaced by variables. This puts us in a position to characterise the propositions we want to call 'inconsistent' or 'self-contradictory'. They are those whose falsity results from nothing but the behaviour of the logical operators.[4] The inconsistencies of primitive thought, therefore, would be those whose falsity could be ascertained solely by observing the behaviour of those native expressions equivalent to our logical

operators. The peculiarity of 'Twins are birds' is due to its content—replace 'twins' by 'chickens' and it ceases to be peculiar. Contrast this with the position in (a). Replace 'soul' in (i) and (ii) by 'toy' and the peculiarity remains—logical peculiarity.

Faced by such logical anomalies, as I shall call them, as (a) and (b) anthropologists have reacted in a variety of ways, and my suggestion for dissolving these anomalies will be better understood by way of contrast with some of these other approaches. Here I merely sketch some of these approaches, later I offer some criticisms.

A. Some take the inconsistencies at face-value and conclude *either* (1) that primitives are 'child-like' and illogical, incapable of appreciating their inconsistency, *or* (2) that primitives are *contra*-logical; that while they see the inconsistencies, they prefer to ignore them in favour of some non-logical principle to which they attach more certainty than to the demands of logic The difference between (A1) and (A2) might be compared with a young child's unawareness of his inconsistencies as contrasted with a professional 'irrationalist' philosopher's self-conscious rejection of the sovereignty of logic in a spirit of 'Credo quia absurdum'.

B. Some insist that the native propositions have only the appearance of being inconsistent—an appearance which further attention to the native context, or to criteria of translation, will remove This view is sometimes expressed in the form of a simple principle of *charity* towards alien thought, and sometimes in the form of a sophisticated principle to the effect that a necessary condition for recognising a translation as adequate is that it should 'preserve logic'

C. Some argue that while the native sentences may have been translated correctly, their purpose and function is not such as to allow sensible discussion of the logical relations between them They are not, in fact, propositions at all—sentences to which truth-values can be seriously assigned. Rather they are essentially 'symbolic' or 'expressive' in function. Raising the issue of inconsistency between the relevant sentences is rather like searching through the works of Ezra Pound for contradictions between different lines. To raise such an issue, or to go on such a search, is already to have misunderstood the functions of the sentences.

Each of the above approaches can easily be associated with certain writers—though no thinker would embrace any of these views in the crude forms I have sketched them. (A1) corresponds to the 'intellectualist' approach of Frazer, and of Tylor with his reference to the 'savage's .. child-like philosophy'. (A2) is the approach, broadly speaking, of the early Lévy-Bruhl, according to whom primitive thought 'does not bind itself down . . to avoiding contradiction. It obeys the law of participation first and foremost' (1926. 78).[5] (B), in its 'charitable' form, is characteristic of much of Evans-Pritchard's handling of the apparent contradictions of Nuer and Azande thought by careful attention to the context and background of the beliefs. In its form as a principle of translation, (B) has been urged by a number of philosophers—notably W. V. Quine. Martin Hollis exemplifies this approach when he writes:

> the only way to find native terms for relations among utterances is to translate the utterances and then to interpret the linking terms ['operators' in my terminology D.E.C] so that the utterances are linked coherently (1970: 216).

(C), in some form or other, weak or strong, can be found in Firth, Beattie, and

Leach among others. Leach, for example, has argued that the statements of myth have the same function as rituals.

> To ask questions about the content of beliefs which are not contained in the content of ritual is nonsense .. I shall make no attempts to find any logical coherence in the myths to which I refer. Myths for me are simply one way of describing certain types of human behaviour (1954 13–14).

I shall argue for:

D Primitive magico-religious thought incorporates an alternative logic to our 'standard' one within the terms of which the apparent inconsistencies are not inconsistencies at all This alternative logic, moreover, is one that certain Western logicians have constructed in connexion with some areas of theoretical science.

Hence I shall be arguing as against (A) that primitives are actually employing a logic, and are not therefore simply illogical or contra-logical in the relevant areas; as against (B) that there is no strong reason to think the relevant propositions have been mistranslated or misinterpreted; and as against (C) that the problematical sentences are genuine propositions between which logical relations, albeit of a non-standard type, obtain and can be sensibly examined.

In slightly more detail, my programme is to show or suggest that primitive thought incorporates a non-standard, many-valued logic of the type Reichenbach has used for 'dissolving' the anomalies of quantum physics; that the natives' employment of this logic has a similar rationale to Reichenbach's own; and that primitive thought, once so considered, ceases to be as anomalous and problematical as is usually supposed. In the next section I characterise the alternative logic in question; in section III I argue that *if* primitive thought incorporates this logic, its anomalies are dissolved, in section IV I suggest criticisms of other approaches ((A)–(C)), hence giving reasons for hoping my interpretation can be made to stick; in section V I argue that there is no convincing evidence against my interpretation. Some residual points are made in section VI.

II

An alternative or deviant logic can only be understood by way of contrast with what it is alternative to, what it deviates from So let us begin by glancing at so-called 'standard', 'classical', or 'orthodox' logic (or rather, at the relevant segment of it).[6] For our purposes standard logic (SL) can be thought of as characterised by:

1. A certain principle about truth-values—namely, the principle of bivalence, according to which every proposition with a determinate meaning is either true or false, and not both;

2. Certain truth-functional definitions of the logical operators in accordance with (1). For example, the definition of 'either . . . or . . ' as that operator which yields a true compound proposition except when each component proposition is false (i.e. 'Either *p* or *q*' is true except when '*p*' is false and '*q*' is false). Or the definition of 'not' as that which always converts true propositions into false ones and vice-versa (i.e. '*p*' is true if and only if 'not *p*' is false, and vice-versa).[7]

3. A list of logical truths (tautologies) and logical falsehoods (inconsistencies) ensuing from (1) and (2). Among the tautologies will be:

Either p or not p (Excluded Middle).
If p, then p (Identity).
Not (p and not p) (Non-Contradiction).[8]

In calling these 'tautologies' we mean that the propositions are always true, whatever the values of 'p', 'q', etc., simply in virtue of the definitions of 'either . . . or . . .', 'and', etc . . . (see p. 243) Among the inconsistencies of SL (propositions which are false whatever the values of 'p', 'q', etc) will be:

If p, then not p
p and not p
If (p or q) and not p, then not q[9]

A logic may deviate from SL in respect of (1), (2), or (3), or all of these. If (1) is rejected the deviation is going to be considerable, for once we reject bivalence our definition of the operators is bound to alter, and this will almost certainly affect the list of tautologies and inconsistencies. What I wish to sketch briefly is the three-valued logic (L3) originally devised by the Polish logician Lukasiewicz, and subsequently elaborated and employed by Hans Reichenbach in connexion with quantum mechanics.[10]

In L3 there are three truth-values—true, false, and indeterminate. Hence it is no longer the case that every proposition is true or false. This forces new definitions of the operators—for we shall want to know what happens to 'Either p or q', say, where one or more of the components are indeterminate in value The following fragments of the new definitions will be worth bearing in mind:

a. A disjunction, 'Either p or q', is indeterminate except when one of 'p' and 'q' is true (in which case it is true), or when both 'p' and 'q' are false (in which case it is false).

b. The negation of an indeterminate proposition is itself indeterminate (i.e. 'p' is indeterminate if and only if 'not p' is).

c. A conjunction, 'p and q', is indeterminate *inter alia* when 'p' and 'q' are each indeterminate.

d. A conditional, 'If p then q', is true except when 'p' is true and 'q' false (in which case it is false); 'p' is indeterminate and 'q' false, 'p' is true and 'q' indeterminate (in both of which cases it is indeterminate).[11]

Rejection of bivalence, and the new definitions, result in a shorter list of tautologies and inconsistencies than in SL. For there will be propositions which are always true (false) in SL which are sometimes indeterminate in L3 Consider, for instance, 'p or not p'. By (b) above, if 'p' is indeterminate, so is 'not p', and by (a) above, the disjunction of two indeterminate propositions is itself indeterminate. Hence 'p or not p' is indeterminate where 'p' is. So the law of the Excluded Middle as represented above does not hold in L3 It can also be worked out that 'p and not p' will not always be false in L3. In other words, not all contradictions are false in L3 (though they are never true).

It is one thing to *call* propositions 'indeterminate'—and to construct an artificial system on that basis—and quite another to *interpret* this third truth-value and the resulting system. A wide variety of interpretations have been proposed, but I shall be concerned with just one—Reichenbach's. Quantum theory, he says, allows for

a number of meaningful propositions which, nevertheless, cannot be verified or falsified. This results from the quantum principles of Complementarity and Indeterminacy. Two propositions are complementary to one another if testing for the truth of the one makes it impossible in principle to test for the truth of the other. For example, a proposition describing a particle's momentum at a certain point in time will be complementary to one describing its position. In terms of Heisenberg's famous principle, if momentum is determined position cannot be—and vice-versa.[12]

Reichenbach proposes that at least one proposition in any complementary pair or group be counted as indeterminate in truth-value It is crucial to realise that 'indeterminate' does not mean 'unknown'. It is not that we do not as a matter of fact know of an indeterminate proposition whether it is true rather than false; the point is that nothing could possibly count as showing this. Where verification and falsification are in principle ruled out, the propositions concerned should be assigned a value distinct from true and false. Not only can we not assert ' "*p*" is true' and ' "*p*" is false', we should not assert ' "*p*" is true-or-false'. Where 'even Laplace's superman could not know what would have happened if we had measured p' (Reichenbach 1944: 146), we should not speak of the proposition describing a measurement as subject to bivalence.

Among the consequences of this proposal will be·

i. A disjunction of the form 'Either the particle's momentum is *m* or it is not *m*', which would count as necessarily true in SL, may be indeterminate in L3—namely in the case where each disjunct is in principle untestable (due to other tests having been made).

ii A conjunction of the form 'The momentum of the particle is both *m* and not *m*', which would count as necessarily false in SL, may be indeterminate in L3—for the same reason given above.

Reichenbach stresses that the whole value of applying L3 to quantum mechanics resides in its power to 'dissolve' the various 'causal anomalies' which threaten the mechanics. It would take far too long to detail his treatment of any of the anomalies—like those concerning the 'two slits' experiment, or the defiance of 'potential barriers'. Instead I shall mention Reichenbach's characterisation of the form of the anomalies and his manner of dissolving them. In each instance we are faced with something like the following:

a. One or more true conditionals of the form 'If A then B'.[13]
b. The knowledge—on observational grounds, say—that 'B' is false.
c. The implausibility of treating 'A' as false.

If we were employing a two-valued logic, like SL, contradiction would ensue. For 'A' would have to be counted as true, in which case we could derive 'B' (for 'B' follows from 'A' and 'If A then B'). We are then faced with the fact that 'B' is both true and false. Suppose, though, that 'A' is a proposition which is in principle neither verifiable nor falsifiable. In that event, although we cannot hold it to be false nor, by Reichenbach's principles, can we hold it to be true. It must be assigned the value indeterminate. But then the anomaly disappears. For it is only if 'A' is counted as true that it, in conjunction with 'If A then B', allows us to derive 'B' as a conclusion and the ensuing contradiction. To avoid contradiction

we do not have to treat 'A' as false, which we know to be implausible. But this is 'without danger', since by treating it as indeterminate it 'cannot be used for the derivation of undesired consequences' (Reichenbach 1944: 162).

Faced by the anomalies of quantum mechanics, something has to give. Either the principles of quantum physics must be rejected, or they must be amended by introducing *ad hoc* 'hidden variables'; or the principles of probability must be given up; or such apparently fundamental principles as 'No action at a distance' must be sacrificed—*or* standard, two-valued logic must be revised in this context. Reichenbach's originality[14] consists in regarding logic as one of the candidates for revision, as itself an instrument in science, and not an inviolable measure against which all other scientific instruments must be cut. The possibility then emerges of preferring the claims of some scientific theory against those of standard logic should conflict between the two arise.[15]

III

If the anomalies of magico-religion are to be dissolved by using an alternative logic like L3, the undertaking must be in two stages. First it must be shown that *if* primitive thought incorporates L3 the anomalies disappear, and second that primitive thought actually does incorporate L3. If only the first stage is completed we shall have shown, merely, that the problematical propositions are capable of being given an interpretation whereby they cease to be inconsistent. Unless we can show that the natives provide, in some way, this interpretation, we shall not have saved *them* from the charge of inconsistency. In this section, though, it is only the first stage that is concerned. If the magico-religious anomalies are to be dissolved in the manner of the quantum ones, the general analogy will have to be something like this:

> The magico-religious thought of a people is a highly theoretic explanatory system, within which propositions occur that, while meaningful in terms of the system, are not capable of any verification or falsification within it. Such propositions are not counted by the people in question as being either true or false, but as having a third truth-value. The anomalies arise because the people explicitly reject the consequences of propositions they appear to accept. However, in every such case, we find that at least one of the propositions is counted by the natives, in virtue of its untestability, to be neither true nor false—hence, despite appearances, the people do not regard as *true* a number of inconsistent propositions.

To dissolve the anomalies in the manner of Reichenbach's dissolution of the quantum ones we would have to show, in other words, (i) the magico-religious anomalies have the form of the quantum ones, (ii) various propositions in magico-religion are treated as indeterminate just as various quantum ones are, and (iii) magico-religious reasoning is that of L3.

We can spell out the terms of the required analogy in connexion with a couple of examples. The Azande, according to Evans-Pritchard, say that witchcraft is inherited through the male line, and also that a person was only a witch if a certain post-mortem intestinal test proves positive (which it does not always do). This results in anomaly when taken together with the fact, which the Azande admit, that everyone in the clan is biologically related to everyone else. For then if anyone is found to have been a witch post-mortem, everyone must be a witch. This con-

clusion, though, conflicts with the fact that the post-mortem test is not always positive—and indeed with the point of conducting it at all.

> Azande see the sense of this argument but they do not accept its conclusions, and it would involve the whole notion of witchcraft in contradiction were they to do so (Evans-Pritchard 1937: 24).

If the analogy I suggested can be applied, we can see why they do not accept the conclusions. The apparent anomaly could be set out as follows:

1. Every biological relative of a witch is a witch
2. If (1) then all Zande are witches (since they are all related)
3. Not all Zande are witches (by the post-mortem test).

Here we have just the form of the quantum anomalies mentioned on p. 245:

A
If A then B
Not B.

Now if (1) is taken as true, together with (2) and (3), contradiction is inevitable—since (1) and (2) entail the contradictory of (3). Suppose, though, that while (1) is not rejected as false, nor is it regarded as true. Rather it is regarded by the Azande as an *in principle* untestable proposition which is consequently assigned the truth-value indeterminate. If we then assume that Azande employ the reasoning of L3 we shall not be able to ascribe to them the belief that all Zande are witches; nor, therefore, shall we able to foist a contradiction upon them. For, to repeat, it is only if the proposition corresponding to 'A' is counted as *true* that 'B', and therefore the contradiction, can be derived.

Consider, second, the example of the Nuer twins mentioned on p. 241. The apparently anomalous set of Nuer beliefs could be set out as.

I. Either twins' souls go 'above' or they go elsewhere
II. If twins' souls go 'above', twins have souls
III. If twins' souls go elsewhere, twins have souls
IV. Some twins—namely, very young ones—do not have souls

This has the form (after a bit of modelling):

Either A or B
If A then C
If B then C
Not C.

If the disjunction (I) is, along with the rest, counted as true, contradiction is inevitable—for it follows from (I)–(III) that twins have souls, which is what is denied (for some cases) by (IV) Suppose, though, that each disjunct in (I) is regarded as indeterminate, because untestable. In that case, by the L3 definition of 'either . . . or . . .', (I) itself will be regarded as indeterminate But now it will be impossible to derive the troublesome conclusion that all twins have souls. This anomaly is of special interest since it has almost the form of Reichenbach's presenta-

tion of the 'Two slits' quantum anomaly. There the troublesome propositions are, roughly,

Either the particle passed through slit A or through slit B.
If it passed through slit A the probability of it hitting X is d.
If it passed through slit B the probability of it hitting X is d
The observed probability of a particle's hitting X when only slits A and B are open is not d.

Reichenbach's dissolution consists in treating the original disjunction as indeterminate on the grounds that each disjunct is.

If we find natives not dissenting from propositions of the explicit contradictory form '*p* and not *p*' our interpretation—unless we are to accuse them of inconsistency—must be that they regard '*p*', and so 'not p', and so '*p* and not *p*', as indeterminate. Only then will they not be assenting to the absurd claim that both a proposition and its negation are true. The analogy would then be with a quantum conjunction like 'The particle passed through slit A and did not pass through slit A' which, under certain conditions, will be counted as indeterminate rather than false on the Reichenbach approach. Contradictions, it might be argued, only demand rejection as *false* when the contradictory propositions describe states of affairs that might conceivably be found to obtain. For then, unless one of the propositions is regarded as false, we shall be in the unfortunate position of imagining an impossible world in which there might be discovered incompatible states of affairs. But if the limbs of the contradiction describe nothing that could in principle be discovered to obtain (or not obtain), then the contradiction is, so to speak, harmless. Instead of dismissing it as false, we can treat it as indeterminate. Or one might put it like this: if the notions of truth and falsity cannot be sensibly applied to single propositions in virtue of their in principle untestability, nor can such notions be sensibly applied to various compound propositions in which the single ones might figure.

It is important to realise that when I have been talking of the possible untestability of various Azande or Nuer propositions I have meant untestability by Azande or Nuer criteria. That *we* might regard a proposition about souls as, at best, untestable would do nothing to save the natives from charges of inconsistency It is required that *they* regard it as untestable and indeterminate.

Perhaps I should stress, at this point, that even if the suggested analogy between magico-religious thought and Reichenbachian quantum thought can be made out, I hardly regard the former as therefore unproblematical, for, of course, the Reichenbachian interpretation is itself problematical. Naturally, one must sympathise with the intuitive reaction against supposing that propositions like 'The particle passed through slit A' or 'Witchcraft is inherited' could be counted as anything but true or false (or, perhaps, meaningless). The problem, though, will have shifted from being one to do with native inconsistency within a readily intelligible standard logic to one concerning the philosophical interpretation of a different logic. I return, briefly, to this question in the final section.

IV

I now want to look critically, though briefly, at the other approaches to primitive 'inconsistency' which were sketched in section (I). I do so for a number of reasons: partly so that my own approach is clarified by way of contrast to these others;

partly to show that it would be *nice* if my approach can be made out, since these others are all more or less defective; and partly because the plausibility of some of these approaches derives, to some extent, from ignorance of alternative logics such as L3.

The first, scarcely original, charge to level against the 'intellectualist' account (A1) is the sheer implausibility of supposing the primitive anomalies reflect a 'child-like' mentality. What is implausible is not the postulate of 'child-like' mentalities as such, but this together with the admission that *outside* the magico-religious arena primitives typically think in logical, sensible ways. That whole peoples, capable of consistent thought in general, should suddenly become 'child-like' when thinking within a particular field, would imply a schizoid intelligence that we should resist having to admit.

More interesting from our point of view is that 'intellectualism' may have derived from the wrong sort of analogy between scientific and magico-religious explanations. 'Spirits', said Tylor, 'are personified causes', primitive surrogates for the natural causes of phenomena that we, but not they, have identified. Where the biologist talks of viruses as causes of illness, or the physicist of molecular activity as the cause of combustion, the natives talk of spirits and their activities as the causes. But it is more revealing, as Horton argues, to construe the spirits and entities of magico-religion as analogous to the *highly* theoretical entities of, say, sub-atomic physics, or to the highly theoretical agencies of psychoanalysis The purpose of such postulates is not so much to provide particular causal explanations of particular phenomena but to provide an explanatory framework in which various particular causal explanations can be placed For example, the virus-illness connexion will, we assume, be eventually subsumable under the laws of sub-atomic physics. The relevance of this for us is as follows: the higher the level of the scientific or 'quasi-scientific' discourse, the less closely tied are its propositions to verification and falsification by observations and tests—hence the greater is the scope for treating some of its propositions as indeterminate in value By making magico-religious propositions akin to the relatively low-level ones of molecular physics or viral biology, the 'intellectualist' debars himself from introducing the notion of indeterminate truth-values. By drawing the wrong analogy Tylor and Frazer made it inevitable that the native anomalies would have to be allowed to stand, since the analogy prevents dissolution of the anomalies along the lines I have suggested. (Not, of course, that nineteenth-century writers could have been in a position to have drawn the *right* scientific analogies.)

The most eminent proponent of (A2), the view that magico-religious thought is *contra*-logical, is Lévy-Bruhl. His primitive is not unaware of inconsistencies, but he takes no pains to avoid them since he 'obeys the law of participation first and foremost'. I shall not criticise the notion of participation, for I think it is simply not relevant to explaining the primitive's apparent indulgence in the inconsistencies that interest us. Lévy-Bruhl is one of those anthropologists who, I mentioned, label as 'contradictions' what are not even putative cases of contradictions in the logician's sense. Indeed, it is hard to find a single example of a logical contradiction cited in *How natives think*. (It is worth stressing that in his later *Carnets*, Lévy-Bruhl considerably revises his earlier accounts of the contradictions in primitive thought—preferring, for instance, to treat propositions like 'Twins are birds' not as self-

contradictory but as statements of 'physical impossibilities'. I have concentrated on the earlier writings for it is in these that one finds material most relevant to the concerns of this article, as well as a particular view (namely (A2)) on how to deal with alleged contradictions in primitive thought). Now whatever else the law of participation might explain, I cannot see how it could begin to explain indulgence in genuine logical contradictions. It is one thing to make sense of 'Twins are birds' by reference to presumed mystical communion, and quite another to try to appeal to this in explaining how anyone could believe that no young children have souls but that some of them do. Participation might explain how primitives accept some very peculiar propositions, but not how they accept inconsistent combinations of these.

Even if mystical participation could help explain indulgence in inconsistency this would only be, I think, in an indirect manner. It would not be mystical thought as such which explained tolerance of contradictions, but rather the indeterminacy lent to propositions by such thought. At one point, in fact, Lévy-Bruhl appears to move towards this view. Having stressed the lack of exactness that even ordinary concepts tend to have within mystical systems of thought, he writes:

> When the essential features of stone are, as it were, registered and fixed in the concept 'stone' . . . it becomes inconceivable that stones should speak or procreate human beings, etc. The more the concepts are determined, fixed and arranged in classes, the more contradictory do the statements which take no account of such relations appear Thus the logical demand made by the intellect grows with the definition and determination of concepts . (1926 379)

Here the idea seems very much to be that contradictions can be tolerated when, through the vagueness of a concept, propositions containing it cannot be determined as true rather than false [16]

A final point: Lévy-Bruhl dismisses the kind of approach advocated in this article when he writes that if primitive thought 'were regulated by a logic different from our own', it would be 'impenetrable', 'incomprehensible' (1926 71). For, according to him, 'the idea of a logic different from our own could only provide us with a negative concept, devoid of meaning' (1926: 71). While it is not clear what Lévy-Bruhl means by 'our' logic, it seems that his dismissal of an approach to primitive thought *via* alternative logics is due to a failure to envisage the possibility of a logic like L3 which is not, I would suggest, 'devoid of meaning' It could be that many approaches to the anomalies of magico-religion have been partially motivated by a (perfectly excusable) blindness to the possibility of viable alternatives to standard logic in terms of which the anomalies might be dissolved [17]

This last point is pertinent in discussing (B). If the principle of charity is put in the form: If you can see no way of dissolving the anomalies of primitive thought when these are taken at face value, do not take them at face value: then there will be no reason *not* to take them at face value if we find a viable alternative logic in terms of which they can be dissolved. For if we find such a logic, there will not be the same motivation there would otherwise be for insisting that we have misinterpreted or mistranslated.

But apart from this, the principle of charity can surely be a dangerous one. Naturally it would be absurd to object to maximum attention to context and criteria of translation, but it would be wholly wrong to insist that we have mis-

interpreted propositions *on the sole ground* that, so interpreted, they appear inconsistent. This would be prodigality, not charity. Such an attitude, for one thing, should lead to total scepticism about any interpretations or translations. For if it is insisted that the interpretations, despite detailed care over context and translation, are wrong, why should we suppose we ever interpret correctly? We should be in the position of those avant-garde Christians who, by reinterpreting those sections of the Bible that do not match contemporary moral and metaphysical fashions, fail to see that they thereby cast doubt on all biblical interpretations—including those that must be accepted by anyone counting himself a Christian No doubt special attention must be paid to apparently anomalous propositions—but, unless we are to jettison all confidence in any interpretation, there has to come a point where they are accepted as being anomalous. Second—to borrow from Ernest Gellner—if the principle of charity is ever used, then it should always be used; but the consequences of this are paradoxical. If, for instance, we invoke the principle in order to criticise Frazer's view that magical thought is radically wrong-headed, we should also invoke it to criticise those who criticise *Frazer* for being radically wrong-headed. Wielded indiscriminately the principle will have the result that nobody—including the opponents of those who wield it—can be accused of having obviously mistaken views; for in each such case the principle would demand that we must have misinterpreted what the views are. 'The trouble', says Gellner, 'with the principle is, of course, that it is *too* charitable. it absolves too many people of the charge of systematically illogical or false or self-deceptive thought' (1970: 36).

I have no time here to deal with the translational version of approach (B), according to which any translation which does not render the natives logical is for that reason alone deficient. The idea, I think, is that we could never have evidence that the natives employed a different logic, since either we understand them, in which case they share our logic, or we do not understand them, in which case there is no reason to think they are genuinely thinking or using language at all If 'logic' here is being used to cover not only standard logic but also clearly and closely related systems like L3, I think the idea is correct. What is *in*correct is to think that there could be no reason for supposing natives employ a logic different from standard logic as I characterised it The human choice is not between standard logic and no logic alone. (Some of my remarks in section (V) bear upon this.)

There is superficial similarity between my approach and the 'symbolist's' (C). Like myself, they want to say that at least some of the sentences involved in the anomalies are neither true nor false. But, in the first place, the 'symbolist' typically claims not that the sentences *cannot* be spoken of as true or false, but that it would be churlish and irrelevant to do so—just as it would be in the case of some expressive line in a poem like 'Death is now the phoenix' nest'. My point, though, was that it would actually be mistaken to talk of the relevant sentences as true or false, since they are *indeterminate*, within the native thought system. Second the 'symbolists' give quite different reasons from mine for not assessing the native sentences as true or false. Theirs is basically that the sentences are 'essentially symbolic' or 'expressive'; mine is that the sentences are genuine propositions, but *in principle* untestable ones.

The 'symbolist' approach, I suggest, faces a dilemma. The claim that magico-religious language and behaviour is symbolic or expressive can be taken in a strong

and a weak way. In the weak sense, it may well be true, but can do nothing to dissolve the anomalies. The strong claim would do this; unfortunately it is implausible. The weak claim is to be found in Beattie's 'On understanding ritual', where it is said that magical procedures and language:

> involve . . . 'a symbolic element, in which something is standing for something else'. This aspect, the characteristic feature of magical behaviour, calls for its own kind of investigation. The matter is as simple as that (1970: 251–2).

The claim, then, is that magico-religion involves symbolic, expressive aspects along with others (e.g. 'technological' ones) For all I know this is true, but such a minimal thesis can do nothing to dissolve the anomalies. That a sentence has an expressive role is in no way incompatible with its also having a propositional, fact-stating role—and if it has the latter, it is capable of entering into logical relations, like inconsistency, with other sentences, which we are surely entitled to comment upon. To dissolve the anomalies we require the strong thesis that the sentences are *merely* symbolic and expressive Only then would they cease to be propositions, assessable in terms of truth-values and capable of entering into logical relations.[18] This strong thesis, however, is simply implausible. Magico-religious sentences play roles which are incompatible with their being merely symbolic. Most obviously they play a role in explaining and lending coherence to causal relations and natural phenomena. Kalabari water-spirits may well symbolise and express the importance of fishing to the natives, but they also serve to explain changes in agricultural fortunes and as one element in a 'triangle of forces' belonging to 'a theoretical scheme in terms of which Kalabari can grasp and comprehend most of the vicissitudes of their daily lives' (Horton 1970: 150). More interesting than merely pointing this out is to suggest why anyone should be tempted towards the strong thesis (and the weak one, too, perhaps). The temptation may arise from an overcrude view of what counts as a scientific proposition. Throughout *Other cultures*, Beattie seems to assume that unless a sentence is testable and verifiable it is not scientific but 'symbolic', 'metaphorical', or 'analogical'.

> A Nuer's assertion about twins is not to be regarded in the same way as a scientific proposition, testable experimentally in the same way as, for example, the statement that water freezes at 32 degrees Fahrenheit is testable. It is rather the statement of an analogical, or, as we might say, a poetical identity (1966. 68)

But the dichotomy 'scientific and testable' or 'symbolic and poetical' is an unreal one—for there are any number of propositions within science that are not testable in anything like the way the one about freezing-point is. The proposition that a particle has passed through a certain slit is, we saw, untestable in certain situations, but nothing less symbolic and poetical can be imagined. It may be that a theory, to count as a scientific one, must be broadly testable against experience, but in no way does it follow from this that each proposition allowed by the theory is testable. On the contrary, it may be part of the theory that some of its propositions are not testable. The point is that we need not jump to the conclusion that magico-religious sentences must be purely expressive or symbolic because they are untestable. An alternative—the one I have canvassed—is to construe them as analogous to those scientific propositions in, say, quantum theory, which possess the indeterminate truth-value.

Each of the other approaches to the anomalies of magico-religious thought, then, is more-or-less defective. Not only that, but some of the arguments that might be urged in favour of one or another cease to be persuasive once the possibility of viable non-standard logics is envisaged; for these approaches are based on the presupposition that no alternative logics are constructible in terms of which primitive thought might be interpreted.

V

But does magico-religious thought *actually* incorporate a logic like L3 in terms of which the anomalies cease to be such? The central question here is whether primitives do in fact regard certain propositions as having the indeterminate truth-value. I shall simply assume that if they do the logic employed in connexion with the propositions is that of L3. (This is to assume, not implausibly, that the Lukasiewicz definitions of the logical operators are the most natural ones for a three-valued logic.) Clearly our approach has to be pretty indirect, for we do not expect to meet native logicians who will *tell* us, in a systematic way like Reichenbach's, that they are using a non-standard logic

Let me begin with two features of primitive thought that at least *suggest* an affirmative answer to our question. First, a number of ethnographers, including Lévy-Bruhl, have stressed that primitive reasoning *outside* the magico-religious arena is of our standard type.

> . the primitive . . . will usually feel, argue and act as we should expect them to do. The inferences he draws will be just those which would seem reasonable to us in like circumstances (1926 78–9).

It would seem no accident that standard logic should be abandoned in just the arena where—due to the typically vague and transcendental nature of the phenomena—testability and verification is least available. A *reasonable* hypothesis is that standard logic is abandoned because a third truth-value is assigned to these untestable propositions Second, it is often pointed out that the typical native attitude towards alleged contradictions in their thought is one of *indifference*, or *lack of interest.*[19] The primitive does not, like some dialecticians or Zen-masters, embrace contradictions as true; rather he feels no obligation to reject what leads to contradiction as *false*. This would be precisely the attitude we would expect of someone who treated the rogue propositions as indeterminate, which would then become analogous to those which, according to Reichenbach, we can ignore 'without danger' It would be going too far to say that these features of primitive thought provide evidence for my interpretation. But we can say they nicely fit with it—and one may wonder whether any interpretation at this level of abstraction can hope for much more than that.

One of the reasons why I referred to my interpretation as highly speculative must now emerge. I am going to assume—without the argument I am anyway professionally unequipped to provide—that various propositions within magico-religious thought are treated by the natives as untestable. This does not seem to me an unlikely assumption to make. There are, after all, any number of propositions within our own religions which few believers would want to regard as testable

(the doctrine of transubstantiation, perhaps). Nor is it an assumption I have found contradicted in the relevant literature known to me. Thus while, for example, Evans-Pritchard details some of the ways in which the Azande test for the activities of ghosts by consultation of *benge*, he does not, as far as I am aware, mention how if at all they verify that witchcraft is hereditary, or how the Nuer establish what happens to twins' souls. (Incidentally, for my interpretation to go through, all that is required is that at least *one* proposition involved in each anomaly be counted as untestable.) So while my assumption is defeasible, it is not, I think, defeated.

Even if this assumption is granted, my interpretation of the anomalies is far from being guaranteed. For I should have to deal with the following rejoinder:

> It may well be that the natives do not regard some of their propositions as testable—still, they do *believe* them. And this is sufficient for the charge of inconsistency to be made against them It is their believing that twins' souls go above, whether or not they regard this as testable, which makes them inconsistent, since they also believe that young children have no souls.

Well, we certainly know that the natives do not reject the relevant propositions as false, for if they did there would be no anomalies to discuss. But the trouble with the above rejoinder concerns how it is that we identify beliefs, especially in the obscure region of magico-religion. Is it that the natives *say* they believe the propositions? Two problems at least arise here The first is one of translation. Here I can do no better than cite Rodney Needham's detailed discussion of the difficulties in identifying native expressions translatable as our 'believe'. Having produced an anthology of such difficulties, and of the consequent translational conflicts between ethnographers, he writes:

> All of this contrasts extremely with the plain statements made by ethnographers to the literal effect that the peoples whom they study 'believe' this or that, or 'believe' in certain matters . the complexity and dubiety that is constantly present in the translation of the concept should now make us wonder more deeply whether reports of this kind can be so confidently accepted (1972: 38)

The second problem runs deeper. Even if certain native expressions can be translated by 'believe', it would have to be shown that this was a sense of 'believe' troublesome for my thesis. For not every use of 'believe' is best rendered by 'hold to be true' (which is the troublesome sense). 'Trust' or 'support' are better renderings in some contexts. Moreover it is not clear that we are always justified in employing 'believe' in some of the contexts where we do employ it. I have in mind here Wittgenstein's objections to speaking of knowledge, certainty, or belief in connexion with propositions like 'Material objects exist' or 'The universe existed before I was born'. Since the notion of *doubt* is inappropriate in such cases so, he seems to suggest, are those of knowledge and belief. Better than 'I know there are material objects' or 'I believe the world existed before I was born' might be something like 'I am *comfortable* with the proposition that there are material objects' or 'That the world existed before I was born *stands fast* for me' (1969*a*). Generalising from such suggestions, we might argue that even when we can translate some native expression as 'believe' we ought not to do so, because the use of 'believe' that we translate into is a wrong or objectionable one. Why impose our objectionable talk, if that is what it is, onto the natives? I do not, of course, want to suggest any great similarity between the propositions that interested Wittgenstein and those

that concern us. I mention him only to help stress the point that attribution of beliefs to people on the alleged ground that they *say* they have them is fraught with difficulty. *Just* what is it they are saying?

How else might it be established that the natives believe the anomaly-making propositions? Typically we identify a belief on the basis of what men do and of what other beliefs we already take them to have. To the extent that what a man does and believes is only consistent with his also believing that *p*, to that extent we are usually reasonably confident that he does believe that *p*. There are obvious problems, though, in ascribing beliefs in all the anomaly-making propositions by these criteria. If a proposition is held to be untestable, then it cannot be thought to have any discoverable consequences if true that it would not have if false—for if the consequences were different, discovery of which set obtained would constitute some test of the proposition's truth Given this, it is unclear what would make behaviour suitable to, or consistent with, the belief that the proposition is true rather than false. Having no discoverable consequences, how can the proposition serve as a guide to behaviour? I am reminded here of another passage in Wittgenstein where he asks 'What does it mean to believe Goldbach's theorem?'—the problem being that there is no construction known by which the conjecture (that every even number greater than two is the sum of two primes) could be established. He points out that it is unclear what the belief could be to the extent that it is unclear what its behavioural consequences are (1969*b* para. 578) Even more obviously we cannot conclude that the natives believe each of the anomaly-making propositions to be true on the grounds that only then would their collection of beliefs be consistent. What makes the propositions anomaly-making is precisely that a person who believed all of them would be inconsistent. This test, applied strictly, would force us to conclude that at least some of the propositions are not believed [20]

Even if there is no clear evidence that the natives believe each of the troublesome propositions in an apparent anomaly to be true, it will not follow—so someone will argue—that they regard any of them as indeterminate in truth-value. Why should it not be that they simply suspend judgement in such cases? They are not assigning a third truth-value, but are simply unsure which of the two values, true or false, to assign As Stroud has pointed out, assigning a third truth-value must be

> incompatible both with assent and dissent, and yet be a genuine verdict, and not simply the expression of uncertainty, doubt, inclination to believe or disbelieve, ignorance, or whatever (1968–9 90).

If we focus only upon native reactions to single, non-compound propositions, we shall not be able to distinguish between their regarding them as indeterminate and their simply expressing doubt. For any single proposition to which someone using a three-value logic will assign the value indeterminate will be one that someone using a two-valued logic will suspend judgement on. But evidence may be forthcoming in the tautologies and inconsistencies that people recognise. Suppose '*p*' is a proposition the natives do not judge to be true and do not judge to be false. Suppose, too, that they do not reject '*p* and not *p*' as false, nor embrace '*p* or not *p*' as true. That will constitute evidence that they regard '*p*' as indeterminate. For were they simply suspending judgement over its truth or falsity, this would not

prevent them insisting that '*p* and not *p*' must be false, and '*p* or not *p*' must be true. Putnam has said:

> a man reveals that he is using three-valued logic and not the ordinary two-valued logic (or partially reveals this) by the way he handles sentences which *contain* components whose truth-value is not known (1957: 76).

Now by this sort of test—if what I have earlier assumed or argued is granted—we can conclude that the natives do employ a third truth-value. The evidence for this will be their indifference towards those tautologies and inconsistencies of standard logic which contain components they regard as untestable.

At the beginning of section III I said we had to distinguish between the questions of whether the magico-religious anomalies could be dissolved by imposing a three-valued logic upon them, and whether primitive thought actually permits of such an imposition. To some degree, though, this distinction is artificial, for a major reason, inevitably, for thinking that a system of thought *does* incorporate certain features is that, on this supposition, difficulties and anomalies within it can be smoothed out. (To this extent I endorse the principle of charity) Such suppositions are, of course, defeasible by overwhelming counter-evidence In this section I have tried to show there is no overwhelming evidence against the supposition that magico-religious thought incorporates an alternative logic of the type described. Since this logic can, I hope, serve to smooth out the difficulties and anomalies, my supposition should be allowed to stand.

VI

The whole of this exercise would not seem too fruitful if it could be shown, as many logicians claim, that L3 (and perhaps any alternative logic) is not an intelligible, coherent system. We should then have transferred the primitive from the frying-pan to the fire Instead of accusing the primitive of 'child-like' bungling of a universal, standard logic, we should be accusing him of employing an incoherent logic. Still, it is worth stressing how very different the accusations are, and that there is a good sense in which one has come to understand a people's thought by uncovering its principles even when the principles may, in the last resort, be unintelligible. There is, for example, all the difference between being dumbfounded by apparently arbitrary behaviour and seeing behaviour as a natural consequence of certain religious doctrines, even though the doctrines may dumbfound us by their peculiarity. So even if there are no intelligible alternatives to standard logic, it would not be without value to interpret the thought of some peoples as incorporating alternative, albeit incoherent, logics

In fact I do not regard L3 as incoherent. But that is a philosophical issue which I do not want to go into in this exercise in applied logic The issue, as I see it, would surround the intelligibility or otherwise of the third truth-value, indeterminate. Defence of this truth-value would, I imagine, take the form of arguing that once all connexion between the notions of truth and falsity on the one hand and of verification on the other are severed, the former notions lose their sense After all, it might be said, we *learn* these notions by being made to apply 'true' and 'false' to those propositions we have seen verified and falsified respectively. And it might

seem that to insist on the truth or falsity of propositions which are in principle untestable is to remove the basis on which our grasp of these notions is founded.[21]

Let me add, finally, that I hold no specially strong brief for L3 in particular. My interest has been more in suggesting how aspects of primitive thought can be approached in terms of a non-standard logic. It is quite possible, if the spirit of this approach wins favour, that different non-standard logics might be suitable for different aspects or different peoples. I hope, in other words, to have suggested an area of ethnographical research and a methodology—an area, moreover, in which philosophers and ethnographers could, and would have to, collaborate.

NOTES

[1] This point is made more explicit in Jarvie & Agassi (1970).

[2] It is not usually particularly easy to eke out contradictions from such peculiar 'identity' beliefs. Thus the Halkomelem apparently believe they *are* otters—but not, it turns out, ordinary otters as opposed to very special 'otter-men'.

[3] cf. Evans-Pritchard 1956 ch. 6

[4] A logical truth (or tautology) can likewise be characterised as a proposition whose truth results from nothing but the behaviour of the operators As the logician W. V. Quine has neatly put it in a number of places, a logical truth or falsehood is one in which only the operators occur 'essentially'—i.e the meanings of the other expressions occurring in the proposition are irrelevant to its truth or falsity.

[5] I would have used 'anti-logical' rather than the even uglier 'contralogical' to characterise (A2) were it not that Lévy-Bruhl explicitly rejects that label. Why he does so he does not say, and I should have thought it a better term for his purposes than 'pre-logical'. I do not use the latter, partly because it is unsuitable, and partly because it is too closely associated with Lévy-Bruhl's particular version of (A2).

[6] 'Standard (etc.) logic' refers, of course, to the constructed systems of logicians—but it is supposed to reflect and uncover the principles you and I allegedly employ in our everyday thought (our 'logic-in-use' as Kaplan (1964) has called it) Hence we will be examining alternatives to our everyday thinking

[7] The logician's definitions will not be of the English expressions 'either . . . or . . .' etc, but of special symbols for disjunction, negation, conjunction, etc Still, to the extent that certain English expressions approximately perform those functions, the definitions will roughly fit them. On the relationship between ordinary English and the logician's symbols, see Strawson 1952

[8] I have attached to these tautologies of SL the names of some traditional 'laws of thought', but it is by no means obvious that these laws are best represented by the corresponding tautologies. A 'deviant' logician will probably insist on different representations.

[9] I make no special mention of the *rules* of SL since, for our purposes, they can be thought of as tautologies put to the job of deriving propositions from propositions.

[10] See Reichenbach 1944 For descriptions of L3, see Ackerman 1967 (chapter 5), and Zinov'ev 1963.

[11] It is worth noting that the definitions in L3 are 'normal', to use Rescher's expression (1968. 78–9). That is, they differ from those in SL only where one or more of the component propositions in a disjunction, conjunction, or whatever, is indeterminate.

[12] Other philosophers have had different reasons for postulating the indeterminate truth-value. For example, Waismann (1968) has urged that certain reports on experience should not be counted as true-or-false because of the 'vagueness' of, e g., our colour-terms.

[13] The letters 'A', 'B', etc., unlike '*p*', '*q*', etc. can stand for, or range over, *compound* propositions. Thus an example of Reichenbach's 'true conditionals' would be· 'If the particle passed through slit x or through slit y, then the probability that it will hit point z is d'.

[14] Actually Birkhoff and Von Neumann had, in 1936, proposed a rather different revision of standard logic to deal with the anomalies.

[15] See Putnam 1969: 232.

[16] Regarding propositions as indeterminate because of their vagueness is, of course, different from doing so because of their in principle untestability. But the views have obvious affinities as well. See Waismann 1968, and Körner 1969· 55 sq.

[17] Lévy-Bruhl's early books, of course, were written before the development of logics like L3.

[18] Does anyone maintain this strong thesis? Jarvie and Agassi (1970) profess to find it, with lapses by the author, in Beattie's *Other cultures*. Beattie has rejected this reading (1970). I confess I read *Other cultures* like Jarvie and Agassi, so perhaps the strong thesis can be attributed to the Beattie-of-common-repute if not to the actual John Beattie Incidentally, he has repudiated the charge of having the *intention* to save primitive thought from accusations of 'backwardness and irrationality' (1970: 248). Even so, he clearly regards this as a welcome consequence of the 'symbolist' approach.

[19] These terms are Lévy-Bruhl's and Evans-Pritchard's respectively.

[20] I would not want to rely too heavily on the outcome of this test, since like the principle of charity it can be used in an objectionably indiscriminate manner to show that no one is ever inconsistent

[21] For an excellent, thought inconclusive, discussion of this issue, see Dummett 1973: 467–70, especially.

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KROEBER *v.* RADCLIFFE-BROWN ON KINSHIP BEHAVIOUR: THE FANTI TEST CASE

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The relationship between language categories and the categories implicit in other aspects of culture (e.g. thought, behaviour, 'structure', etc.) has long been of serious anthropological interest. The Kroeber-Radcliffe-Brown controversy—whether or not our behaviour towards kinsmen segments in the same way that our kinship terms do—is central to this language-culture problem. In this article the positions of the two are described, the basic issues of their argument are extracted, and a logical typology of the possible terminology-behaviour relations is constructed. Data collected in a study of kinship terminology and behaviour among the Fanti of Ghana are shown to fall into only one of the types. In a narrow sense, Kroeber is found to be the 'winner' of the controversy, but, additionally, a new formulation of the language-culture relationship is described which makes sense of the regularities to which Radcliffe-Bown was responding and which Kroeber ignored.

I

Early workers in cognitive anthropology (or ethnoscience, as it was then called) along with many other anthropologists (cf. references below to Radcliffe-Brown and his students) tended to consider that linguistic categories were the categories by whose means people in a culture thought of the world and behaved towards it. There is now a growing awareness that semantics is not equivalent to cognition, or that there is at least some lack of fit between semantic categories and non-linguistic categorisation. Keesing, for example, has partially recognised the problem (1972, also cf. Ardener 1971: xxvi and lxxxv), but offers no good description of the fit that he is denying (cf. 1972: 303–5) and no analysis of the types of fit which might logically be possible.

Keesing shares these failings with most others who have written on the topic. There have been almost no anthropological studies to date of the cognitive structuring of non-linguistic categories and relations, and thus no comparisons of non-linguistic cognitive structures in some given domain with equivalent linguistic domains. Gladwin does discuss the problem (Gladwin & Kronenfeld in preparation) and Gladwin & Gladwin (1971) do explicitly deal with aspects of the problem in the context of Fante fish selling. D'Andrade (1965: 1974) offers a good analysis of how both anthropological and native semantic systems strongly skew our (i.e. anthropologists') supposedly non-linguistic observations. But these works are the only ones that I am familiar with which actually deal with this problem.

The present article describes one part of a study which explicitly investigated the fit between linguistic categorisations and the non-linguistic cognitive categorisations implicit in behaviour. The study was particularly focused on the relationship between linguistic categorisations and behavioural categorisations of kinsmen by the Fanti of Ghana.[1] Kinship was picked as a domain since its semantics are com-

Man (N.S.) 10, 257–284.

paratively well understood and since work on kinship has played a central role in the development of anthropological theory.

The article describes a typology of the kinds of relationship that one might expect to find between terminological categories and categories of behaviour. The typology is based on discussions by kinship theorists, and the data used for finding the occurrent types come from my Fanti study. The terminology-behaviour relationship is examined in terms of the old Kroeber/Radcliffe-Brown controversy because no later work that I am aware of seems to have attacked the problem as directly or as well as this work (with the partial exception of the article by Pospisil (1964) which is discussed later). Southwold (1971) does discuss many of the problems raised in this article, but I think without successfully bringing into focus the questions that his own observations raise. However I shall have to wait until after I have presented the problem and my solution to it before discussing what I feel to be the inadequacies in his treatment

Kroeber stated in 1909 that kinship terminologies were to be understood as linguistic phenomena. First Rivers and then Radcliffe-Brown disagreed with him (cf. Radcliffe-Brown 1941: 59–64), claiming that kinship terminologies do accord directly with behaviour. The kinship question has been only one aspect of a more general linguistic and philosophical argument concerning the relation between linguistic categories and our perceptions or conceptions of the things labelled by the linguistic categories. Some have felt that our linguistic categories simply reflect our conceptions of the world, while others have claimed with Whorf, that our linguistic categorisations dictate the form of our thought But, whatever the direction of causality, these linguistic theorists have maintained, as did Radcliffe-Brown, that the categories of our language largely correspond to the patterns by which we conceive of and behave towards the things labelled by those categories. I maintain that, on the contrary, Kroeber was right, at least in terms of the argument as he and Radcliffe-Brown then saw it, and I adduce data below to substantiate that claim. For me, and I think for Kroeber, the problem lies in Radcliffe-Brown's claim that there is a direct one-to-one relationship between kinship nomenclature and social practice. The problem does not lie in his feeling that we should expect kinship terminological categories in some way to express information relevant to social practices among kinsmen, a feeling I share.[2]

A solution to the problem of the nature of the relationship between kinship terminologies and social practices first requires well-defined conceptions of what we mean by the two terms. Different conceptions may well imply different solutions to the disagreement. What kinship nomenclature is seems clear enough By the commonest interpretation it refers to kinship terminological systems, i.e. to the set of terms used to classify kinsmen (and, thus, to the kinsmen denoted by each term). For some time now we have had a reasonable understanding of how these terminological systems are organised, even if that understanding is still less than total. By another interpretation—which I shall consider in this article—kinship nomenclature could refer to the set of terms by which kinsmen are organised into social groupings such as lineages. There has also been much anthropological analysis of these categories, though not in an attempt to resolve the present argument.

What has been lacking in anthropological discussion has been any clear and

systematic consideration of what we mean by social practices. Particular behaviours, attitudes, or sets of these have often been spoken of with regard to some terminological category, but no one has systematically looked at the relation of such behaviours to one another, at the range of applicability of these behaviours, nor at the criteria for applicability. My conclusions in this article are based on a broad but clearly defined concept of social practices: the social practices of kinship refer to those actions or attitudes that are directed by one kinsman towards another kinsman, and that seem either to informants or to the ethnographer to pertain to kinship relations. Hereafter, for convenience, I shall refer to social patterns so defined as 'behaviours'. A broad definition is used in order to assure that any behaviour patterns germane to the argument at hand are included There also seems to be some disagreement in anthropology about whether the behaviour relevant to kinship terminologies is that behaviour which actually occurs between real individuals or whether it is that which natives conceive as being appropriate to certain kinds of relatives. Both aspects of behaviour have been used in this study.

II

Having now characterised the basic disagreement, I would like to turn to a more detailed analysis The Radcliffe-Brown practice has been to write as if the kinship terminology provided the basic behaviour categorisation of kinsmen for the people being described That is, the practice of anthropologists of this school has been to write as if certain behaviour was appropriate to certain named classes of kinsmen, and other behaviour to other named classes. For present purposes I wish to examine the relationship between the occurrence or non-occurrence of a pattern of behaviour and a) shared membership in a kin-term, and b) genealogical variables such as distance and seniority. The four logically possible types of conclusion which might derive from this comparison are characterised below. Radcliffe-Brown and Kroeber are cited for the views that they appear to have held. Since my concern is with the general question of the relationship between kinship categories and behaviour in anthropological theory, I have cited where appropriate a few other important anthropologists who seem to have held positions somewhat at variance with those of Kroeber and Radcliffe-Brown. However, I do not mean to imply that these writers have never held other views on the problem. The citations illustrate the reality of the problem, and thus the need for real data. No attempt has been made to trace or document systematically the literature relating to the various views.

1 *Traditional view*. Much traditional anthropology appears to have assumed a) that kinship terminological categories were relevant to behaviour and that there were appropriate and different ways of behaving toward each labelled category of kinsman, and b) that kinsmen were perceived on a genealogical grid, with some kinsmen being 'real' members of the category or class, and others 'classificatory' members.

According to this view, each kin-term has appropriate forms of behaviour for it, but the incidence of the behaviour attenuates with genealogical distance from ego. Thus one might expect the paradigm:

VIEW 1. Kin categories and genealogical grid relevant.

		A			B		
Kin-term Genealog Feature (e.g., gen. distance)		1	2	3	1	2	3
Behaviour	A	H	M	L	0	0	0
	B	0	0	0	H	M	L

H=High frequency, M=medium frequency, L=low frequency, 0=Absence or near absence.

Radcliffe-Brown (1924—reported in Radcliffe-Brown 1952) gives a classic presentation of this view in 'The mother's brother in South Africa' In particular, he says:

> But on this hypothesis that I have put forward the matter is fairly simple In primitive society there is a strongly marked tendency to merge the individual in the group to which he or she belongs. The result of this in relation to kinship is a tendency to extend to all the members of a group a certain type of behavior which has its origin in a relationship to one particular member of the group (1952 25)
>
> The characteristic of most of these societies that we call primitive is that the conduct of individuals to one another is very largely regulated on the basis of kinship, this being brought about by the formation of fixed patterns of behaviour for each recognized kind of kinship relation (1952· 29)

Elsewhere Radcliffe-Brown says.

> We may say, then, that in general there is a certain pattern of behaviour for each kind of relative, to which an individual is expected to conform in his dealings with any relative of that kind, subject to important modifications according as the relationship is near or distant (Radcliffe-Brown 1930 45–46)[3]
>
> If in an Australian tribe I observe in a number of instances the behaviour towards one another of persons who stand in the relation of mother's brother and sister's son, it is in order that I may be able to record as precisely as possible the general or normal form of this relationship abstracted from the variations of particular instances, though taking into account these variations (1940 4, reprinted in 1952 192)

Many British social anthropologists have quarrelled with Radcliffe-Brown's extension of sentiments theory, but have accepted the categorical importance of kin-terms for behaviour Goody (1959 71, 85), for example, in attacking Radcliffe-Brown, uses a genealogical sub-categorisation (as opposed to Radcliffe-Brown's genealogical ordering) to account for internal variation of behaviour in a terminological class of kinsmen Goody, thus, still uses genealogical facts to arrange members of a labelled kinship category in relation to behaviour. Southwold (1971), more recently, presents a particularly sophisticated version of this view which I will discuss in some detail later Among American students of Radcliffe-Brown, Tax (1955), while elaborating his analytic framework, appears to have followed him in assuming the equivalence of terminological and behavioural categorisations.[4]

2. *Structuralist view* Since 1949 a non-traditional view has developed which shares the assumption of the relevance of kin-term categories for behaviour with the traditional view, but which denies the relevance of the genealogical grid, claiming 1) that 'kinship terms' are not properly defined by genealogical information, and 2) that all members of a terminological class are equally members of the class, or at least that any internal variation in the class is ordered by non-genealogical facts like economic relations and community residence (but not household membership or the like).

According to this view, each kin-term has forms of behaviour appropriate to it, and presumably non-genealogically based variation is expected to average out across genealogical distinctions (i.e., all members of a single, named class are treated the same for all relevant behaviours)—certainly, if one assumes all the people live in one town. In this case the expected paradigm would be:

VIEW 2. Kin categories relevant, genealogical grid not relevant.

Kin-term		A			B		
Genealogical feature		1	2	3	1	2	3
Behaviour	A	Y	Y	Y	o	o	o
	B	o	o	o	Y	Y	Y

Y = average frequency; o = absence or near absence.

Leach gives a basic characterisation of this view in his paper on Trobriand clans (1958: 123, 124, 143). In his paper on Jinghpaw kinship terminology, and in reference to the passage from Radcliffe-Brown just quoted above, he says:

> In this quotation, the 'persons who stand in the relation of mother's brother and sister's son' might be taken to mean either persons standing in that blood relationship, biologically defined, or persons categorized by a particular pair of native kinship terms Identification of these two meanings may lead to confusion The focal point of apparent norms may thereby be displaced For example, in the system I shall describe, the mother's brother and the wife's father fall into the same term category, *tsa*, which includes also other relatives There may be a norm of behaviour which characterizes a man's attitude towards his *tsa*, but it is not legitimate to assume that this characteristic attitude is especially typical either of the respect for a mother's brother or of that for a wife's father (1961 · 30, reprinted from Leach 1945).

These statements of Leach's as well as others by Needham (*e g*, 1962: 45, 85) seem to imply the kind of view which I have here called 'structuralist'.

3. *Independence view.* The third view would be that kin-term categories and behaviour are both based on genealogical information, but are independent of each other.

This view implies that behaviour should be ordered along genealogical dimensions of distance, sex, generation, and the like, but should be approximately constant across kin-terms. The resulting paradigm would be:

VIEW 3 Genealogical grid relevant, kin categories not relevant

Kin-term		A			B		
Genealogical feature		1	2	3	1	2	3
Behaviour	A	H	M	L	H	M	L
	B	H	M	L	H	M	L

H = high frequency; M = medium frequency, L = low frequency.

Kroeber, in 1909, suggested a version of this view, although he was more concerned with its implications for the history of sociological institutions than with its general implications for categories of behaviour:

> In the Dakota language, according to Riggs, there is only one word for grandfather and father-in-law. Following the mode of reasoning sometimes employed, it might be deduced from this that these two relationships were once identical. Worked out to its implications,

the absurd conclusion would be that marriage with the mother was once customary among the Sioux . . .

The use of such identical or similar terms for distinct relationships is due to a considerable similarity between the relationships. A woman's male cousin and her brother-in-law are alike in sex, are both of opposite sex from the speaker, are of the same generation as herself, and are both collateral, so that they are similar under four categories. In view of the comparative paucity of terms as compared with possible relationships, it is entirely natural that the same word, or the same stem, should at times be used to denote two relationships having as much in common as these two . . .

A woman and her sister are more alike than a woman and her brother, but the difference is conceptual, in other words linguistic, as well as sociological It is true that a woman's sister can take her place in innumerable functions and relations in which a brother cannot, and yet a woman and her sister, being of the same sex, agree in one more category of relationship than the same woman and her brother, and are therefore more similar in relationship and more naturally denoted by the same term .

It is a much more conservative view to hold that such forms of linguistic expression and such conditions (as the Levirate or group marriage) are both the outcome of the unalterable fact that certain relationships are more similar to one another than others. On the one hand this fact has led to certain sociological institutions, on the other hand, to psychological recognitions and their expression in language To connect the institutions and the terms causally can rarely be anything but hazardous (1909: 82, 83).

Many people, I think, since have felt this view to be the case, but have decided that their feelings were mistaken and that they had simply not been careful enough to find the categorical differences that must exist. Ardener (1971: xxvi and lxxxv) notes at least a transitory situation in which he would claim a position similar to this view. Pospisil (1964: 400–4) is one of the few people to my knowledge who has tried to link up the distinctive features of a componential analysis of the kinship terminology with behaviours—instead of trying to link the behaviour to the labelled categories.

4 *No relation view.* A fourth logically possible view, though as far as I know, one not proposed by anyone, would be that kin categories have nothing to do with behaviour. In its strong form, this view would presume the irrelevance of any genealogical information, producing a paradigm like

VIEW 4 Kin categories and genealogical grid not relevant

Kin-term		A			B		
Genealogical feature		1	2	3	1	2	3
Behaviour	A	Y	Y	Y	Y	Y	Y
	B	Y	Y	Y	Y	Y	Y

Y = average frequency

This view states a null position: that behaviour is homogeneous for the whole town—or at least is unaffected by the distinction between kinsmen and non-kinsmen. This is the situation which we ideally expect to occur in the behaviour of traffic police toward those who exceed the speed limit.

III

The research problem was to find out what behaviour fell into which of these classes. Most existing analyses of kinship imply that at least some kinds of behaviour

should fit the first or second views; Kroeber's position seems to have gone out of style.

The present article will not suggest total support for any of these four ideal views. It will suggest 1) that View 3, the independence view, is the closest approximation to the situation that obtains with most behaviours, 2) that the chief effect of terminological categories is an indirect one, which operates through a biasing of underlying dimensions (e.g., real generation being modified by putative generation), rather than a direct effect which would provide the basis for the assignment of behaviour. This article will suggest, thus, that terminological categories do have some independent effect on behaviour, other things being equal, but only a relatively small effect,

Views 1 and 2, the traditional and structuralist views, in their abstract form, do not imply any particular causal ordering between behaviour and terminological classification. Leach, in his Trobriand paper, seems to suggest a behaviour-causes-terminology causal sequence. Other anthropologists, taking a more Whorfian approach, have often implied that kinship categories, in effect, told people how to behave to one another. These considerations of causality are, for present purposes, largely irrelevant since either causal sequence would imply the same correlations. Views 3 and 4, the independence and no relation views, imply no particular direct relationship, causal or otherwise, between kin-term categories and behaviour. In so far as the present article stops short of View 3 (cf point 2 above), it does recognise that categorical assignments have a minimal relevance to behaviour but it should be stressed that these kin-term categorical effects are only considered to be modifications of essentially kin-term-independent behavioural distinctive features.

While strictly this article is only concerned with kin-term categories, the data also pertain to another kind of Fanti kinship category, the lineage. In looking at the effect of kinship categories on behaviour we will also want to consider the effects of shared lineage membership. The Fanti kinship terminology differentiates lineage members from nuclear or domestic family members in a number of terms:

for male ego:

	nuclear family	both	lineage
Generation +1	*egya* (father)	*na* (mother)	*wofa* (uncle)
0		*nua* (sibling)	*akyereba* (sister)
—1	*ba* (child)		*awofasi* (nephew/niece)

The same differentiation of lineage considerations from nuclear family considerations accounts for the marking off of the skewed set of expansion-reduction rules from the unskewed set. Therefore, in testing the relevance for behaviour of the semantic dimensions which underlie the terminology, I also am testing the relevance of lineage membership to behaviour.

At the minimum one would expect the lineage to be relevant to those behaviours which are important to the natives' definition of it (in the Fanti case, inheritance, certain funeral obligations, and certain marriage restrictions). Much anthropological theorising[5] leads one to expect that some behaviours beyond those few that are definitionally related to the lineage should also have a pattern of occurrence based on lineage membership.

There are two ways in which lineage membership might affect the incidence of

behaviour: a) One might take the lineage itself as a behavioural unit. Here, one would predict the existence of behaviours limited to the lineage, but frequencies of direction of the behaviour to different members of the lineage would be ordered by genealogical features (traditional view) or other information (structuralist view).

b) Alternatively, one might consider the kin-term categories to be the units of behaviour, and expect that lineage membership would produce behavioural differences between pairs of categories that were identical in all but this one feature. In this case one might expect that, where a terminological category contains both lineage members and non-lineage members, lineage membership would produce some sharp behavioural breaks between the two sub-sets of the single terminological category. As before, the traditional view would imply internal variation along genealogical lines while the structuralist view would imply internal variation along other lines.

In either of the above cases the independence view still states that behaviour does not depend on formal kinship categories, but on basic genealogical relations which underlie the formal categories (lineage membership *not* being one of these basic dimensions). The no relation view still considers behaviour to be totally independent of kinship-relevant information.

This article will make essentially the same claim about lineage categories as it made about kin-term categories Lineage categories are relevant to behaviour in the trivial sense of mapping on to those behaviours which the Fanti consider to be definitionally tied to lineage membership; they are not in general relevant to any wider set of behaviours (as much anthropological work would have led us to expect). Lineage membership does seem to have the one added effect of extending the behavioural effects of kinship *per se* in much the same manner as lineage membership extends the distance at which kinship normally is terminologically recognised.

IV

My study was conducted between September 1967, and September 1968, in the Fanti fishing village of Egyaa No 1 on the coast of Ghana's Central Region in west Africa. The village has a population of *c* 360 people, including *c*. 60 adult men. Most men fish, the remainder farm (as also do some fishermen). Gin distilling is a major secondary occupation Women dry and sell fish, farm, and distill gin The village appears to have retained much of its traditional culture; it certainly shows no signs of any Westernisation beyond that general to most of rural Akan Ghana, even though it is only one half mile from a major highway.

The Fanti are an Akan tribal grouping of somewhere over 1,000,000 people in the central part of southern Ghana, and are closely related to the Ashanti, who have been described by Rattray and Fortes, among others. They are split into a number of traditional states (cf Christensen 1954: 7–18). Fanti kinship has been described by Christensen (1954), but much of my information differs significantly from that reported by him.

Egyaa No. 1 is in the Anomabu Traditional State. The Fanti coastal towns such as Egyaa and Anomabu have had approximately 500 years of continuous European contact, and thus would seem to be in at least a *stable* contact situation.

Kinship terminology

I have elsewhere (Kronenfeld n.d. *a*) described the Fanti kinship terminology and its analysis in detail. For the present I will only summarise the results of that study. The analysis was a formal one, of the kind described by Lounsbury and was based on data from observation, various kinds of interviewing, and over fifty genealogies.

The Fanti have semi-localised matrilineages which primarily serve to organise funerals and to control inheritance. Inheritance normally goes from senior to next junior within the set of matrilineal heirs of the person who first created or acquired the property being inherited, in the absence of such people, it goes to another junior in the lineage. Men's property normally goes to men and women's to women. Seniority, here, is a compound of generation, relative age, and relative age of maternal ancestresses. The lineage (in the person of its elders), however, retains the right to give the inheritance to anyone it chooses, and in any case will not give it to someone who is incompetent to handle it.

The Fanti kinship terminological system consists of at least three alternative sub-systems, all of which use the same set of lexemes in reference to the same set of kernels; the sub-systems differ in their rules and range of extension from the kernels. My informants recognised the existence of the separate sub-systems, and their use of one or the other at any given time was internally consistent.

Kernel kin-types are the focal or primary kinsmen in the different terminological classes The set of consanguineal kernel kin-types consists of one kin-term for each generation of the nuclear family, and a separate kin-term for the nearest matrilineage member of that generation if that kinsman is not in the nuclear family. Sex-of-alter is not a distinctive feature except where the nuclear family member of one sex in a generation is in the lineage while the one of the other sex is not, in this case the preceding process applies independently to each sex In G^{+1}, 'mother' is in the lineage, but 'father' is not, so there is a term for 'maternal uncle' who is in the lineage (but there is no corresponding female 'aunt' term). In G^{0}, siblings are in the lineage, and so there is only a sibling term for the generation. In G^{-1}, a female ego's children are in the lineage, so for her there is only a 'child' term; a male ego's children are not in the lineage, and so he has both the 'child' term and a term for his sister's children who are in the lineage. Because she is his link to his nephews and nieces, a man also has a special optional term for his sister which is particularly *apropos* when speaking of her children. Finally, the parents of G^{+1} kernel relatives and the children of G^{-1} ones are all grouped together in a 'grandrelative' term. These consanguineal kernels are presented according to their distinctive features in table 1. Non-consanguineal kernels are not discussed here, but are shown to be generated in the same manner as the lineage ones are here (in Kronenfeld n.d. *a*).

The three sub-systems represent three patterns of extension from these kernels. The central pattern which I will refer to as the *unskewed* pattern is based on generational extension from the kernels, but preserves the kernel distinction between mother's and father's sides of the family for male relatives in G^{+1}, and its reciprocal G^{-1} distinction. In this pattern, every consanguineal kinsman of ego is called by the same term as his kernel relative of that generation of the correct side of the family. This pattern is the one most commonly used in speaking of actual kinsmen.

TABLE 1. Fanti kernel kinsmen by distinctive features.

	Nuclear Family	Both	Lineage
		FEMALE EGO	
G+2		[*nana* grandrelative]	
G+1	*egya* father	*na* mother	*wofa* maternal uncle i.e. mother's brother
G0		*nua* sibling	
G−1		*ba* child	
G−2		[*nana* grandrelative]	
		MALE EGO	
G+2		[*nana* grandrelative]	
G+1	*egya* father	*na* mother	*wofa* maternal uncle
G0		*nua* sibling	
		(*akyɛrɛba*) (sister)	
G−1	*ba* child		*awofasi* sister's child
G−2		[*nana* grandrelative]	

The next pattern, the skewed one, is less commonly used than is the unskewed one, but is considered by all informants to be the more 'correct' system It adds one self-consciously held specific terminological equation to the generational base of the unskewed system: One's mother's brother's child (*wofa n'ba*) is equated with one's own child (*ba*), moving him down a generation Father's sister's child (*egya n'awofasi*) is reciprocally equated with father or mother (*egya* or *na*), rising a generation. Relatives derivative from these connexions move up or down accordingly, producing a Crow Type 1 pattern (cf. Lounsbury 1964). Informants feel that this equation places together otherwise dissimilar kinsmen, a feeling that they do not have about the equations implicit in the unskewed system. This equation appears to be counter-intuitive to them, apparently because it breaks up the generational pattern of their terminology, and so they feel a need to explain it. The explanation they give is very specific; B, the sister's son, inherits from A, his mother's brother;

formal kinship obligations are among what is inherited; and thus B may become 'father' to C, his mother's brother's child. No such explanation was offered for any part of the unskewed pattern, and this equation represents the only basic difference between the two patterns since all the other differences in terminological labels of kinsmen follow logically from this one (see fig. 1).

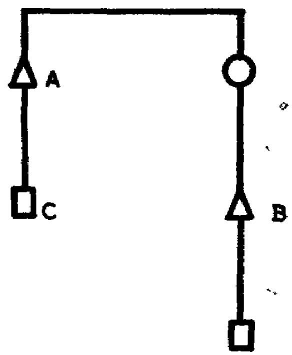


FIG 1

The third pattern is the *courtesy use* of kin-terms to non-kinsmen. This is the most common use of kin-terms in the village. In this pattern of extension the *nuclear family* terms (and the grandrelative term, but not the lineage terms) are extended to everyone in town according to approximate relative age (biased a little by wealth and social importance)

V

The behavioural study

The behaviours used in the text were selected as follows. I found as many discrete behaviours as I could that informants could unambiguously refer to and that they considered relevant to kinsmen. I added a few behaviours that I considered relevant Good, unambiguous frames were developed in asking about these behaviours in a set of interviews. I then asked a sample of about forty-five informants to answer the following two types of questions (in Fanti) for each behaviour: 1) Do Egyaa people do X with their fathers, mothers, etc.? for a list of thirty-five kin-types and three kin class terms; 2) Have you ever done (or can you do) X with Kwesi Abekah, Kwame Gyan, etc.? for a list of about twelve real people who were related to the informant in known ways and who were themselves in my sample. The list of kin-types and kin-terms to which they belong, the questions asked relative to the list of kin-types (K-series questions), and the questions asked *re* real people (P-series questions) are available from me on request.

I ran several checks for consistency first. I independently checked with both people involved for some of the actual behaviour between dyads of real people; people's description of real events agreed more often than not. The percentages of *yes's* and *no's* agreed quite well for pairs of questions such as 'Have you done X

to your father?' and 'Has your son done X to you?' Examination of the pattern of figures has shown the pattern for each question to be internally consistent.

Sample of Informants: All adult fishermen in the village who had reasonably extensive consanguineal relations in the village were used in the sample. A few non-fishermen who had extensive kin relations with the rest of the sample were also used.

It was difficult to get some people to answer particular questionnaires at a particular time and so eventually there was an attrition from sixty down to forty-eight informants (K–A), 34 (K–B), and 23 (K–C) for the K-series. The P-series represents forty-five informants. The difficulties which caused the attrition were a) informants travelling, b) fights between informants and my Ghanaian research assistant, and c) the death of one informant.

In sum the sample represents over half of the adult males in the village and is not particularly self-selected. The 'self-selected' informants whom I first talked to upon arrival in Egyaa turned out to have few relatives in the village, if any, and do not form part of my present sample.

For the P-series, ten or so alters were selected for each informant. The alters chosen were, in so far as possible, relatives, and in the sample, themselves Relatives who were kernel members of kin-term categories, but not in the sample, and non-relatives who were in the sample, were used to fill out the lists, but have not been used in the present analysis, because the concern of this article is with differences between classes of relatives and with certain characteristics (such as relative age, community residence, precise relationship) of these relatives. This concern made it desirable to limit informants' sets of alters for this analysis to ones who were independently known to me, and related to ego.

When I first constructed these lists, the alters were chosen, within the above framework, in so far as they were people whose position in the community was also of interest to me Later, I totalled the numbers of various genealogical types, and constructed subsequent lists in order to fill out under-represented categories. The principal categorisations at that time were kernel/non-kernel, and skewed/non-skewed for each primary male kin-term Thus, most of the Egyaa-resident, kernel, adult, male kinsmen of men in the sample are included, with decreasing percentages of more distant kinsmen living in the town.

To summarise: 1) All informants and alters are co-resident in the same town, and thus the sample is controlled for possible differential access based on general resi-

TABLE 2a. K-series results, skewed system.

	Kin-term	Father				Uncle	
	Kin-type*	F	FB	FZS		MB	
	B–9 inherit from	3	0	0	0	100	n=34
Behaviour							
	B–1 give mon. reg.	97	74	50	50	91	
	B–4 play games with	9	9	15	35	9	
	B–7 advise	12	3	9	38	9	

TABLE 2b. K-series results, unskewed system.

	Kin-term	Father		Uncle	Sibling			Child
	Kin-type*	F	FB	MB	B		MZS	S
					e	y		
	B-9 inherit from	3	0	100	100	97	100	0
Behaviour	B-1 give money reg.	97	74	91	100	100	100	100
	B-y play games with	9	9	9	100	100	100	100
	B-7 advise	12	3	9	94	100	100	100
	C-2 give clothes	87	0	0	65	87	74	100

TABLE 2c Sibling status v lineage membership.

	Kin-term	Sibling-unskewed system → (Sibling-skewed system →)							
		in-lineage			out-lineage				
	Kin-type*	B		MZS	FBS	MBS		FZS	
		e	y			e	y	e	y
	B-9 inherit from	100	97	34	1	0	0	0	0
Behaviour	B-11 serve	100	100	100	97	59	59	97	97
	B-7 advise	94	100	100	97	97	91	9	38
	B-17 arrange wife for	3	97	94	74	9	71	0	0
	C-2 give clothes	65	87	74	74	52	18	0	0

for B's n=34, for C's n=23

Numbers are in percentages, based on the number of yesses over the possible number

*Different kin-types represent different bundles of genealogical features, variation among kin-types represents variation among genealogical features

dence considerations 2) All informants and alters are at least sixteen years old, and almost all are over twenty-three, i e., they are all adult. 3) All subjects and alters are in the same basic socio-economic system and so the sample is roughly controlled for possible effects of differential style of life, education, Westernisation, etc. 4) Most of the adult males in the town who fit the explicit criteria which I describe above are in the sample, implying the unlikelihood of any hidden bias in the selection of the sample.

Results. Tables 2 and 3 illustrate the nature of the results for the K-series and the P-series data, respectively. Only enough material is presented to make clear the nature of the results. In particular these tables are intended to show why positions like those represented by the traditional or the structuralist views are considered unacceptable. The full set of data is presented in endnotes to this paper.[6]

Tables 2 and 3 are modelled after those used to illustrate the four abstract views which were presented earlier in this article. No behaviours are particularly confined to or excluded from single kin-terms, either in the skewed or in the unskewed system. For only one behaviour, inheritance, do the frequencies clearly break on a feature which is also a kin-term boundary. The feature, in this case, is lineage membership which is defined by the Fanti as relevant to inheritance.

TABLE 3. P-series results.

3a. Skewed systems.

	Kin-term	Father					Uncle	
	Kin-type**	F	FB	FZS e	FZS *y	FZS (e+y)	MB	Other
	A-32 can inherit from	29	21	16	0	9	100	68
Behaviour	A-6 serves you	82	86	67	100	91	72	86
	A-9 you taught work	18	7	25	50	29	28	14
	B-2b ever give mon reg	88	50	50	0	43	72	27

3b. Unskewed system.

	Kin-term	Father		Uncle		Sibling			Child
	Kin-type**	F	FB	MB	Other	B e	B y	MZS	S
	A-32	29	21	100	68	100	100	100	
Behaviour	A-6	82	86	72	86	100	88	100	91
	A-9	18	7	28	14	37	63	0	
	B-2b	88	50	72		62	86	33	

3c Sibling status *v* lineage membership.

Unskewed Sibling ⟶ (spans all columns from in-lineage B e through FZS (e+y))
Skewed Sibling ⟶ (spans in-lineage B e through FBS)

	Kin-term	in-lineage			out-lineage						
	Kin-type**	B e	B y	MZS *	FBS	MBS e*	MBS y	MBS (e+y)	FZS e	FZS y*	FZS (e+y)
	A-32	100	100	100	0	0	12	9	16	0	14
Behaviour	A-6	100	88	100	66	100	88	91	67	100	71
	A-9	37	63	0	16	0	75	55	25	50	29
	B-2b	62	86	33	40	0	25	18	50	0	43

Numbers are in percentages, based on the number of yesses over the possible number.

*Total possible number for these was 3 or less, making the percentages useless.

**Different kin-types represent different bundles of genealogical features; variation among kin-types represents variation among genealogical features

This last is the only clear recognition of lineage membership, representing only a minimal recognition of the feature.

The particular behaviours present in these tables have been chosen semi-randomly to illustrate the situation, but the picture is the same for the full set of behaviours as the reader can see in note 6.

Part *a* of each table compares the incidence of several behaviours for the 'father' and 'uncle' kin-term categories (with a special focus on the skewed system). This comparison further contrasts a clearly in-lineage term, 'uncle', with a clearly matched non-lineage term, 'father'. It can be seen in part *a* of each table that for the one behaviour, 'inherit from', the frequencies for all the 'father' kin-types clearly

and sharply differ from the 'uncle' frequencies: fathers essentially do not receive the behaviour, while uncles do. This behaviour shows what I mean by a clear break of the frequencies. For each of the other behaviours the frequencies of the 'father' kin-types clearly bracket those of the 'uncle' ones, some father types having higher frequencies and some having lower ones.

Part *b* of each table considers the incidence of several behaviours to the 'father', 'uncle', and 'sibling' kin-term categories of the unskewed system. Some data for 'children' are also provided, where they help clarify the picture. Again, for inheritance there is a sharp break between 'uncle' and 'father', though a comparison of these with the 'sibling' kin-type makes it clear that lineage membership is the issue, and not kin-term category. The other behaviours again produce a significant overlap between the frequencies of the 'father' and of the 'uncle' categories.

Part *c* is concerned with the comparison of in-lineage 'siblings' with out-lineage ones. The four left columns represent kin-types which are 'siblings' only in the skewed system; the other columns represent kin-types which are 'siblings' in both systems. The results here are consistent with the above picture. Inheritance (which is what the lineage is definitionally concerned with) is structured by lineage membership, but for all other behaviours the in-lineage frequencies clearly and strongly overlap with the out-lineage ones.

There is no separate table that takes the lineage itself as a conceptual unit, but the necessary data are present in tables 2 and 3. To make the overall in-lineage out-lineage comparison one compares 'uncles' and in-lineage 'siblings' with the other kin-types The results of this overall comparison follow the pattern described for the individual parts of the two tables: inheritance alone splits along the lineage *v.* non-lineage division.

In summary, this sample of behaviours and kin-types illustrates the exact form of the test that was used in the study. Inheritance illustrates clearly what I mean when I speak of a behaviour sharply distinguishing a category of kinsmen. These tables show that both in the case of ascription of behaviour to abstract kin-types, and in the case of discussions of behaviour towards actual kinsmen, inheritance does not isolate a kin-terminological category, but rather isolates lineage members; the tables suggest (as is actually demonstrated in the fuller tables of note 6) that no other behaviour isolates either kin-terminological categories or lineage members. The inescapable conclusion is that, at least in the area of Fanti kinship, terminologically defined categories cannot be regarded as the categories of behaviour. The terminological categories do *not* label classes of people who are behaved towards similarly; Radcliffe-Brown and Leach were wrong in thinking that they did. Similarly, lineage defined categories are only trivially (in the sense defined earlier) related to behaviour There is not sufficient space in this article to go into the question of what terminological categories *do* represent; that question is discussed elsewhere (Kronenfeld 1973).

Some considerations of convincingness. Statistical tests of significance and the like have not been used here for several reasons. Tests of the significance of correlations would prove nothing. The discussion assumes that there is some correlation of behaviours with kin-terms (because of shared underlying causes); it only claims that in all cases there exists some other kinship-related information which is as well or better correlated (i.e., that kin-term membership is not the 'key' fact). Thus, the

degree of statistical significance or insignificance of the correlations here does not matter; what would matter would be a general situation in which behavioural frequencies sharply delineated some kin-term—a situation which observably does not here exist (with the exception of P-series A 30). A consideration of the full set of data indicates that for each behaviour there exist categories (such as generation, relative age, closeness) which predict behaviour better than does kin-term membership.

The computations involved in assessing the relative effect of various possible dimensions on the full set of data would be more complicated and would require more effort than is justified by the present point: that terminological assignment is clearly not the single, key, fact for the assignment of behaviour. An eventual goal is a good statistical model of the relative effects of the various features on the assignment of behaviours. Research towards this goal is now being conducted; a first report on that research is now in press (*cf.* Kronenfeld & Kronenfeld n.d.).

A second consideration of *convincingness* concerns the degree to which the data presented represent a single coherent system shared by my informants. This consideration is especially important since the demonstration has been an essentially negative one of 'no special connexions indicated', a conclusion which would result from the sloppy or random collection of data (by asking the wrong questions in the wrong manner). For the P-series, a number of questions are in pairs for both giving and receiving the behaviour in question. Inspection of these revealed a fairly consistent pattern in the figures, though possibly with some bias according to whether one is giving or receiving the behaviour. The breakdowns were sufficiently detailed, also, to allow one to observe how mutually consistent the parts of each table were. Again, the results appear to be reasonable, with relatively few cases of particular figures (i.e., particular dyads of people) which do not seem to mesh reasonably with the rest.

The evidence for the regularity of the K-series was more readily apparent. For the K-series the informants and the rated kin-types were arranged in a Guttman-like scale for each behaviour. (The scale errors were few (e.g. 39–75 errors in 48 informants × 38 kin-types scales) producing coefficients of reproducibility of ·96 to ·98). Informants appear to differ greatly on the assignment of behaviours to *particular* kin-types, but the scales suggest that this is only variation up or down on a single unidimensional scale of kin-types, in each case. This shared scale seems to imply good agreement on the relative importance or weight of the factors, whatever they may be, which underlie the assignment of each behaviour to particular kin-types. Thus, these scales suggest that there do exist shared cultural bases for the assignment of the various behaviours, but that the terminological categories have nothing even approaching a one-to-one relationship to these bases. Where lineage membership is a key feature, as in the cases referred to above, it is very apparent.

VI

Summary

The data considered have been almost totally negative regarding the traditional and the structuralist views discussed above; that is, the available evidence is that the categorisation of relatives by kin-terms is not the basis on which behaviour is

assigned to these relatives. The data only reveal the significance of lineage membership in the trivial sense that behaviour which is tightly tied to the Fanti definition of lineage relations does assume the pattern more or less as it should. There is none of the wider relevance which one would expect if lineage membership were a generally important feature.

The claim is not that kinship categories and lineage relations have no relation to or effect on behaviour; I have suggested previously reasons for thinking that they do have some such effect. The claim is that these categorical facts are not the most important factors in the assignment of behaviour, and that the effect they do have is an indirect one—in the sense that membership in a category affects the evaluation of position on a dimension of relationship (e.g., generation, closeness), which dimensional position, in turn, affects behaviour.

The relevant categories for behaviour

Inspection of the detailed analysis of 'behaviour' and the unidimensional scales referred to above offers some positive reasons for preferring the independence view over any of the other three positions described earlier. It has already been noted that the K-series of behavioural attributes each produces a largely unidimensional scale of kin-types. This fact alone argues strongly for the independence view that there is some underlying genealogical information which is important to the assignment of behaviour. Inspection reveals that relative age and real generation are consistently important factors. Sex, putative (i e., skewed) generation, and genealogical closeness also seem to play a role, as does relatedness itself in some cases. These appear to operate more or less in a dimensional (as opposed to taxonomic) manner

The P-series data again show the importance of relative age and real generation. Genealogical distance, in at least the sense of 'close' *v.* 'far', often matters. Putative generation has some effect, as does lineage membership. Information bearing on sex and relatedness is not included in the kin-types shown.

It would be desirable to be able a) to find which of these dimensions could be subsumed in others, and b) to assign weights (different for each behaviour) to the selected dimensions such that the combination of weighted dimensions would produce scales like those produced by the K-series data and would predict orderings of behavioural frequencies like those produced by the P-series data (though perhaps with slightly changed weightings). This would be the good statistical model that I referred to previously. The information that a Fanti uses in deciding whether or not to behave towards a certain alter in a particular manner would then be summed up in the statements of the dimensions, the kinsman's position on each, and the weight to be assigned to each for a particular behaviour.

The construction of such a model is a problem which is currently being worked on, a first step towards such a model is described in Kronenfeld & Kronenfeld (n.d.). For present purposes, it is only necessary to note that the factors mentioned do have a fairly visible and consistent form, and thus clearly make the relationship between kinship information and behaviour fit the independence view; it is, i.e., a relationship in which membership in a particular terminological category is relatively unimportant.

Putative generation does appear to affect the generational assignment of a kin-type. Often the case seems to be, in effect, that the decision as to whether to take putative or real generation depends on which is higher. Also, lineage membership does extend the limits of 'relatedness' in relevant cases. This especially happens in one's father's lineage. Thus, two situations in which categorical information has been noted to be relevant are consistent with the conclusion that the independence view is the correct one, since their effect is a matter of shifting a kin-type's value on one of the basic dimensions, rather than a matter of isolating some class of behaviour and people.

The research presented here has implications beyond the narrow range of kinship alone. The fact that kin-term categories do *not* provide the categories by which kinsmen are perceived nor behaved towards suggests the possibility that verbal categories in other domains will also not be the categories of action or perception. The fact that the kinds of information (or dimensions of meaning) which underlie the kinship terminological categories are only partially the same as those kinds which underlie the incidence of behaviour among kinsmen further suggests that the connexion between our linguistic categories and perceptions has been over-stressed.

Those kinship theorists such as Radcliffe-Brown who have argued that terminological categories necessarily label classes of people who are behaved towards in some particular fashion are clearly wrong on the basis of the study described here. Wrong for similar reasons are those Whorfian anthropologists and linguists who have claimed that the categories of our language are necessarily equivalent to our categories of conception or understanding.

The set of semantic features by which a given set of linguistic categories is defined seems only partially to be equivalent to the set of features which govern pragmatic interaction with the objects to which the linguistic categories refer. Language, thought, and reality seem to me to be much less closely related than many recent semantic studies have presupposed.

My view of the matter is that both kinship terminological systems (patterns) and the patterns of behaviour of an individual towards his kinsmen are the products of certain basic social relations with certain close kinsmen. But both patterns are extended out from these close kinsmen to more distant ones according to different mechanisms (in the one case related to how the meanings of words are combined and in the other case related to the attributes of kinsmen). Thus, the pattern by which terminological categories are extended is not isomorphic with the pattern of the extension of behaviour, and neither extended pattern can be said to *cause* the other. The one extended pattern is a linguistic categorisation while the other is a social one, but the two are closely related to each other, especially at their core of close kinsmen (cf. Kronenfeld 1973).[7] If the reader will refer back to the passages quoted from Kroeber's 1909 article, he will see that my position is quite consistent with Kroeber's, although in some ways it goes beyond it. The reason for going beyond his position is the desire for a detailed rather than a vague general accounting of the relationship between linguistic categories and behaviour. This concern with detail necessitates actual and precise definitions in places where Kroeber might have preferred general statements. It is this concern for detail that allows us to see why Radcliffe-Brown's concern with the social factors that generated and were

expressed in kinship terminologies, while incorrectly dealt with in his work, was well-founded.

VII

Having defined the problem and proposed a solution to it, I would now like briefly to consider Southwold's difficulties with what I think to be the same problem.

Southwold has been an acute enough observer to realise that neither kinship-related behaviour nor the most frequent use of kin-terms in day-to-day interaction is limited to genealogically defined terminological categories. However, he lacks any concrete systematic data on exactly how the terms get used and apparently lacks even a concrete notion of what appropriate data on kinship related behaviour would look like. This lack of something concrete in hand leads him into at least two important ambiguities in his proposed new theoretical concepts (in spite of his explicit concern with rigorous definition).

'Parentation' is the term he wants to introduce for genealogically-based kinship relations He defines 'parentation to degree *n*' as 'the union of ['parent of', 'child of'] and all possible relative products of these having not more than *n* factors.' His definition clearly refers only to genealogical relatedness *per se*, and *not* in any way to particular terminological categories of genealogically related alters.[8] The problem is that he immediately follows the above definition with.

> Though my exposition may have obscured the fact, parentation is actually a very familiar relation to us · it is what Radcliffe-Brown called 'genealogical relations' (or '— relationships') (Radcliffe-Brown 1952 52) It may be recalled how, in Radcliffe-Brown's exposition, a network of these is created by the interlocking of elementary families And how he continued.
>
> > 'In any given society a certain number of these relationships are recognised for social purposes, i e. they have attached to them certain rights and duties, or certain distinctive modes of behaviour It is the relations that are recognized in this way that constitute what I am calling a kinship system, or, in full, a system of kinship and affinity' (Southwold 1971: 43).

Radcliffe-Brown clearly does not refer simply to genealogical relatedness, but instead refers to genealogically defined roles (most often, kin-terms). This is important because the question of how behaviours distribute across kinsmen is a different one from the question of whether or not any particular behaviour is limited to kinsmen. Furthermore, within the realm of kinsmen Southwold's treatment obscures the problem of whether behaviours (or other 'social relations') are linked to terminological categories (cf. his discussion at end of Section VI [1971: 52, 53]) or to other genealogical variables (cf. Section VII [1971: 53, 54])—or simply to genealogical relatedness *itself* (cf. his definition of 'parentation').[9]

His discussion of what he actually means by 'social relations' is similarly confused, again apparently because he has no particular kind of concrete event in mind. On page 43 he at least implicitly defined social relationships in terms of interaction and separated them from particular native kinship terminological categories. But by pages 46 and 47 social relations are linked with roles, and by page 52 social relations have come simply to represent the norms of behaviour for particular kin-terms. This shifting meaning of his concept of social relations results in his begging both the empirical question of whether or not kin-terms indeed label the

roles that natives actually interact with and the more basic question of whether duties and behaviours are in fact assigned on the basis of *roles* at all (*v.* on the basis of variables as I am claiming in this article).

By retreating from actual behaviour to 'norms' (1971: 46) Southwold falls into yet another confusion. There is no evidence that norms themselves actually exist. That is, natives will make normative statements about behaviour, but such statements always imply a context in which the question is being asked and always imply rules for *proper* implementation of the normative statements. Anthropologists and sociologists often reify such normative statements into norms of behaviour for some particular role—but only at the cost of losing the relevant context and implementation rules Elsewhere (Kronenfeld n.d. *b*) I use the data from this article concerning actual behaviour to real people and putative behaviour to abstract kin-types, as well as other informant statements about proper behaviour towards kinsmen designated by particular kin-term labels, to examine this problem of the meaning of native normative statements

We may summarise Southwold's relevance to the present article as follows. He has made some very good observations about the distribution of social relations (i.e. kinship duties and behaviour, and the actual *use* of kinship terms). But his lack of method of systematically recording or studying actual behaviour separately from norms, and social relations separately from the semantics of kinship terms, forced him to retreat from his good observations back to Radcliffe-Brown's simple equating of behavioural categories with terminological ones. This inadequate theory of the relation of language to cognition and behaviour obscures for him what it is that structures behaviour. It is ironic that his attempts to strip kinship of some of its special confusions should founder because of his taking that category too seriously

In a more general sense Southwold offers us a particularly clear and well-reasoned example of an error that has long bedevilled anthropology. Categorising (or creating taxonomic categories) in the absence of a theory and the theory-generated variables which would generate, in turn, the categories leads to an infinite regress situation: any two events in the world, after all, differ from each other in some sense, and one can never guarantee that there is not some context for which that particular sense is relevant. This regress problem can best be seen in the continuing proliferation of *types* of African political systems, as each new ethnographer finds that his people don't quite match any of the existing 'type specimens.' It is only a theory of what is happening in some given situation that allows us to decide what information is relevant (and hence must be preserved in our data) and what is irrelevant (and thus can safely be ignored) to that situation. This taxonomic problem is most clearly discussed in an anthropological context by Greenberg (1968: 124–32, especially 130).

NOTES

This research was supported by Public Health Service Fellowship No. F1-Mh-34, 116 and Grant No. R04-MH13769 from the National Institute of Mental Health while I was a research associate at the University of Ghana, Legon, for all of which I wish to express my thanks.

I also wish to thank Andrew B. Ashun, Andrew Baring, Roy G. D'Andrade, Charles O. Frake, Hugh Gladwin, Judy Z Kronenfeld, Martin Orans and Naomi R. Quinn for their help at various points in this research. Alan R. Beals, Jerrold E. Kronenfeld, Judy Z. Kronenfeld and Jean C Lave have provided valuable editorial help.

My thanks are also due to the chiefs, elders, and citizens of Egyaa No. 1, Ghana, for their very generous co-operation.

[1] The first paper (Kronenfeld n.d.) describes the terminological analysis That analysis shows the variety of terminological usage patterns that exist, the systematic relations among these, and the social bases of each. The analysis of genealogically-based usage relies heavily on the work of Lounsbury on Crow- and Omaha-type terminological systems (1964; 1965); non-genealogically based ('metaphoric') usage is analysed in a similar way. The analytic fit between genealogical and non-genealogical usage is shown (at least partially solving the kind of problem raised by Bloch (1971: 82)). The relative frequencies of various kinds of usage are similarly presented and explained (also cf. Bloch's comment (p. 82) on frequencies). The second paper (Kronenfeld in prep) presents a formal account of how the Fanti themselves actually calculate terminological relations.

The present article (the third paper) explicitly considers the question of possible isomorphism between linguistic (i.e semantic) categorisation and non-linguistic ones. The fourth paper (Kronenfeld & Kronenfeld n d.) offers a direct analysis of the structure of (categories and relations among categories) non-linguistic behaviour The paper shows one way in which such non-linguistic cognitive patterns may be analysed. The results of the particular study suggest one general but explicit theory about how such cognitive categories are structured, and about how these non-linguistic cognitive systems relate to semantic categorisations of the same domain.

The overall results of the study of the semantics and cognition of Fanti kinship are summarised in the final paper (Kronenfeld 1973).

[2] My own view is that social practice among kinsmen is directed according to some of the same information that structures the terminological classification of kinsmen, but that this information is used quite differently in deciding how to behave toward a kinsman from deciding how to categorise that kinsman (for a fuller discussion see Kronenfeld 1973) In a sense my views are implicit in those of Kroeber, but they also reflect Radcliffe-Brown's feeling that kinship terminologies are used to communicate about social practices and thus should in some manner represent these social practices The problem concerns the nature of the representation, and I think that my conception of it adequately represents the data—in a form that Kroeber would perhaps not be troubled by. Kroeber, himself, did not consider the question of why some societies or languages use some of his 'categories' while others use different ones It is this last question which seems to have been of primary concern to Radcliffe-Brown. It is this apparent arguing past each other by Radcliffe-Brown and Kroeber that allows the logical possibility of a solution that would be consistent with the concerns of both of them.

[3] Radcliffe-Brown recognised that the observed situation could sometimes be cloudy, although without changing his position on the issue, for in the quoted passage he goes on to say, 'For some relationships this pattern is definite and well organised, for others it is vague, and comparatively less important'

[4] In a footnote Tax says, 'These rules may be most fluently stated in so far as they apply to kinship terminology, but in each case not only the term, but the entire relationship is referred to; thus where, later, the word 'calls' is used, one may substitute 'calls or behaves towards' or 'has relationship with' (1955: 19).

[5] Schneider's 'Introduction' to *Matrilineal kinship* (1962) presents a very clear summary of this theorising

[6] See tables 4 and 5

[7] 'Let us look at a particular somewhat simplified example to see what these points mean The feature or attribute of generation is important for the terminological division of focal or kernel relatives because it separates those who are doing the socialisation from those who are being socialised; spanking is one behaviour that is used in the process of socialisation, and so is relevant to the kinship terminological base. In particular, in Figure 2, generation separates one's mother B, father C, and uncle D, from one's sibling, A, who is socialised together with ego, and from one's child, E, and sister's child, I for whom one is the socialiser. Lineage membership separates uncle from father, and child from sister's child, and sex separates mother from father

These same features provide the basis for the relative product equivalence rules. One of these rules places father's brother, G, in the 'father' category. The rule is based on the generation, nuclear family, sex, and lineage equivalence of true same-sex siblings, but the actual application of the rule is dependent only on the fact that the kinsman to whom the rule is being applied is a male who can be referred to as 'sibling' by father. Thus, the rule applies equally well to father's paternal half-sibling, H, who does not share father's lineage Similarly another rule puts father's sister's son, F, in the 'father' category where the rule (but, again, not its application) is based on the shared sex and lineage membership of a man and his true sister's son.

TABLE 4. K-series results.

Kin-type	FBS	MZS		B		MBS		FZS	MB	FB	F	Ques. No.
			y	e	y	e	y	e				
Egyaa people X their ———												
												A n=48
Can abuse their ———	77	85	96	23	77	44	15	0	0	2	0	1
Can work-punish	79	88	98	31	79	38	19	6	2	2	0	2
Can summons to court	83	83	85	81	83	83	81	79	73	77	73	3
Can report to elder	96	94	94	90	92	92	94	94	88	90	90	4
Can advise	100	100	100	100	100	100	100	100	100	100	100	5
Can pinch	65	75	77	13	50	17	8	4	2	4	2	6
Can spank	77	83	90	8	75	15	17	2	0	0	0	7
Can pull ear of	65	73	79	10	75	17	13	0	0	0	0	8
Can warn	79	88	94	27	63	33	15	0	0	2	0	9
Can shout at	79	88	56	46	73	63	23	0	0	2	0	10
Can cane	79	42	94	13	58	23	15	0	0	2	0	11
Can knock	73	58	85	13	52	19	13	0	0	2	0	12
Can hit	73	81	88	10	54	23	15	0	0	2	0	13
Can beat	67	79	85	13	54	21	15	0	0	2	0	14
												B n=34
Do give money regularly to	100	100	100	100	62	53	50	50	91	74	97	1
Do give money when see	0	0	3	0	0	0	0	0	0	0	0	2
Do talk freely with	100	100	100	100	97	100	94	97	97	94	100	3
Do play games with	100	100	100	100	88	71	35	15	9	9	9	4
Do sit with at cases	100	100	100	100	88	88	88	88	100	97	100	5
Do help at cases	100	100	100	100	94	94	94	94	100	97	100	6
Do advise	97	100	100	94	91	97	38	9	9	3	12	7
Do receive death-gifts from*	6	97	97	100	0	0	0	0	100	3	3	8
Do inherit from*	3	100	97	100	0	0	0	0	100	0	3	9
Do respect	100	100	100	100	100	100	100	100	100	100	100	10
Do serve	97	100	100	100	59	59	97	97	97	97	97	11
Do find work for	88	97	94	3	32	3	0	0	0	0	0	12
Do teach work to	91	97	91	3	91	3	3	0	0	0	0	13
Do move with	100	100	100	100	100	100	100	100	100	100	97	14
Do close to	100	100	100	100	91	91	97	97	100	97	100	15
Do like to live with	100	100	100	100	100	100	100	100	100	100	100	16
Do arrange wife for	74	94	97	3	71	9	0	0	0	0	0	17
												C n=23
Do give nets to	83	83	100	78	74	57	0	0	0	0	0	1
Do give clothes to	74	74	87	65	78	52	0	0	0	0	87	2
Can send anywhere	100	200	87	100	4	0	0	0	0	0	0	3

NOTE: Numbers are given in percentages, based on the number of yesses over the possible number.

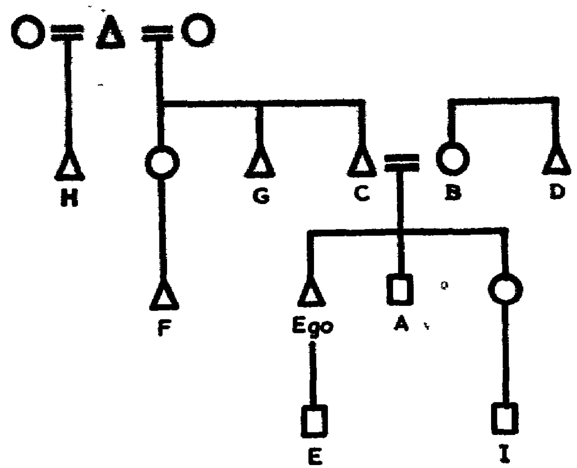


FIG 2

These same features also structure the incidence of behaviour, but in yet a different manner The fact that one's father's brothers, G and H, and one's mother's brother, D, actually share the feature or attribute of generation with one's father, C, directly causes one to expect that they can spank him, independently of the terminological category they fall into. Similarly, the fact that one's father's sister's son, F is (as is one's sibling) in one's own generation causes ego to expect that F *cannot* spank him, even though F is called 'father' and even though father has been found to be the focal relative for spanking

Thus, the behavioural categories are componential, and are only structured in each case by the immediately relevant attributes, while the terminological categories are extended from kernels, and always code information about all of the attributes (because of the shared set of kernels), regardless of which kind of variable is being stressed by the use of one or another extension pattern' (Kronenfeld 1973 1587–8)

[8] While it is not germane to the present article, I should point out that Southwold is creating an artificial problem by treating English translations as the significata of Ganda kin-terms (1971: 36, 37) Such a device unfairly builds in the English bias that he decries (p. 39), since anthropologists normally use a more fine-grained set of analytically derived categories to describe the designata, and from them to define the significata, of native kin-terms—even if that traditional notational scheme and set of categories have the kinds of problems that Romney (1965) and Kay (1968, 1969), among others, have pointed out.

The P, P̌ notational scheme (taken from Tarski) that Southwold next opts for (1971: 42) is inadequate to the task without some further development, as exemplified in Greenberg (1949), Atkins (n.d), and Kay (n d.)

[9] Southwold (1971 38) seems to mean several different things by his genealogical *v.* non-genealogical contrast He contrasts genealogical connections with membership in corporations linked by affinal alliances. If 'affinal alliances' were the supposed problem, then we would infer that he wants to equate 'genealogical' with 'consanguineal'— a position that he later (1971: 45) clearly and properly eschews If corporation membership is the problem, then the issue is still muddy, since these corporations normally recruit by birth, and since recruitment by 'conversion' would not belong to a genealogical paradigm according to whether it was conceptualised by the natives as adoption or as simple enlisting Corporation membership, *per se*, would not seem automatically to rule out most usual senses of 'kinship'.

The fact that a kin-term, say 'brother', is most often used for people who are not one's true brothers—and in fact is most often used in reference to people who are not even genealogically related to one—does not *by itself* imply that brother is not a genealogically defined term. This latter judgment depends on a theory of semantics and a theory of linguistic usage (Ardener 1971 [see especially: lxxix–lxxx] contains a good discussion of this kind of problem). 'Extension' does not represent an 'ingenious evasion' (Southwold 1971: 39), but instead represents one possible semantic theory. The fact that the extension theory works does not in itself prove

TABLE 5. P-series results.

				you serve	serves you	you found work for	found work for you	you taught to work	taught you to work	you given nets	given you nets	you can send anywhere	can send you anywhere	you move with	you close to	you help with cases	help you with cases
				Question Number													
Kin-types (all for male ego)			A	5	6	7	8	9	10	11	12	13	14	15	16	21	22
F			n=17*	100	82	29	88	18	95	29	71	41	100	82	100	94	100
FB			n=14*	100	86	14	50	7	86	29	36	50	100	79	100	93	100
MB			n=18*	94	72	16	57	28	78	28	28	39	100	89	100	94	94
FZS	e		n=12*	92	67	33	33	25	83	16	25	50	100	82	100	92	92
	y		n=2	100	100	50	50	50	100	0	0	50	100	100	100	100	100
MBS	e		n=3	100	100	33	0	0	67	0	0	100	100	100	100	100	100
	y		n=8*	75	88	38	37	75	50	12	12	100	63	100	100	100	63
B	e		n=8*	100	100	63	37	37	75	12	12	75	100	100	100	100	100
	y		n=8*	100	88	63	50	63	63	50	50	100	88	88	100	100	100
MZS			n=3	66	100	0	0	0	33	0	0	0	100	66	100	100	100
FBS			n=6*	66	66	16	50	16	50	33	16	50	50	50	84	100	50
Total	unskewed non-kernel	F	n=24*	100	79						21	38		87			100
Total	non-kernel	MB	n=22*	91	86			14				50		95			95
Total	FZS (skewed, non-kernel	F)	n=27*	85					59								78
Total	non-kernel in-lineage	B	n=21*				10										
Total	out-lineage	B	n=20*														
Total	MBS		n=28*														

Number given as a percentage.

TABLE 5 Continued

Kin-types (all for male ego)				you have helped with cases	has helped you with cases	you could marry daughter of	could marry your daughter	you can inherit	can inherit you	you are responsible for	you are responsible for the children of	is responsible for you	is responsible for your children	you have got a loan from	has got a loan from you	you have ever lived with
				Question Number												
			A	23	24	30	31	32	33	34	35	36	37	38	39	43
F			n=17*	76	100	0	0	29	29	94	88	100	100	53	35	100
FB			n=14*	64	79	7	0	21	21	79	79	93	93	29	29	93
MB			n=18*	72	78	100	6	100	89	94	94	94	94	39	22	65
FZS	e		n=12*	33	66	33	0	16	8	75	75	84	84	33	33	84
	y		n=2	100	100	0	0	0	0	100	100	100	100	50	50	100
MBS	e		n=3	16	0	0	0	0	33	100	100	100	100	0	0	100
	y		n=8*	100	50	12	12	12	12	88	88	88	88	38	62	38
B	e		n=8*	88	100	0	0	100	100	88	100	100	100	25	25	100
	y		n=8*	75	88	0	0	100	100	88	100	88	100	62	75	100
MZS			n=3	100	66	0	0	100	100	100	100	100	100	0	0	100
FBS			n=6*	50	0	0	0	0	0	84	84	67	67	33	50	84
Total	unskewed non-kernel	F	n=24*			8		13			56				16	79
Total	non-kernel	MB	n=22*			68		68	64		82					
Total	FZS (skewed non-kernel	F)	n=27*			41	19	7	4		70					56
Total	non-kernel	B	n=21*												48	
Total	out-lineage	B	n=20*	45			10									
Total	MBS		n=28*	50												

NOTES:

Numbers in the table refer to percentages of total responses for each kin-type which were positive in a given question. The n for each kin-type is given under the question series indices (A, B, C).

Asterisks indicate kin-type for which n seemed sufficiently large to talk about.

TABLE 5 Continued.

Kin-types (all for male ego)			B	1b you gave money regularly to	2b ever did	3 gives you money occasionally	4 ever did	7 you go for advice to	9 you have ever abused	10 you have ever punished with work	11 you have ever summonsed to authority	12 you have ever reported to an elder	13 you have ever advised	14 you have ever punched	15 you have ever spanked	16 you have ever pulled the ear of	17 you have ever warned	18 you have ever shouted at	19 you have ever caned	20 you have ever knocked	21 you have ever hit
F			n=17*	65	88	82	52	100	0	6	12	6	65	0	0	0	6	6	0	0	0
FB			n=14*	14	50	93	64	100	7	16	0	7	50	0	0	0	0	7	0	0	0
MB			n=18*	12	72	89	72	100	6	33	11	11	72	0	0	0	11	16	0	0	0
FZS	e		n=12*	8	50	100	75	92	8	50	16	16	67	0	0	0	8	25	0	0	0
	y		n=2	0	0	100	100	100	50	100	0	0	100	0	0	0	50	50	0	0	0
MBS	e		n=3	0	0	100	100	100	0	100	0	0	100	0	0	0	0	0	0	0	0
	y		n=8*	12	25	100	88	50	37	88	25	25	100	0	25	12	37	37	25	25	25
B	e		n=8*	25	62	100	100	100	12	62	50	25	100	0	0	0	0	37	0	0	0
	y		n=7*	43	86	100	100	100	86	100	43	29	100	29	57	29	71	71	57	43	57
MZS			n=3	0	33	100	100	100	33	66	33	0	100	0	0	0	0	0	0	0	0
FBS			n=5*	0	40	100	100	60	60	80	0	40	80	20	60	0	20	20	40	20	40
Total	unskewed	F	n=24*	8													0	4			
Total	non-kernel	MB	n=22*				55			14		5	50				14	9			
Total	FZS (skewed, non-kernel	F)	n=25*														8	20			
Total	non-kernel	B	n=19*																		
Total	out-lineage	B																			
Total	MBS																				

Question Number

that extension is the correct theory (cf. Hawkes in prep.), but evidence concerning native definitions (cf. Kronenfeld n.d. *a*) does seem to contribute to such a conclusion. I have elsewhere (Kronenfeld 1973) shown how it would follow from Marking Theory (Greenberg 1966) that a set of kin-terms would be most frequently *used* in a non-genealogically based manner, even though these terms were primarily defined genealogically. The Fanti situation that I treat may well not apply to Southwold's Ganda, but his observation that kin-terms are most frequently applied to non-kinsmen does not in any way prove such a lack of application. Since I have shown that genealogical definitions, by at least one reasonable semantic theory, are compatible with such frequency of use data.

Given that he defines as 'congeniate' 'those social relations in which appropriate behavior is characteristically perceived as similar to that expected in familial relationships,' (1971: 49), and given that he later (1971: 52) says that the Ganda terms 'in their clan-type application ... do have a kin-sense,' I find his denial of extension (p. 39) or of a kinship based kernel meaning of Ganda terms so used (pp 50–53) hard to understand. Certainly a set of words can easily be extended beyond the primary range of their meaning without clouding the primacy of that primary range–as Basso (1967) has nicely shown concerning the extension of the paradigm of parts of a horse to automobiles by Western Apache.

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LÉVI-STRAUSS ON SHAMANISM

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Lévi-Strauss's discussion of the story of Quesalid and his analysis of the effectiveness of shamanism are examined His suggestions in terms of social consensus (once sorted out) and psychological coherence (in so far as suffering is suffering from unintelligibility) are found valuable, and extended to psychoanalysis But his physiological correspondence account of the effectiveness of symbols fails. The underlying physiology is a fantasy physiology, and in any case merely pointing to parallels or correspondences would not constitute an explanation. Here a general suspicion of structuralist explanation is realised.

What is the role of thoughts or beliefs in Western and non-Western therapies? Why should thoughts have any role? What sort of thoughts must they be? Does it matter whether they are conscious or unconscious, fantasies or memories, true or false . . .? What is the connexion between the aetiology of a disorder and the importance of the meanings or analyses provided in interpretative therapies?

Western therapies for psychological disorders can be ranged along a spectrum in accordance with the role they assign to thoughts. At the extreme ends can be placed drug and shock therapies, where no thoughts are involved, and Freudian psychoanalysis, where the patient's understanding of his suffering seems essential to 'cure'. In this context, shamanism is especially interesting because it seems to involve thoughts, but thoughts and a theory which need not be true. Is mere belief or a special form of faith enough? If so, why? And when? Lévi-Strauss, in his two essays, 'The sorcerer and his magic' (1963*a*: 167–85) and 'The effectiveness of symbols' (1963*b*: 186–205), makes a number of suggestions. In the first essay, he suggests that what is essential to the effectiveness of shamanism is the provision of a theory or conceptual scheme that enables the patient to reintegrate an otherwise alien experience. It does not matter that the understanding provided is 'mythical' or 'symbolic'. The shaman's theory need not be (literally) true, perhaps because his society accepts it, and in any case so long as the patient believes it and it is significant to him. When it comes to 'The effectiveness of symbols', however, it seems that Lévi-Strauss would insist on at least 'structural' correspondences, over and above coherence. It is these suggestions that I here wish to examine.

I

Lévi-Strauss tells the story of Quesalid. It is a fascinating and instructive story. Quesalid was an unbeliever in shamanistic powers, so he undertook training as a shaman in order to refute their claims. But, much to his amazement, his use of the bizarre procedures (especially the technique of extracting a concealed tuft of bloodied down from his own mouth and claiming he had sucked out the pathological foreign body in the form of a bloody worm—p. 175) produced cures. And he became the most powerful shaman of all.

Now the initial undertaking, in so far as it was an effort to refute the shamans, is

itself odd. What would Quesalid's failures contribute that a collection of the failures of established shamans would not? Indeed, on the face of it, such failures promise far less. A man who set out to refute astrology would gain nothing by himself mastering the techniques of astrology. It could be claimed that his failures counted only against him, not against the techniques. Similarly, if Quesalid had failed to cure, it would simply have shown that he had not mastered the techniques, that he was an inadequate shaman (not that shamanism was inadequate). This mode of defence would inevitably be open, and perhaps even more open for shamanism than astrology. Fellow astrologers might be sufficiently scientific in spirit to try to replicate the critic's procedures with greater precision and success. The principles might be sufficiently objective, so that their application by the believers yielded the same results: false predictions. The shamans have an extra out. It may be one of the principles of the practice, that only a believer in the principles can effectively use them. Or if not a 'believer', someone infused with the proper 'spirit', one with a 'calling'. Quesalid's failure would only reveal him as a failed shaman, not the failure of shamanism. Even if other shamans failed to succeed where he failed, they would have whatever outs would normally be available.

There is a problem about what distinguishes a routine failure from an 'anomaly', i.e., a failure arising in the context of a challenge or test of their powers. (What makes something a problem in what Kuhn calls 'normal science' rather than an anomaly leading to crisis and breakdown, and so revolution?) Two things to notice: nothing is added to the confirmation or falsification situation by Quesalid's becoming a shaman, or an unbeliever becoming an astrologer. The same difficulties arise for the theory from the failures of professionals. Second, if the usual outs used to explain failures seem *ad hoc* (and the juggling seems more *ad hoc* should Quesalid's failures receive special explanations: he lacks the spirit, etc.), one still has to explain what makes a change or adjustment in theory seem *ad hoc* rather than a natural development or articulation or elaboration. Every theory has to accommodate awkward facts But sometimes the accommodations become more awkward than the facts. When there are too many epicycles and an alternative theory is available, a shift is possible. What exactly, however, makes a theoretical claim or shift in theory a 'simplification' rather than '*ad hoc*' raises many of the deepest questions in the philosophy of science (and some of the hardest disputes within scientific argument itself) I will leave these issues here. Quesalid did not manage a successful refutation of the shamans through failure His was a different success, and raises different issues.

We should note that Quesalid's undertaking is not so odd if it was to *understand* rather than to *refute* the shamans. Indeed, many psychoanalysts claim that critics cannot understand the character of psychoanalytic claims or the nature of the evidence for them without themselves being psychoanalysed It is not, however, necessary that critics themselves become psychoanalysts. But Quesalid was dealing with a practice that depends on concealment: to appreciate the character of magic it may well be necessary to practise it rather than simply experience its effects.

II

Lévi-Strauss tells the story of Quesalid mainly in order to emphasise the importance of belief by the community or group: cure by magic is a 'consensual' phenomenon (1963*a*: 169). There are a number of separable claims which may get

confused under this description. The point here is different from that made by saying that what counts as a 'symptom' is a matter of the view of the society. Though that is (largely) true, once the character of sickness and health is fixed within a society, the transition from one to the other (cure) may not be achievable by change in public opinion (or anyone's beliefs). There are limits to the powers of consensus: a myth (of converging rivers, etc.) will not mend a broken bone, and faith will not turn an aspirin into a birth control pill. The point Lévi-Strauss wishes to make he puts as follows: 'Quesalid did not become a great shaman because he cured his patients; he cured his patients because he had become a great shaman' (1963*a*: 180). Here, too, distinctions are necessary. We must distinguish group consensus in relation to the *status* of a sorcerer and in relation to the *power* of a sorcerer. Community belief may provide the criterion for a social role—this is a logical or conceptual connexion. On the other hand, community belief may produce the power associated with a social role—this may be logical, but more interestingly may be an empirical connexion. That social role and power can be separated is shown, in the cases of doctors and patients, by the fact that people may be able actually to heal or suffer independently of the opinion of their society. Confirmation by the community does, however, help determine who we are, whether doctor, shaman, or patient. In so far as this is a matter of social role, group consensus does indeed, meaning logically, determine who we are. This seems to be what is happening in the case of the Zuni boy who appears to become a witch in the course of his witchcraft trial (as his accusers and he too come to believe he is one). If the relevant community believes it, it is true. Their belief is part of what constitutes its being true. Who counts as a shaman, a doctor, a patient depends (in this way) on group consensus. But group consensus may also operate causally. It might be called the 'Genet Effect'. Told at the age of ten that he was 'a thief', Genet (1949) conceived himself in that way and decided he must become one, and did He realised the label in his life. (Cf. the development of 'patienthood' as an identity, e.g., in Dora. See Erickson 1964: 173.) In the case of shamanism and healing, the mechanisms may be more devious The community's belief in the shaman's powers may help produce those powers. The ways in which this comes about may be quite various and complex. Before I proceed to look at some of those ways, let me try in another way to bring out the contrast I have been trying to make.

Consider the case of a 'king'. All believe x is the king and therefore he *is* (in a sense) the king. But because there are some conditions which hold whether or not a community believes them to hold, and which a community cannot make hold by believing, the case of the 'shaman' is importantly different from that of the king. If all accept x as a shaman he may therefore be a shaman, have the social role and have the social powers of the position. But unlike the king, all of whose powers are social, he may still lack the power to heal (though belief in that power may be what gave him community consensus to the role in first place). That is, the doctor or shaman may not be the man who can heal. He is the man the community believes can heal. The title comes from belief in the power. The power, however, need not be a necessary condition of the belief, indeed, it may not exist prior to the belief. And it is possible that, in some cases, it arises as a result of the belief. If everyone believes 'x is a king', he may have the power to rule *in virtue* of that belief. If everyone believes 'x is a shaman', he may still lack the power to heal. The interes-

ting question is whether the acceptance can produce the power (where this is an empirical connexion) and how.

Lévi-Strauss cites Cannon on the physiology of voodoo; and one can understand how the withdrawal of the community, the isolation produced by a voodoo curse in virtue of the community's accepting it, might lead to physical collapse. (Consider models for self-fulfilling prophecies.) Freud himself recognises the importance of acceptance by the community. (Consider the importance of the title 'Professor' in Vienna.) Such recognition increases the 'authority' of the physician, and may contribute to the effectiveness of his technique. But it is not by itself sufficient explanation of effectiveness. As late as 1910 Freud could write

> Hitherto, this authority, with its enormous weight of suggestion, has been against us All our therapeutic successes have been achieved in the face of this suggestion: it is surprising that any successes at all could be gained in such circumstances.... I can only say that when I assured my patients that I knew how to relieve them permanently of their sufferings they looked round my modest abode, reflected on my lack of fame and title, and regarded me like the possessor of an infallible system at a gambling-resort, of whom people say that if he could do what he professes he would look very different himself. Nor was it really pleasant to carry out a psychical operation while the colleagues whose duty it should have been to assist took pleasure in spitting into the field of operation, and while at the first signs of blood or restlessness in the patient his relatives began threatening the operating surgeon. ... Social suggestion is at present favourable to treating nervous patients by hydropathy, dieting and electrotherapy, but that does not enable such measures to get the better of neuroses. Time will show whether psycho-analytic treatment can accomplish more (Freud 1910: 146–7).

Certainly co-operation from surrounding family and community is helpful and perhaps even necessary. Their belief, faith, recognition, etc., may yield such co-operation. But perhaps the essential element is the *patient's* acceptance of the doctor's authority. And belief by the community, consensus, may contribute to producing belief in the individual patient. But perhaps such belief can arise independently and still be effective. [Quesalid himself attributed his first success to the patient's faith. The treatment worked 'because he [the sick person] believed strongly in his dream about me' (Lévi-Strauss 1963*a*: 176). Freud says that the patient need not enter treatment with any initial belief (1913: 126). If it is necessary, it can develop in the course of treatment.] The question now becomes the relation of belief by the patient to the effectiveness of the therapy.

Lévi-Strauss, aside from emphasising the consensual elements (without distinguishing the logical and causal) in shamanism, goes on to speculate about the mechanisms by which shamanistic ritual and magical symbols produce cures. And he extends his account of the effectiveness of symbols to psychoanalysis. This account requires that one pay attention to the content of the rituals and symbols (not just to their acceptance by the community).

III

There is ground for general suspicion of structuralist explanations: does being told that one thing we do not understand is similar in structure to another thing we do not understand help us understand either? The nature of the 'similarities' must be specified. We must look to the details of each such explanation offered to see what, if any, enlightenment can be found. Lévi-Strauss offers parallels between the shaman and the psychoanalyst, and between the theories of both and physiological theory.

Quesalid effects cures by his technique of the bloody down despite the fact that he knows it to be bogus: it is not connected with any known or believed aetiology ('sickness' is not really captured by bloodying a concealed tuft and Quesalid does not think that it is). But it works and even enables Quesalid to expose 'imposters' (p. 178). His success reveals that the therapist need not believe in order for the therapy to work. We should explore (though we can only begin to, here) the natural implication of this: if belief by the practitioner is not necessary, then perhaps truth is not necessary either. Which is not to say that the *content* of rituals and symbols and interpretations is irrelevant—even if truth as literal correspondence to an objective reality is.

Lévi-Strauss, in his discussion of shamanistic therapies in non-Western cultures, suggests that they work because they provide a way of understanding problems and the world. The patient is given a theory, a set of terms and relationships, that enable him to fit his experience into an intelligible order.

> The system is valid precisely to the extent that it allows the coalescence or precipitation of these diffuse states, whose discontinuity also makes them painful (p. 182). The song seems to have as its principal aim the description of these pains to the sick woman and the naming of them, that is, their presentation to her in a form accessible to conscious or unconscious thought (p 195) That the mythology of the shaman does not correspond to an objective reality does not matter. The sick woman believes in the myth and belongs to a society which believes in it. The tutelary spirits and malevolent spirits, the supernatural monsters and magical animals, are all part of a coherent system on which the native conception of the universe is founded. The sick woman accepts these mythical beings or, more accurately, she has never questioned their existence What she does not accept are the incoherent and arbitrary pains, which are an alien element in her system but which the shaman, calling upon myth, will re-integrate within a whole where everything is meaningful (p. 197).

For Lévi-Strauss the beliefs need not be true, because in so far as the problem is *not understanding*, *any* coherent story or theory will solve that problem (though, of course, it must not fail to correspond at key points with known realities) and so end the suffering which is a *suffering from unintelligibility*. Psychoanalysis may be the new mythology of our culture (p. 183) I think Lévi-Strauss's point sheds a certain light. Coherence matters because patients are partly suffering from incoherence, the alien unintelligibility of the experience. I think, however, that a Spinozistic view of emotions and the mental would contribute much to a clearer understanding of the mechanisms, of how therapies which operate through beliefs can transform mental states. Rather than treating emotions as blind sensations or physiological responses, the Spinozist view emphasises the importance of thought (of cognitive components) in the classification and discrimination of emotional states, and the consequent importance of reflexive knowledge in changing those states. If we follow Spinoza and accept that emotions essentially involve beliefs, we can begin to see how changing beliefs can transform emotions. In the realm of the mental, understanding of the state becomes part of the state, because it is identified or specified through the associated beliefs. Where knowledge is self-reflexive, knowing can transform the thing known. Following this line, we would begin to see why insight is important, and we might come to see why insight is not enough. But Lévi-Strauss develops his theory in a different direction.

Lévi-Strauss thinks symbols are effective, not because they are literally true and not because they change beliefs and so transform the associated mental state, but because they correspond with an underlying reality. (Consider Baudelaire on

'*correspondances*' and Eliot on 'objective correlatives'.) The myth and the true account are similar in 'structure'. He speculates that the parallel in the case of a mythical incantation sung by the Cuna shaman to ease difficult childbirth is an underlying *physiological* reality. The myth is about a quest for the lost soul of the mother, a myth of passage through and over obstacles, and its elements correspond to or represent the vagina and uterus of the pregnant woman (p. 188). He goes on to speculate that psychoanalysis too hooks onto the same physiological reality:

> Given this hypothesis or any other of the same type, the shamanistic cure and the psychoanalytic cure would become strictly parallel. It would be a matter, either way, of stimulating an organic transformation which would consist essentially in a structural reorganization, by inducing the patient intensively to live out a myth—either received or created by him—whose structure would be, at the unconscious level, analogous to the structure whose genesis is sought on the organic level. The effectiveness of symbols would consist precisely in this 'inductive property', by which formally homologous structures, built out of different materials at different levels of life—organic processes, unconscious mind, rational thought—are related to one another. Poetic metaphor provides a familiar example of this inductive process, but as a rule it does not transcend the unconscious level. Thus we note the significance of Rimbaud's intuition that metaphor can change the world (p. 201).

But this pays too little attention to the elements on either side of the parallel, and to the nature of 'induction' and of what he calls 'reliving' or 'abreaction'. Again I would look to Spinoza for a better understanding of the power of metaphor, at least where the world changed is the mental world But Lévi-Strauss means to explain how the myth can produce physical, organic, changes or cures.

He mentions that 'Freud seems to have suggested . . . that the description in psychological terms of the structure of psychoses and neuroses must one day be replaced by physiological, or even biochemical concepts' (p. 201). This is true, but as the *Project for a scientific psychology* shows, the sort of reduction Freud had in mind was neuro-physiological. (To be fair, the *Project* was published a year after the original publication of Lévi-Strauss's essay) And neurophysiology is rather different from gross physiology: our mental states may be embodied, but they are not precisely 'paralleled' on a gross level Indeed, in the specific case of hysteria, no reduction to the physiological is possible. As Freud insists. 'Hysteria behaves as though anatomy did not exist or as though it had no knowledge of it' (Freud 1893: 169) And Lévi-Strauss does not notice the difference between 'reduction' and 'replacement'. The sort of *reduction* Freud envisaged did involve precise parallels, preserving psychoanalytic laws in different terms.[1] But the sort of biochemical basis Lévi-Strauss mentions (chemical basis for psychosis) would not provide parallels for psychoanalytic claims. Instead of reducing psychoanalytic theory, it would *replace* it. Indeed, it would leave the role of thoughts, their relevance, obscure. Why and in what way should ideas or thoughts provide a handle onto chemical forces? The term 'induction' covers the obscurity without clarifying it. How does 'inducing the patient intensively to live out a myth' help? Lévi-Strauss calls this 'abreaction' (1963*b*: 182 sqq, 199), but this living or reliving is not the same as the early psychoanalytic notion (which involved 'energy discharge' and hence relief). In relation to the shaman's bloody worm rituals, etc., Lévi-Strauss actually speaks of the sorcerer abreacting *for* the silent patient (p. 183), which is without sense in psychoanalytic terms. And again, he does not explain why symbolic thoughts should provide a

lever for producing physiological changes, except that the thoughts run 'parallel' to the physiology. But do they? And if they did, would that *explain* anything?

The difficulty may be brought out more clearly if we notice something Lévi-Strauss seems to notice but then neglects. He acknowledges at one point that the complicated itinerary of the Cuna song 'is a true mythical anatomy, corresponding less to the real structure of the genital organs than to a kind of emotional geography, identifying each point of resistance and each thrust' (p. 195). What he fails to emphasise is that this means that what is paralleled is not actual anatomy, but, as with hysteria, a fantasy anatomy I am in fact inclined to think that the therapy will be effective only if the difficulties are hysterical, i e., ideogenic (Lévi-Strauss says the shaman cures 'true organic disorders'—p. 199). But the deeper problem is that structuralist explanation will here, in any case, fail to explain. The general suspicion is here specifically realised: one process we do not understand is said to be similar to another process we do not understand, but this does not help us understand either of them. We cannot understand the process of psychoanalysis and the process of shamanism in terms of a third, underlying, process which they both parallel. We might begin *if* there were a third underlying physiological process common to them, but they each parallel fantasy physiologies, and there is no reason to expect that fantasy physiologies (unlike actual human physiology) will be the same for different cultures and individuals. Even if there were parallels to a common anatomy, how would this *explain* the effectiveness of either shamanism or psychoanalysis? Lévi-Strauss thinks these psychological treatments have direct physiological consequences, but does not explain how. He says, 'In our view, the song constitutes a *psychological manipulation* of the sick organ, and it is precisely from this manipulation that a cure is expected' (p. 192). Why should telling a story, even if parallel with actual anatomy, 'work', e g , make obstructions disappear? We can see how giving unintelligible physiological processes meaning may ease our psychological pain, but why should it alter the physiological processes themselves? The rhythm of the song may help (as a lullaby helps put a child to sleep) but this would not explain the relevance of the content of the song, of the meanings of the words or symbols.

Lévi-Strauss's account is inadequate. Mere parallels do not explain why changes in one branch should bring about changes in the other. Even if there were the claimed parallels, why should there be a necessity to *preserve* the parallel (through bodily changes as the song progresses)? His account should be restricted to shamanistic cures of hysterical disorders rather than of physical disorders in general. Indeed, the fact that a disorder was curable by shamanistic story-telling, etc., might be part of what shows it to be 'hysterical' (along with the absence of organic causes, dissimilarities in pattern of disorder, . . .) Non-hysterical disorders may lack even fantasy parallels (and hence lack symbolic meaning). Shamanistic

cures are of three types, which are not, however, mutually exclusive. The sick organ or member may be physically involved, through a manipulation or suction which aims at extracting the cause of the illness—usually a thorn, crystal, or feather made to appear at the opportune moment, as in tropical America, Australia, and Alaska. Curing may also revolve, as among the Araucanians, around a sham battle, waged in the hut and then outdoors, against harmful spirits Or, as among the Navaho, the officiant may recite incantations and prescribe actions (such as placing the sick person on different parts of a painting traced on the ground with coloured sands and pollens) which bear *no direct relationship* to the specific disturbance to be

cured. In all these cases, the therapeutic method (which as we know is often effective) is difficult to interpret. When it deals directly with the unhealthy organ, it is too grossly concrete (generally, pure deceit) to be granted intrinsic value. And when it consists in the repetition of often highly abstract ritual, it is difficult for us to understand its direct bearing on the illness. It would be convenient to dismiss these difficulties by declaring that we are dealing with psychological cures But this term will remain meaningless unless we can explain how specific psychological representations are invoked to combat equally specific physiological disturbances (p. 191).

But Lévi-Strauss's attempt to generalise from the text he considers fails. The fantasy anatomy that a psychoanalyst may confront may be different from the fantasy anatomy a myth deals with. So psychoanalysis is not parallel to shamanism (in this respect) for they are not parallel to the same third thing (actual anatomy). And many shamanistic cures are without 'correspondences' or physiological parallels at all So Lévi-Strauss's account cannot be extended to cures in general. Even the particular symbolic treatment he discusses lacks real physiological parallel (and even if it had it, the mechanism of change would not be clarified by merely pointing to the parallel) How relation to fantasy anatomy allows effects on actual anatomy depends, I think, on the original disturbances in anatomy being hysterical (i.e., also depending on fantasies of anatomy—though perhaps different ones). The importance of thought in therapy should be clarified by the importance of thoughts in aetiology. Psychoanalysis tries to explain ideogenesis Lévi-Strauss seems to deny or ignore it.

To summarise. Lévi-Strauss makes suggestions in terms of (social) consensus, (psychological) coherence, and (physiological) correspondence to explain the effectiveness of shamanistic symbols and magic, and extends his account to psychoanalytic treatment. Properly sorted out, I think the first two suggestions contain valuable insight, but that the third fails both for the particular case and in general.

NOTES

The substance of this article was originally included in a larger paper on 'Thought, theory, and therapy' which was presented to a seminar on 'Nosology' at the San Francisco Psychoanalytic Institute (March, 1973) and a symposium on 'Images of body states' at the American Anthropological Association meeting (November, 1973)

1 Whether such parallels exist we still do not know. And it is not clear, even if such parallels exist, whether 'reductions' are possible There is an enormous philosophical literature on the question of whether even 'sensations' could be 'brain processes' The importance of a distinction between 'reduction' and 'replacement' was first impressed on me by Alasdair MacIntyre

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AGNATES AND OUTSIDERS: ADOPTION IN A CHINESE LINEAGE

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How does a member of a powerful Chinese lineage choose an heir if he is unable to produce his own? According to orthodox rules, he must first exhaust all possibilities of finding a suitable heir within his lineage before he selects an outsider. Yet a significant number of adopters in the lineage village under consideration ignored the rules and decided to purchase a male infant from complete strangers rather than to adopt an agnate. In doing so, the adopting fathers had to submit to a humiliating initiation ceremony and pay for an extremely expensive banquet. In contrast, agnatic adoption is relatively inexpensive and painless. The following article examines a number of adoption cases and concludes that segmentary rivalries within the lineage are the root cause of this unusual pattern of outsider selection. Supporting evidence is presented from the written genealogy of the lineage involved, as well as from first-hand field data.

I

Prior to the Communist Revolution in 1949, segmentary patrilineages played a dominant role in the power circles of southeastern China. Lineages owned much of the agricultural land in this region and controlled some of the more lucrative aspects of market town commerce. Under the circumstances, it is not surprising that these kinship organisations became primary targets during the land reform campaigns of the early 1950's and the class struggles that followed. The weight of evidence now suggests that the lineage has been eliminated as a significant focus of social activity within the People's Republic. However, functioning lineages can still be found in the rural areas of Hong Kong, a British Crown Colony on the southern coast of China. This article is based on seventeen months of field research (1969–70) in one of Hong Kong's most traditional lineage communities.

Viewed from the outside, the Chinese lineage resembles a homogeneous unit with few important structural variations. On closer inspection, the same organisation might appear to be suffering from internal conflict which threatens its very corporate nature. Although neither view is entirely correct, the larger lineages are invariably divided into rival segments that compete for local power and wealth. A good deal is known about the Chinese lineage (see e.g., Ahern 1973; Baker 1968; Freedman 1958; 1966; Pasternak 1972; Potter 1968), but it is not always clear under what circumstances a member will allow segmentary rivalries to interfere with his allegiance to the lineage as a whole. This problem is examined in the following discussion of adoption in one lineage. I intend to demonstrate how the selection of male heirs by those who are unable to produce their own reveals a basic conflict between the need for agnatic solidarity and the realities of segmentary opposition. Every adopter has to make a choice from a limited set of alternatives, some of which benefit the individual and others the lineage. Before proceeding with the

main argument, however, it might be appropriate to present some background information on Chinese adoption in general.

II

Adoption in Chinese society

'Three things are unfilial, and the worst is to have no sons.' This popular aphorism is attributed to the sage Mencius who lived during the fourth century B C. It summarises one of the most widely held values in traditional Chinese society, namely the absolute necessity of producing male heirs (see De Groot 1908: 107). In the traditional view, it is the duty of every son to provide his own father (and father's father, etc.) with grandsons to carry on the patriline and to worship the ancestral spirits. A person who dies without heirs is doomed to everlasting misery because no one survives to make the appropriate offerings of food and other goods that nourish the spirits in the afterlife. Unless some provision is made for these unfortunates, they become 'hungry ghosts' that prey upon the offerings of others (Hsu 1948: 76; Jordan 1972: 34).

Spiritual considerations of this nature are certainly important but there are also a number of material factors which motivate sonless individuals to seek an heir According to customary practice, the property of a man who dies intestate automatically passes to his nearest agnatic kinsmen Most people insist on designating their own successor (who may or may not be a close agnate) in the belief that this gives them more control over the eventual disposition of their property. Furthermore, as outlined later in this article, many Chinese adopt a son specifically to provide companionship and financial support in old age.

The decision to adopt is usually postponed until all possibilities of producing one's own heir have been exhausted For instance, if the first wife is suspected of infertility the husband might take a second or even a third wife If our subject has proven his own capabilities by producing daughters, he may be encouraged to try his luck with concubines. There are limits to these experiments, however, because the additional women ordinarily require support throughout the rest of their lives Economically it is often more practical to arrange an uxorilocal marriage between one's own daughter and a poverty-stricken man who is willing to change his surname, or allow some of his sons to take their grandfather's surname (see M. Wolf 1972 192–199) In this way an heirless individual who is at least fortunate enough to have daughters can 'produce' his own grandsons. Despite its convenience, the uxorilocal arrangement is commonly regarded as the most dangerous method of securing an heir. The son-in-law may threaten to take his children and leave the family at any time, especially if he feels entitled to a larger share of the inheritance. A father who chooses this option can easily find himself at the mercy of an unscrupulous outsider

It is not surprising, therefore, that most Chinese prefer to adopt their own heirs directly from another family. Ideally, an adopter is expected to choose from among his closest collateral agnates in the descending generation (see discussion below). Adoption between agnates in China is known as *kuo chi*, which literally means 'to cross and continue' (Nelson 1969: 118). The terminology reflects the view that the adopted son crosses over into a new position in the patriline to continue a segment or line that would otherwise die out (see also A. Wolf 1974: 141–2).

During the Imperial era (pre-1911), agnatic adoption was encouraged by the state laws regarding family succession and inheritance. Children of other surnames were not to be adopted as sole heirs until all agnatic possibilities were exhausted. However, in most parts of China these legal restrictions were never enforced with great vigour by the government; and, as a consequence, non-agnatic forms of adoption were also common (Freedman 1958: 28 n2). In fact, many adopters found it advantageous to buy an infant son from a total stranger rather than to adopt an agnate. For reasons that I hope to make clear, this is particularly true for people who were members of powerful lineages.

III

The setting: a single-lineage village

The data for this study were gathered in San Tin, one of the largest 'single-lineage villages' in Hong Kong's New Territories. In 1970, San Tin had a population of approximately 4,000, all members of the Cantonese-speaking *pen-ti* ('native') ethnic group which dominates this part of China. Except for a handful of shopkeepers, every male in the village bears the surname *Man* (Mandarin *Wen*) and traces direct lines of descent from a common founding ancestor who settled in the Hong Kong region during the early fourteenth century. The Man lineage grew by a process of segmentary expansion until it controlled a large section of land near the mouth of the Sham Chun River. This river now forms the border between British Hong Kong and the People's Republic of China

San Tin has been under colonial rule since 1899 when the British government took possession of the New Territories. The occupied land (which was obtained on a 99-year lease) currently serves as an agricultural hinterland for the cities of Kowloon and Victoria. Although the New Territories have undergone considerable change in recent decades, five original lineages still manage to dominate the region (see Baker 1966). One of these is the Man lineage at San Tin.

Now in their 27th generation, the San Tin Mans are divided into seven major segments and dozens of minor segments. Each 'segment' is defined as a land-owning corporation comprising the male descendants of a specific ancestor who was wealthy enough to leave an estate Alternatively a group of agnates might buy land and set it aside as an estate to honour their father or grandfather, who then becomes the apical ancestor of an incipient segment. The profits from these estates pay for the annual worship of the founding ancestors and subsidise the social activities of the segments involved If a surplus exists, the money is usually divided among the surviving members. Estate dividends in some lineages are large enough to constitute an independent income for the members of certain segments (see e.g , Potter 1968: 110–12)

The Man lineage has shown remarkable resilience over the centuries. It still retains many of its traditional functions, including the management of commonly-owned property, maintenance of five ancestral halls and support of the village school. The lineage continues to be the primary reference group for most people in San Tin no matter how much experience they have had in the outside world.

The conflict between the need for lineage-wide solidarity and the realities of segmentary opposition is a part of everyday life in San Tin. Historically, three

dominant segments were fierce rivals for control of the village. The oral tradition of the lineage abounds with stories of cross-segment fights for control of the village market and other sources of income. According to a local maxim, 'the rich segments (*fang*) exploit the poor, and the powerful exploit the weak.' Even though these segmentary distinctions have become less important in recent years under the influence of massive emigration and other changes,[1] the Mans are still conscious of the hierarchy of segments within the lineage.

At the same time, however, all the Mans are acutely aware that their dominant status in the surrounding countryside depends upon the power and prestige of the lineage as a whole. Intra-lineage rivalries are not very apparent when the Mans deal with the outside world. Before the British occupation, the lineage controlled a large section of the northern New Territories region. This gave the Mans economic and social advantages that ordinary peasants did not enjoy. For instance, whenever a San Tin resident needed to contact a government bureaucrat or a town merchant he could rely upon his lineage leaders to act as intermediaries. The Mans also maintained their own militia for protection against bandits and rival lineages. Even today the Mans feel superior to the people living in small, neighbouring multi-lineage villages and they expect to be treated with deference when they visit the nearby market towns. The lineage is thus an integral part of every member's personal identity.

In exchange for the benefits of lineage membership, individuals are expected to observe a set of rules which are designed to protect the interests of the group. Some of these rules can be traced to the Confucian classics but most are quite parochial and tend to elevate the lineage to a position of importance over everything else in the society. Written rules are generally recorded in the genealogies kept by powerful Chinese lineages (see Liu 1959), while unwritten rules are part of the oral tradition in lineage communities. Any transgression of these accepted modes of behaviour can be interpreted by the membership as a threat to the prestige of the entire group. The rules governing adoption among the Mans are a reflection of the concern for maintaining the lineage as a powerful and privileged elite. And, as I shall demonstrate, the same rules also help protect the corporate wealth of the various segments within the lineage.

IV

Principles of adoption

Adoption rules in San Tin (most of which are unwritten) encourage a sonless individual to select an heir from the most immediate agnatic group whenever possible. Brothers are expected to share their sons with less fortunate siblings. If there are no extra heirs among brothers, the adopting father is encouraged to look within the next highest agnatic group, which consists of the descendants of his grandfather. When these possibilities are exhausted, adoption is encouraged within ever higher agnatic groups until all options are eliminated and the searcher reaches the kinship boundaries of his immediate localised lineage (i.e., the lineage in his own community).

The rules of preferred selection do not stop even here because adoption is then encouraged within the non-localised 'higher-order-lineage'. Members of this

kinship unit share a common founding ancestor but live in separate communities often dispersed over a wide area (Freedman 1966: 21–3). Beyond these wider lineage ties, the hardpressed searcher is urged to select an heir from a family with the Man surname (i.e., members of the same *clan* which may not be based on actual lines of descent). The selection process has reached the boundaries of recognised agnatic kinship at this point and the adopting father must then procure a son from an outside family.

The rationale behind these rules is not hard to find. As noted earlier, lineage segments are property-owning corporations and it is natural that segment members should seek to restrict access to these sources of wealth by encouraging adoption within the pre-existing group. Each time an adopting father brings in an heir from another segment (or another lineage), one more beneficiary is added to the estate of the apical ancestor (cf. Fei 1939: 70; Liu 1959: 73 sqq.).

V

Within the lineage: adoption of agnates

The 'crossing' of sons between brothers is the most common form of adoption in San Tin. Lineage rules stress that the oldest brother in any set of siblings has rights over the sons of his younger brothers because, as the first-born, he is primarily responsible for maintaining the patriline.[2] For instance, when there are two brothers, the older can claim the only son of his younger brother and raise the boy as his own.[3] If the younger brother has several sons, the heirless older brother is permitted to choose the one he most desires. Younger brothers also have certain rights to the excess sons of their male siblings, but only after the oldest brother is reasonably certain that he will have an heir capable of reproduction.

The position of the adopted son is somewhat ambiguous, especially if he is the sole surviving heir of both brothers. When this happens, the son inherits from two lines and is expected to 'light the lamps of both houses,' meaning that he is responsible for the ancestral rites commemorating his biological and his adoptive father.

Beyond the minimal agnatic unit of brothers (i.e., descendants-of-same-father), the rights of a sonless searcher to claim an heir are not clearly defined and depend upon his personal wealth and status. Ideally, of course, the members of his most immediate agnatic group should help, but no one wants to relinquish a younger son to someone who offers a bleak future for the boy. Wealthier searchers, however, have little difficulty finding a suitable agnatic heir, *if* they choose to do so.

There is a practical reason for selecting a son from a close agnate in San Tin because adoption becomes increasingly more difficult and more expensive the further one 'ascends' the patriline in search of an heir. Adoption beyond the level of brothers has to be legitimised by an expensive, nine-course banquet for lineage members who belong to the same minimal agnatic unit as the adopting father and the biological father. This unit is easily computed by tracing the lines of descent for both men up through the lineage until a common ancestor is reached. Every male elder (*fu lao*, age 61 and over) and leader (estate trustee, village guardsman, etc.) who descends from this ancestor must be invited to the banquet. During the

festivities, the elders are asked to place their signatures on a red cloth banner certifying that they recognise the adoption as legitimate.[4] The adopting father has to pay a fee for the signatures in some cases.

The certification is an attempt to preclude conflicting claims on the adopted son later in life, and all who sign the banner are expected to accept the boy as an authentic heir of his new father. These certification banquets theoretically become larger and larger until they reach the maximal segments of the localised lineage, one of which now incorporates nearly a thousand living members. Adoption beyond this point requires a banquet and a certification ceremony for all the elders and important leaders in San Tin.

VI

Beyond the lineage: adoption of outsiders

When a member is unable, or unwilling, to find a prospective heir within the lineage, he must go beyond the village and buy a son from a poor outsider. The founding ancestors placed severe restrictions on this type of extra-lineage adoption because they felt it threatened the 'purity' of the patriline.[5] Furthermore, members who have been fortunate enough to produce their own sons ordinarily resist all efforts to break down the closed nature of the descent group. They prefer to keep the economic and social benefits of lineage membership circulating among their own heirs. According to the rules in San Tin, any adopting father who attempts to bring an outsider into the lineage must submit to an initiation ceremony during which he is humiliated by his peers. (Although this is technically the son's initiation, his father undergoes it for him.) If the father proceeds with an outsider adoption without submitting to the ceremony, the ultimate sanction is enforced by the elders and the offender is formally expelled from the lineage.

The initiation takes place during an elaborate banquet which the father must give for all the elders and leaders in the lineage, plus all males who descend from his own grandfather. In recent generations this can easily include up to 400 people. Unlike wedding banquets, the guests do not bring gifts of money to compensate the host for his hospitality. The whole tone of the banquet is different because the adopting father must compensate his fellow lineage members for accepting an outsider into their midst. The guests try their best to humiliate the host by shouting insults about his inability to produce his own heirs. During the banquet any guest may seek out the host and borrow money on the spot. This is done with full knowledge that the lender will never ask for repayment, for it would only be an embarrassing reminder of the initiation. At the culmination of the feast the adopting father asks every elder present to sign a cloth banner certifying his recognition and acceptance of the new lineage member. The host must buy each signature for about £3, although some elders may demand more (in 1970, there were 164 elders in the Man lineage). As they leave the hall, the guests berate the host for defiling the lineage and complain about the miserable food.

If the elders agree to accept the new member by signing the certification banner, the adoption is sealed and the boy's name is recorded in the genealogy. He is given full inheritance rights, as well as an annual share of sacrificial pork which symbolises membership in the lineage. All his ties to his original family are cut and he becomes

dependent on the lineage. His position, however, remains somewhat ambiguous because the other members never forget his origin (cf. Smith 1900: 252). In later life the adopted outsider may be called 'mixed breed' or 'bought son' behind his back, but never to his face. His father has paid the price of entry so the son has a legal and moral right to be accepted as a full member of the lineage. Many of these adopted outsiders try to overcompensate for their ambiguous position by becoming the staunchest upholders of lineage traditions.

VII

Genealogical data: ideal upheld

One might ordinarily expect that these adoption restrictions would encourage the Mans to choose sons within their immediate agnatic group whenever possible. Furthermore, one would assume that cases of adoption from outside the lineage would be extremely rare because the restrictions are so severe and the cost so high. Yet, when the available sources of adoption data for the Man lineage are examined, an unusual pattern emerges which supports the first assumption but not the second.

Written genealogical records are one of the few sources of quantifiable data on adoption in the New Territories Like most other powerful lineages in China, the Mans have a genealogy (*tsu p'u*) that lists as nearly as possible all males who descend from the founding ancestor. Besides the names and lines of descent, the Man genealogy also records a limited amount of personal data for selected individuals including surnames of wives, scholarly achievements, unusual traits, success in business, and other details of particular interest to the chroniclers (cf. Hu 1948: 43 sqq.). The version of the Man genealogy used here[6] also lists 89 cases of agnatic adoption showing the lines of exchange for cases within the localised lineage at San Tin and the village of origin for cases in which the sons originated outside the localised lineage. Before proceeding with the analysis of these cases, a number of qualifications are in order.

It is well known that Chinese genealogies contain inaccuracies because later generations often attempt to enhance their own prestige by embellishing the record of their ancestors (see e.g., Eberhard 1962 46 sqq., Freedman 1966: 26–31). Furthermore, research by Meskill has shown that adoption data recorded in Chinese genealogies reflect only those cases conforming to lineage ideals while extra-lineage adoption is disguised (1970: 150). These qualifications certainly apply to the Man genealogy discussed here. There are no examples recorded of a member procuring an heir from a family with a different surname although I have evidence of several such cases from interview sources. One would expect, therefore, that the adoption cases included in the genealogy would conform to the ideal system previously outlined.

The Man genealogy covers twenty-four generations and includes entries on approximately 2,000 male descendants of the founding ancestor. The chroniclers chose to record only eighty-nine cases of adoption, eighty of which occurred within the localised lineage, while only nine involve sons born outside the village. These cases are best analysed from the perspective of the adopting father who occupies the position of ego in the following discussion. It is convenient to categorise the cases according to agnatic levels arranged hierarchically within the

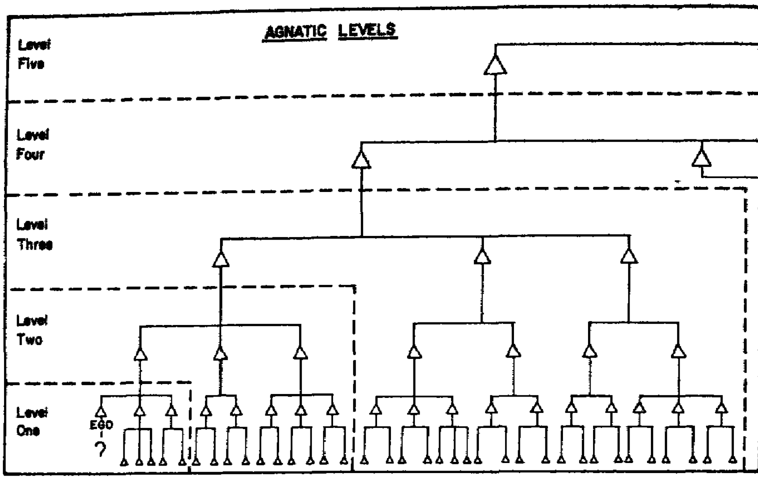


FIGURE 1

lineage (see fig. 1). Each level corresponds to a specific group of kinsmen and is identified by the phrase 'descendants-of-same-(agnate).' According to this classificatory system, descendants-of-same-father (i.e., brothers) constitute agnatic level one; descendants-of-same-grandfather, level two; and on up to level five, descendants-of-same-greatgreatgreatgrandfather.

As expected, adoption entries in the Man genealogy conform closely to the lineage rules that encourage adoption within the closest possible agnatic level (see table 1). For example, adoption between brothers at agnatic level one constitutes 55 per cent. (N=49) of the cases recorded. The second agnatic level, descendants-of-same-grandfather, account for 27 per cent, the third level 6 per cent., and the

TABLE 1. Adoption entries in the *Man* genealogy.

A *Adoption cases within localised lineage.*

Agnatic Level	*Kin group* ('*Descendants-of..*')	*Number of cases*	%
One	Same father	49	55
Two	Same grandfather	24	27
Three	Same greatgrandfather	5	6
Four	Same greatgreatgrandfather	2	2
No cases recorded beyond agnatic level four in localised lineage.			
	Sub-Total	80	90

B *Adoption cases outside localised lineage*

Level			
Man higher-order-lineage (same founding ancestor)		8	9
Man clan (same surname)		1	1
	Sub-total	9	10
	Total adoption entries	89	100

fourth level 2 per cent. The Man genealogy examined in this study does not list any cases of localised lineage adoption beyond agnatic level four. However, nine cases of adoption outside the localised lineage are recorded. Eight of these are at the higher-order-lineage level (same founding ancestor) and only one case is noted at the clan level (same surname).

Undoubtedly there were numerous cases of adoption that the chroniclers chose not to record during the twenty-four generations covered. The genealogy lists 237 lineage members who died without an heir to carry on their line (approximately 11 per cent. of the total entries), which means that sonless males were quite common in lineage history. Unfortunately it is not possible to determine from the genealogy how old these members were when they died. Similarly, the entries do not distinguish between cases of adoption while the adoptive father is still alive and post-mortem adoption in which the son, who may be an adult, is crossed over to continue a branch of the patriline (Nelson 1969: 119). It is clear that these genealogical data should be used with caution because they reflect the ideal system of adoption. However, they do demonstrate that the lineage rules have had an influence on actual behaviour in a large number of cases.

VIII

Adoption rules and practical considerations

In reviewing the genealogical evidence a question arises that can only be answered by assuming that the rules of adoption are sometimes in conflict with the practical considerations of procuring a reliable heir. Why do the chroniclers stop recording adoption cases at the fourth agnatic level, and yet find it appropriate to include nine cases of adoption beyond the boundaries of the localised lineage? The explanation may be simply that the men who kept the genealogy considered these nine cases worthy of recording. The adopting fathers had followed the rules at least to the extent of selecting heirs who were members of the same higher-order-lineage or the same clan. It is understandable that the chroniclers chose not to record any cases of outsider adoption, but it is not so clear why they did not make note of adoption beyond agnatic level four in the localised lineage at San Tin. Cases at level five and beyond would be even closer to the ideal than the higher-order-lineage entries. It is reasonable to conclude that there may not have been any cases of adoption beyond agnatic level four in the local lineage, or if such cases did exist, they were extremely rare. (I did not discover any instances of agnatic adoption beyond this level in my interviews with residents of San Tin.)

It is my contention that the pattern of adoption revealed in the genealogy is not a reflection of the chroniclers' biases but is, in fact, a consequence of the segmentary structure of the Man lineage itself. There must be a good reason why the Mans chose not to adopt too 'deeply' into their own localised lineage. Furthermore, as outlined below, there is considerable evidence that the Mans have not been entirely successful in limiting outsider adoption even though the restrictions are still enforced. With these qualifications in mind, it would appear that the adoption rules have predictive value only up to agnatic level four in the lineage. Beyond this level a number of practical considerations begin to interfere with the fulfilment of the ideal model.

Besides the genealogy, the other sources of information on adoption in the Man lineage are my own observations and interviews in San Tin. After several months, I began to learn that there were more cases of outsider adoption in the village than anyone was willing to admit. Some of these adopted outsiders have become respected elders entrusted with responsible positions of authority in the lineage. Others have not been able to overcome their ambiguous status and live on the social periphery of the lineage. Except when they are angry or intoxicated, few of the villagers will ever discuss individual cases of outsiders who were brought into the lineage. Although it is difficult to be certain, these probably account for no more than one per cent. of the males in the lineage.[7] However, given the severe restrictions against non-agnatic adoption, this is more than I had originally anticipated

The usual story offered by the villagers to explain outsider adoption is that the father was unable to find a son in the lineage and therefore had to buy an heir from strangers. Anyone who was interested could probably have found a suitable heir in a lineage settlement the size of San Tin The father must have made a conscious decision to skip over the lineage and adopt an outsider in the majority of the cases known in San Tin. From the adopting father's point of view, these decisions are eminently reasonable

The adoption rules in the Man lineage are quite explicit regarding the obligations and duties of the father, but they are somewhat vague about the position of the son. As in the case of two brothers 'sharing' a single heir, the duties of the son are never clearly defined In cases beyond this first agnatic level, the son is expected to serve his adoptive father without any consideration of his original position in the lineage. However, these obligations are not subject to enforcement by the lineage unless the son's neglect of his adoptive father becomes so blatant that it reflects badly on the lineage as a whole. The adopting father can never be certain of his son's loyalty if the biological father, or the biological father's immediate family, lives in the same community Herein lies the cause of the unusual patterning of adoption in San Tin.

In cases of adoption within agnatic levels one and two (descendants-of-same-grandfather) which constitute 82 per cent of the genealogical entries for San Tin, the adopting father presumably is secure in his control over the son. At this level the adoption is usually contracted only after consulting with everyone concerned The parties involved may own property together and could even share a common budget if the grandfather is still alive. This does not imply that levels one and two are free of tensions (they certainly are *not*) but it is unlikely that a biological father could ever raise claims on the adopted son of a close agnate without risking severe sanctions. Adoption within the first two agnatic levels, therefore, is easily regulated by the members involved.

A similar pattern of social control may operate at levels three through five. The fifth agnatic level in our discussion corresponds in part to the *wu fu*, a recognised kinship category in Chinese society. Literally, the term *wu fu* means 'the five mourning grades' which relate to a set of clearly prescribed mourning obligations for deceased agnates within four ascending and four descending generations of ego (see Freedman 1958: 41–5). Mourning, however, is only one aspect of the *wu fu* relationship. Freedman has demonstrated that the *wu fu* forms the agnatic core of

the Chinese kinship system. For instance, domestic ancestor worship (as distinct from worship in lineage halls) is normally confined to the ascending four generations (Freedman 1970: 166). Interpersonal relations within the *wu fu* are often intimate and the members are expected to co-operate in many ways. It is understandable that an adopting father might feel reasonably secure in choosing an heir from his own *wu fu* kinsmen.

As one moves beyond the fifth agnatic level in the lineage, however, the kinship ties become less immediate and the possibilities of ambiguity in the adoption relationship increase accordingly. The certification ceremony is an attempt to preclude any later claims, but there is always a chance that the biological father may reassert himself later in life. This is especially true if the adopted son happens to become a successful leader or a rich merchant.

If the adoption has been contracted between fathers who are members of opposing lineage segments, later claims on successful sons are difficult to prevent. Intra-lineage rivalries were very common in San Tin during traditional times and there is little reason to doubt that the segment of an adopted son's biological father would miss the opportunity to press a claim if it were to their advantage.

The only way to avoid these conflicts is to choose an heir from a family that has no chance of causing problems when the son has matured. An outsider son is completely cut off from his past once he enters the lineage as a certified member. The Mans insist on actually purchasing the infant because, in so doing, they remove some of the ambiguity in the relationship between families. If he is challenged, the adoptive father can often produce a signed contract which proves he has already compensated the boy's biological family. As a further protection against later claims, the adoption might be arranged through trusted intermediaries [8] who keep the identities of both parties secret. When this method of procurement is unsuitable the searcher will make every effort to buy a son from a family living as far away as possible from San Tin The real origin of these outsider sons is one of the most closely guarded secrets in the community; adopting fathers never discuss the subject, and they are never asked. Lineage members can only assume that the son came from the lowest strata of society in the Hong Kong region.

No matter how humiliating it might be for the adopting father, this arrangement is more secure than finding a son whose family is known in the community. The adopting father has more at stake than his own pride since he is responsible for producing heirs who will carry on the worship of his immediate ancestors. Furthermore, his personal comfort and security in old age depend almost entirely on the support of his son. Although some lineages offer welfare benefits to destitute members, there are few provisions in Chinese peasant society for taking care of the aged other than those made by the immediate family. This is one reason why a filial son is often more highly valued than a rich or successful one.

An outsider son may be a cause for embarrassment on occasion, but his loyalty to his adoptive father is never in question. His continued membership in the lineage is entirely dependent on the father's decision to keep him in the family. If the heir proves unfilial or undesirable for any reason, his father can terminate the relationship by simply stating in public that he is looking for a new son. The outsider has no right of appeal and is expelled from the lineage without even the dignity of a formal expulsion (*ch'u tsu*, 'out of lineage') ceremony. Thus, the father has absolute

and complete control over an outsider son. If he had chosen an heir who was already a member of the lineage, the father would not have this advantage and might find himself abandoned by his adopted son.

At this point I would like to return briefly to the problem of agnatic adoption beyond the localised lineage (i.e., at the higher-order-lineage and clan level). The fact that the genealogy records only nine cases of adoption at this level in twenty-four generations indicates that it was not a very popular option. However, even in these few cases the main argument of this article holds true. The villagers could not recall any actual examples in recent generations, but they reasoned that the adopting fathers would always have the upper hand because the other branches of the Man higher-order-lineage are much smaller and far less powerful than San Tin.[9] It is doubtful that the biological family of an adopted son would feel strong enough to press demands on a member of the dominant lineage branch.

IX

Conclusions

In this article I have demonstrated that the rules of agnatic adoption are often in conflict with the realities of life in a Chinese lineage The evidence suggests that an adopting father conforms to the rules whenever possible, but only if he is reasonably certain that he will have firm control over the heir. Adoption among the Mans might be viewed as a game of chance with exceptionally high stakes The father can never be certain that he has chosen a filial son or that his authority over the heir will go unchallenged later in life. However, by selecting carefully he can minimise the possibilities of future claims.

It is clear from genealogical data that adoption within agnatic level two, descendants-of-same-grandfather, is perceived as a relatively secure choice. Adoption further up the agnatic hierarchy is less secure because it becomes enmeshed in the continuous and fluctuating round of segmentary rivalries that characterise powerful Chinese lineages These rivalries make intra-lineage adoption a high risk for the adopting father who is unable to find an heir among his more immediate agnates. Under the circumstances, the odds of a secure adoption favour skipping over the lineage and seeking an heir among poor outsiders.

NOTES

The author wishes to thank the Foreign Area Fellowship Program, SSRC-ACLS (USA), for its generous support of the field research upon which this 1969–1970 study is based. An earlier version of this article was read at the 70th annual meeting of the American Anthropological Association in New York City (1971)

[1] Fifteen years ago San Tin's agricultural economy collapsed and the village became an emigrant community which is now completely dependent on remittances from restaurant workers in Europe (see Watson 1975) The land around San Tin fell into disuse and, as a consequence, many of the lineage segments lost their independent sources of income The Mans are beginning to underplay the significance of segmentary divisions. This is a relatively recent development, however, and it does not change the main argument of this article. Except for a few cases, all of the adoptions under discussion were contracted well before the economic decline of San Tin's ancestral estates.

[2] Any male offspring can carry on a man's patriline, but the Mans feel that it is particularly important for the first-born son to have heirs whenever possible. The domestic ancestral tablets are kept in the house of the oldest brother and he sometimes inherits an extra share of the family property.

[3] This form of adoption is 'real' in the sense that the older brother usually takes the son into his own household and acts as his legal guardian. Men who are forced to give up their only sons in this way inevitably feel a deep resentment toward their brothers. In one case I knew particularly well, the older brother took possession of the child as an infant and guarded him jealously throughout his life. The original father was never permitted to be alone with his son, even after forty years (see also Eberhard 1962: 159–60).

[4] Baker (1968: 49) and Potter (personal communication) also report the practice of certification for cases of adoption in the nearby single-lineage villages of Sheung Shui and Ping Shan.

[5] The restrictions against outsider adoption in some lineages are more severe than those described for the Mans. In a study of the written regulations of sixty lineages, Liu found that fifty-one had negative attitudes toward outsider adoption (1959 75). In some cases the penalty for choosing an outsider son was public flogging in the ancestral hall and denial of all lineage privileges (Liu 1959. 74).

[6] There are several versions of the Man genealogy because each major segment keeps its own records All the versions, however, are copies of the official genealogy kept by the lineage master. They differ only in the records of the most recent generations which have not yet been consolidated into the official version. The one used here is a hand-written edition I managed to photograph in San Tin

[7] This figure is a rough estimate based on the seven known and seven suspected cases of outsider adoption I discovered in San Tin Assuming a membership of approximately 1,700 males in the current generation, 00 8 per cent. is a reasonable estimate

[8] According to several informants, the retail rice merchants in the nearby market towns of *Sham Chun and Yuen Long were intermediaries for many adoption arrangements.* These merchants have extensive networks in the surrounding region which allow them to set up 'blind' adoptions between families that do not wish to meet The practice of buying sons appears to be quite common in south China (see e g , Freedman 1966: 7)

[9] There are several other branches of the Man higher-order-lineage in San Tin's immediate vicinity. These include two smaller single-lineage villages, part of a tri-lineage village (three surnames, one of which is Man), and several fragmentary groups scattered throughout the New Territories All the branches trace descent to San Tin's founding ancestor and they all send representatives to the annual worship at his grave In the past, members of the Man higher-order-lineage relied on San Tin's militia for protection against hostile neighbours

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CORRESPONDENCE

Samoan tattoos and structural analysis

SIR,

Professor Paul Shankman (*Man* (N.S.) 10, 126–8) seems to have missed the main argument of my article (Milner 1969) and to be tilting at phantom targets. I did not set out to vindicate structural analysis. On the contrary I argued that one can account for Samoan tattooing on structural grounds but that the result is not conclusive. This is because (i) corroboration is not obtainable; (ii) rationalisation cannot be ruled out, (iii) intuition (in the Bergsonian sense) is involved.

Since the original argument is already six years old, let me sum it up briefly:

1 In Samoa an indigenous form of tattooing has survived. The process is traumatic and potentially dangerous. A minority of young male adults are extensively treated over an area of the body which centres on (but excludes) the genitals.

2 The traditional tattooing song includes the following two lines

> A man grows up, and is tattooed
> Women grow up, and bear children.

3 The myths state that it was two Samoan women who first learnt the craft from two Fijian men, and explain why in actual fact the Samoan practice of extensively tattooing men, but not women, is the inverse of what used to happen in Fiji.

Now, apart from any construction that one may wish to place on these data, surely no one could dispute the following:

1 Samoan women cannot be extensively tattooed in the area centring on the genitals, in certain age groups they can bear children.

2 Samoan men can be extensively tattooed in the same area, they cannot bear children.

Ignoring these and other correlations mentioned in my article, Paul Shankman singles out the fact that nowadays both tattoo and pregnancy are covered by clothing, and he states that this is modern, due to missionary influence and hence irrelevant. Now I was describing a synchronic situation, but even if we allow an appeal to history, this argument is not cogent.

He is correct in stating that even today small portions of a man's tattoo are visible at times above and below his clothing, just as (to paraphrase Dorothy Parker) we all know that a pregnant woman has it in her, however well she may try, or pretend, to disguise her condition. In any case, cover, partial or total, for whatever reason, is often more suggestive than exposure, so the argument cuts both ways.

Seclusion and pre- or post-missionary modesty aside, if as Shankman alleges, no notional or cognitive equivalence is involved between tattoo and pregnancy, the onus is on him to tell us why the area covered by a man's tattoo (from the lumbar region to the knees) and the area affected by a woman's pregnancy, are co-extensive. Thus while a man's design reaches down the length of his thighs, I am reliably informed that the stretch marks that are left after pregnancy are not only visible on a woman's abdomen, but also continue down her thighs.

Shankman asks 'how do we know that a cultural need to restore the balance of the sexes exists in Samoa? Or anywhere else for that matter?' Surely a rather naive question. For evidence of women's response to inequality between the sexes I need only mention women's liberation movements. On the other hand *Symbolic wounds* (Bettelheim 1954) is a most comprehensive summary of evidence that in many different cultures men too are deeply affected by, and react positively to, sexual imbalance. In a recent article (Milner 1974) I have drawn attention to the existence of other similar phenomena (including some in Western societies).

Finally, Shankman ascribes to me the words which I quoted with approval, but which were not mine. 'Structure has a grip on us' was my attempt to translate the idiomatic expression used by the man to whose memory Lévi-Strauss dedicated *La pensée sauvage* (Merleau-Ponty 1960, 147). 'Elle (*sc.* la structure) "les a" plutôt qu'ils ne l'ont' (*cf.* Milner 1969. 22, note 6).

Space, Sir, forbids me to say more. However, until Shankman can offer you a more satisfactory explanation than mine, perhaps we can put away the tattooing combs and let the case rest.

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Anthropology and Nannies

SIR,

It may seem odd for an American to respond to a letter written by another American about the English family in a British journal, but since anthropologists are usually dealing with social matters in societies other than their own, it may be permissible to take such a liberty The correspondence under consideration is that of Mr James A Boone (*Man* (N S) **9**, 137–40) titled 'Anthropology and nannies' In it the author maintains that 'it took the Nayars to boot the "father" out of universal social-interactional families, perhaps the Nannies can do likewise for the "mother"? That position was supported primarily by evidence from Gathorne-Hardy's (1972) description of nannied families, and on the basis of this work, Boone contends that Nannies are the mainstay of a system in which 'the nurturance-emotional complex that Goodenough functionally related to one woman and a set of biologically or (by extension) juridically assigned children is parcelled between two women'

Neither Goodenough nor anyone else has maintained that mothers are solely responsible for and involved in the care of children Quite to the contrary, it would seem that almost every society provides culturally available roles to assist the mother in these functions. These others who help the mother in performing those activities may include husbands, grandparents, siblings, co-wives, elder offspring, servants, slaves, teachers, etc. In the case of Victorian England, this other was a specialised servant called the 'Nanny' She (as both Boone and Gathorne-Hardy mention) wiped bottoms, wiped up sick, gave baths, and so forth, in essence, the Nanny was primarily concerned with removing what might be considered the less desirable aspects of the mother-child relationship These authors are, however, less concerned with the fact that this could have left the mother and child the opportunity to enjoy the more pleasurable aspects of the relationship. If anything, this may have allowed for more endearing bonds between mother and child since the child was no longer a messy, loud, irritating burden requiring constant attention. The mother was similarly no longer in the immediate role of disciplinarian to the child. The child was allowed to be a greater delight to the mother, and the mother was free to concentrate on fulfilling the role of a warm, loving, and kind adult to the child. It would seem that the significance of a minimum of one pleasant hour every day with another individual has been underestimated, it is probably far more than most mothers and children share with each other when they are forced into ten to twelve hours of unrelenting interaction daily. The Nanny was, therefore, an extremely effective device for removing almost all the displeasing aspects of motherhood, and it is probably true that warm emotional relations often developed between the Nanny and her charge. This does not, however, 'boot' the mother out of the nuclear family. No evidence was introduced by either author to indicate that the mother-child bond was in any way diminished or that the mother's responsibility for the child (in a social-jural sense) was lessened. It could easily be the case that the mother-child bond was in fact more strenuously maintained. To this effect, one might consider Bateson's observations on the English parent-child-nurse relationship as a 'ternary system' in which 'the nurse teaches the child how to behave toward its parents' (Bateson 1942). Thus, instead of weakening the mother-child relationship, the Nanny is actually concerned with sustaining it in a culturally prescribed manner.

One might ask what it was about Victorian society which led families to need or want to bring in this professional socialiser to assist the mother in fulfilling her role to her children. The answer to this would seemingly be found in a particular constellation of social factors in Victorian society, among these are the following. 1) the unprecedented decline in infant mortality rates which meant that families were having more children to socialise, 2) the relative prosperity of England at the time; 3) the manner in which this prosperity was differentially distributed so as to supply a large number of families with the economic resources to have servants and an even larger number of individuals available to perform such work; 4) an already well developed pattern of servant employment among the upper class (with a concomitant de-emphasis of kinship ties in assistance with domestic duties), and possibly later; 5) a newly developing avenue of prestige in having a specialised children's servant and in having well-behaved (socialised) children.

This peculiar set of circumstances allowed for the development of the institution of

'nannied families' on a much larger scale than would have previously been probable. As such, however, the Nanny appears to have been a relatively temporary phenomenon as most of the above conditions began changing rapidly with the onset of the twentieth century (e.g., as technology began moving more into the home to assist in the care of children and into sexual practices to allow for decreasing family size, as England's relative wealth declined and the distribution within the country became more equalised, as the overall employment of servants declined, etc.) While she lasted, however, the Nanny rather than being a threat to the nuclear family was an extremely effective mechanism for helping the family meet the threats from a set of extraordinary demographic, economic, and social forces.

As previously mentioned, Boone's correspondence was based primarily on Gathorne-Hardy's *The rise and fall of the British Nanny*, a book which Boone seems to recommend to anthropologists as an instance in which the author of children's books has made some profound contribution to social theory. Boone supplies a list of twelve social phenomena (ranging from the 'Origin of the Factory Acts' to the 'critical acclaim of Proust in England' and including along the way an obscure analogy of 'nursery: parlour-dining room: brothel. business-place') which Boone seems to believe are significantly illuminated by Gathorne-Hardy's analysis of the British nursery. While one may concur with Boone in the desire that anthropologists maintain acquaintance with writings in other fields, it is difficult to ascertain what it is that one is suppose to derive from this particular book. Gathorne-Hardy has written a very readable account of the institution of nannied families based on thirty-four interviews (of at least two hours each), 250 letters, and a vast array of fiction (including *The kandy-kolored tangerine-flake streamline baby* and *The beastly beatitudes of Balthazar B* which are quoted extensively as illustrations of snobbery among Nannies and of 'Oedipal feelings' which young boys have for their Nannies). Gathorne-Hardy includes very exciting descriptions of a grizzly cut-throat murder in a nursery (291–302), a 40-person bed in a mid-eastern harem (154), the lust of upper-class English men for lower-class women, and there is even mention of an unnamed primitive tribe in which adolescent males must copulate with their mothers prior to entering manhood (97). In all, it represents interesting material for an enthusiast of the television programme *Upstairs, downstairs*, but hardly a sound base upon which to make a serious analysis of the nuclear family in a cross-cultural perspective

Gathorne-Hardy has introduced a theoretical tone into his book by citing the tremendous consequences which the Nanny had in the overall fabric of English society (*ergo* Boone's twelve theses). This he does *à la* the national character studies of earlier anthropological theory. In brief, Gathorne-Hardy does not think that the swaddling of babies has in fact determined the fate of Russia, rather it was the English Nanny which determined the fate of the entire western world.

It would seem that before anthropologists turn their full attention to the writers of children's books, it might be more profitable to examine the relevant research in other, related fields—for example, some of the recent writings done on the basis of ethological studies. As these studies have been conducted over longer periods of time with the same groups of primates, new data have begun to emerge on the importance of 'kinship' among a number of the higher primates (e g , as is indicated in the works of Tsumori 1967; van Lawick-Goodall 1967; Jolly 1972). For the most part these relationships are centred around mother-child bonds and sibling bonds both of which often persist long after infancy It is even noteworthy that among many non-human primates, mothers are not solely involved in the care of their young; any number of other females will at various times assist the other without in any apparent way diminishing the relationship between the mother and child (Jolly 1972, Poirier 1968)

The mention of such research as this is not an attempt to locate some instinctual or genetic explanation for the mother-child bond as the centre of the nuclear family It is merely intended to draw attention to the fact that a diversity of social organisations in a diversity of species (which are closely related evolutionarily) have developed, and all these are based ultimately upon the bonds between females and the children which they bear This biologically based role relationship may be incorporated into a harem band of gorillas, a monogamously-based Victorian family, or a matrifocal and fatherless family among the Nayar All contain at their core the mother and her offspring, regardless of whether the mother is assisted by other female gorillas in the band, maternal kinsmen, the husband, or even a Nanny

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Radcliffe-Brown and Daisy Bates

SIR,

I wish to correct a statement for which, so far as I know, there is no basis in fact

Needham (1974 151) cites a portion of a letter written by Mrs Daisy Bates to J Mathew (published in Salter 1972 147) In this letter Mrs Bates states that a manuscript of her book on the Aborigines of Western Australia, which had been in Radcliffe-Brown's possession, had been returned to her (apparently by representatives of the Government of Western Australia), but it 'was mutilated beyond recovery' Two pages later Needham observes, 'We do not know what it was that Radcliffe-Brown cut out, so extensively as to "mutilate" the copy and make it useless, from Mrs Bates's manuscript, nor do we know what he did with the information contained in the parts that he had removed' Mrs Bates herself twice accused Radcliffe-Brown of plagiarism

I am curious about Needham's grounds for asserting that Radcliffe-Brown cut out, removed, and kept portions of Mrs Bates's manuscript It seems that Needham has inferred this from her assertion that the manuscript returned to her was 'mutilated' It is worth noting, however, that in her letter to Mathew she did not describe the way or ways in which it was 'mutilated', nor did she say that Radcliffe-Brown was responsible for the alleged mutilations, though by reading between the lines, as it were, one could be led to think that she meant to imply that he was Because Needham cites no other documentary sources and adduces no indirect evidence to support his assertion that Radcliffe-Brown cut out, removed, and kept portions of Mrs Bates's manuscript, I must conclude that his statement has no basis in the facts known to him.

There is, moreover, additional evidence which Needham did not consult. As he notes, Mrs Bates's notes and papers are on deposit in the Australian National Library in Canberra I made an extensive study of them in 1971–72, and I examined drafts and copies of the study to which Needham refers At the time it did not occur to me that anyone, other than Mrs Bates herself, would take her charges of plagiarism seriously. The information she suggested Radcliffe-Brown had acquired from her notes and used without her permission, and without acknowledging the source, is, after all, the kind of information he could have acquired for himself in the field If he published or reported it before she did (knowing that she also had it) she could have construed this as plagiarism Throughout her life she felt ill-treated by the Australian anthropological community and she often complained of not receiving credit for 'discoveries' which were in fact common-place ethnographic findings more or less readily ascertainable by anyone who would take the trouble

Moreover, since to my knowledge Mrs Bates never claimed that Radcliffe-Brown cut out, removed, and kept portions of any of her manuscripts, it did not occur to me to examine any of the copies with this possibility in mind However, as I recall, at least one copy of one draft manuscript had been cut up and pasted back together with some additions and, probably, some deletions This was necessary because, in its early stages, the study was styled as a 'history' of the tribes of Western Australia and it consisted mostly of information compiled from published sources Mrs Bates realised quite early that most of this information was incomplete or defective, and she set out to fill in the gaps and to correct the errors by doing her own fieldwork Thus, by stages, she added her own information and discarded most of the references to earlier works, and gradually transformed the study into an ethnographic survey At first this survey was organised along topical lines but later it was organised by areas and tribes. Various drafts were read and commented on by Andrew Lang and Radcliffe-Brown They sometimes wrote queries, directions for revision, and the like on the pages of the manuscript itself. Much later, when the manuscript was revised and retyped during Mrs Bates's retirement in Melbourne, Lang's and Radcliffe-Brown's comments and her responses to them were typed on separate sheets Mrs Bates then wrote additional replies (to herself) on these sheets

It seems to me that, although there is no evidence that Radcliffe-Brown 'mutilated' Mrs Bates's manuscript in the way Needham claims he did, there is ample evidence that the editorial process required much writing on the manuscript and much cutting and pasting back together of its pages. There is no evidence that Radcliffe-Brown did any of the latter, or that, if he did, it was without Mrs Bates's knowledge and consent. Salter (1972: 139) reports that they worked together for a while editing the manuscript in her office in Perth, and so it is possible that some of the reorganisation of the manuscript was done then. The end result of the editorial process, as far as Radcliffe-Brown had anything to do with it, may have been a draft manuscript which Mrs Bates considered 'mutilated'

Perhaps I will be permitted an additional comment on Radcliffe-Brown's claim to 'discovery of the Kariera system' in 1911 Eggan (1974) has already pointed out that Needham is able to show, to his own satisfaction, that Radcliffe-Brown did not 'discover' anything that was not already known about 'the Kariera system' only by restricting his (Needham's) definition of 'the Kariera system' to the four-section system. Radcliffe-Brown, however, used the expression in a much broader sense which included a conception of how the Kariera social system as a whole is structured. It seems worth adding to Eggan's remarks that the central aspect of this conception is the idea that the Kariera system of kin classification is a system of *kin* classification, that its terms are polysemous, that it is structurally prior to the four-section system, that the structure of the four-section system is dependent on the structure of the system of kin classification, and that marriage is regulated first and foremost by the system of kin classification rather than the four-section system This theory or interpretation is quite different from the theory proposed by the other major figures in the field of Australian studies at the time, most notably Spencer, Gillen, Fison, Howitt, and N. W. Thomas who depended mostly on their work—and Mrs Bates Radcliffe-Brown's report on 'the Kariera system' is unquestionably superior to the previously published reports on various aspects of it and, no doubt, he thought that his report demonstrated the validity of his interpretation. I find it more sensible to suppose that this is what he intended by his 'discovery' than to suppose that he was claiming to report, for the first time, certain ethnographic facts which were already well publicised.

Because I am one of the few persons who has had or who has taken the opportunity to study Mrs Bates's unpublished works, perhaps I will be permitted to add that they provide no evidence that Radcliffe-Brown could have learned much about the Kariera from them. There is very little information on the Kariera themselves in Mrs Bates's manuscript or in her field notes. There is much information, however, on the closely related Ngaluma and Mardudhunera in her manuscripts and notes, and Radcliffe-Brown did describe the social organisation of these groups in his famous paper of 1913. But it is clear from the numerous differences between his published accounts and her unpublished accounts that he got his own information on both these groups.

Needham (1974 157, note) suggests that there is much valuable information in Mrs Bates's unpublished notes and papers and that they should be edited for publication Indeed, there is much valuable information in these papers, especially on the areas of Western Australia—the south coast from Perth to Eucla—with which Mrs Bates was most familiar. The task of editing Mrs Bates's manuscripts for eventual publication, perhaps by the Australian Institute of Aboriginal Studies, is now well under way, but I regret that it may be several years before I am able to finish it

Harold W Scheffler

Yale University

Eggan, Fred 1974. Aboriginal sins. *Times Lit Suppl* December.

Needham, Rodney 1974 *Remarks and inventions skeptical essays about kinship*. London· Tavistock Publications.

Salter, Elizabeth 1972 *Daisy Bates* Sydney Angus & Robertson

SIR,

Professor Scheffler's letter compounds a number of questions which are better taken up separately

1 What are my grounds for 'asserting' anything about Radcliffe-Brown's treatment of Mrs Bates's manuscript?

I assert nothing on my own account. I simply repeat what the author herself wrote about the fate of her own work I claim no acquaintance with any facts of the matter over and above what are in the sources that I cite. They are my grounds

2. What was done to Mrs Bates's manuscript? According to Scheffler, Mrs Bates herself 'never claimed that Radcliffe-Brown cut out .. portions of any of her manuscripts', and the assertion that he did so is merely what 'Needham has inferred'

Mrs Bates wrote that the only copy she had of her book was 'mutilated'. She did not write that it had been, as Scheffler conjectures, reorganised, revised, or otherwise edited; nor did she write, as by Scheffler's supposition she might have done, that it had been written over or tampered with or defaced. She said it had been mutilated. To mutilate *means* 'to render (a thing, esp. a record, book, etc.) imperfect by cutting off or destroying a part' (*O.E.D.*) The word comes from the Latin *mutilāre*, to cut or lop off.

When Mrs Bates wrote that her manuscript had been 'mutilated beyond recovery', she could hardly have been alluding to even the most zealous editing. Her words 'the mutilated portions' mean literally those parts that remained after other parts had been cut away.

3. Who mutilated the manuscript?

The manuscript was given into Radcliffe-Brown's custody in 1910, by the government under-secretary, on the condition that he should edit and publish it In September 1912 Mrs Bates wrote that the government had relieved Radcliffe-Brown of the responsibility and had returned the mutilated and 'useless' manuscript to her

It is wholly improbable that the under-secretary or any other government official should have mutilated it, and there is no indication that any such person did so Radcliffe-Brown was responsible for it, and it was he who must be held responsible for the reported mutilation I do not 'claim' that he did it. the evidence to that effect consists in Mrs Bates's own words Elizabeth Salter reads these in the same sense (1972. 141) and, speaking of Radcliffe-Brown, writes of 'his mutilations' (147) Scheffler, on the other hand, provides no evidence that whatever was done to the manuscript was the work of some person unknown, and there is no reason indeed to think that anyone other than Radcliffe-Brown had any part in the affair

4 What is the extant manuscript evidence?

Scheffler reports seeing ('as I recall') in Australia a copy of a draft manuscript that had been cut up and pasted back together with some additions and 'probably' some deletions. A recollection of this kind, however interesting as a reminiscence, is not textual evidence

To make a relevant argument about the present issue would require a positive identification of the remaining parts of the original manuscript as it was when returned to Mrs Bates in 1912, together with textual proofs of any alterations to it In the case of deletions, we should need to know (e.g , by comparison with Andrew Lang's copy, if this could be traced) what was written on them. Scheffler has supplied no such factual demonstration. I have published a plea (Needham 1975) for information on the rumoured survival of the original manuscript, but have so far (12 March, 1975) received none.

5. Did Radcliffe-Brown commit plagiarism?

Mrs Bates twice at least charged that he did Everything that can be learned from published accounts (Hill 1973: Salter 1972) about her character testifies to her rigid sense of duty in every matter, and to her utter and unfailing rectitude. Scheffler, without addressing himself to the facts of either instance of alleged plagiarism, resorts instead to impugning Mrs Bates's good faith

6 What did Radcliffe-Brown discover?

I have nowhere written that Radcliffe-Brown 'did not "discover" anything that was not already known about "the Kariera system". ' Even Eggan (1974) does not accuse me of as much. What I actually say is, to the contrary, that Radcliffe-Brown 'must have contributed something original' to the quantity of 'detailed and useful information' in his 1913 paper (Needham 1974 156). Of course, what is in fact original is at present undecidable, so we do not know for sure what ethnographical discoveries he did make Nevertheless, I find the genealogical specifications and the details of the bisectional local groups particularly valuable, and I am perfectly ready to accept that these are Radcliffe-Brown's independent findings

What is not correct is that Radcliffe-Brown discovered, in any sense, 'the Kariera system', however this may be defined The materials brought together in my essay (1974. ch 3) already demonstrate this, and it would be easy (though a rather lengthy task) to draw up a further point-by-point reckoning of all the ethnographic particulars on which Radcliffe-Brown was forestalled For the present, I repeat (cf Needham 1975) that Eggan is mistaken in implying that Radcliffe-Brown discovered the kinship system, rules of marriage, division of labour, totemic cults, etc., or that he had an original conception of their systematic interconnexions.

Similarly, Scheffler is mistaken in suggesting that Radcliffe-Brown's conception of 'how the Kariera social system as a whole was structured' was a discovery. The four-section system was discovered in the Kariera region in 1878 by Forrest, who also recorded the rules of marriage and section-assignment, together with a diagrammatic demonstration of the congruence between genealogical connexions among relatives and the articulation of the sections (see Needham 1974 140–1). Withnell, in 1901, reported the terms

for relatives, with definitions by which they can be directly plotted on to Forrest's diagram, and also more on the rules of marriage and other institutions (Needham 1974: 141–3). With these two sources alone, therefore, an integral account of the structure of the four-section system in Western Australia was in print years before Radcliffe-Brown even read the 'Australian data' (including Mrs Bates's chapter on social organisation) in 1909. The conception that Scheffler credits to Radcliffe-Brown is indeed no more than what is conceded to be an 'interpretation', ascribing functional efficacy or relative degrees of importance to known social facts By no accepted criterion can this be regarded as a discovery in the field

In any case, with each additional subtlety or systematic conception or item of knowledge that Eggan or Scheffler may attribute to Radcliffe-Brown, the more glaringly awkward does it become that Radcliffe-Brown himself said nothing of the kind in the passage in question. All he said by way of definition of the Kariera system, in his claim to have discovered it, is 'some such system'

7. What is the significance of Radcliffe-Brown's claim to have made a discovery in Western Australia?

The crucial issue in this whole affair, and that to which neither Eggan nor Scheffler pays any attention, is that the discovery has been represented as a remarkable ethnographic confirmation of a prior surmise That is what my investigation is all about prediction.

Any reader of Lévi-Strauss's repeated statements on this score will think he has it on the best professional authority that Radcliffe-Brown, by his surmise and subsequent discovery, achieved 'a memorable deductive success' that 'will forever remain one of the great results of socio-structural studies', and that this feat contributed to the understanding of 'the unconscious workings of the human mind' The even more hapless reader who relies on Kuper's account of modern British social anthropology (1973. 60) will similarly be deceived into thinking he has scholarly warrant that Radcliffe-Brown's putative discovery was a 'triumph'—a *triumph* of structural prediction and ethnographic confirmation.

I hope I shall not have to repeat again (cf Needham 1974: 135–7, 1975), after this occasion, that there is no independent evidence of Radcliffe-Brown's surmise, or of the formal terms in which he stipulated in advance the distinctive features of the hypothetical system that he later claimed to have discovered among the Kariera, and that every single particle of circumstantial evidence tells against it. If Scheffler is so interested in the facts of the matter, he might better seek to provide a factual 'basis' to the claim that Radcliffe-Brown's surmise (whatever he may be supposed to have had in mind) was ever made, let alone triumphantly borne out by a discovery in the field. Without such decisive and indispensable testimony it cannot be argued that Radcliffe-Brown (whatever he may really have discovered) successfully confirmed a *prediction*. In default of any present factual qualification of the kind, all the evidences accumulated in my study of the Kariera case indicate that the story is untrue, and that Lévi-Strauss and, egregiously, Kuper have perpetrated a rhetorical imposition on those who are not in a position to know otherwise.

I should not wish anyone to be any less sceptical in weighing these contentions than I have been in assessing Radcliffe-Brown's claim and the consequent panegyrics. But I think it may well be looked for that any attempted refutation of my historical findings shall consist in the scholarly treatment of factual evidence, not in tendentious supposition or vague recollection or the imputation of discreditable personal motives.

Rodney Needham
University of Oxford

Eggan, Fred 1974. Aboriginal sins [review of Needham 1974] *Times Lit Suppl.* 13 December, 1974, 1402–3

Hill, Ernestine 1973 *Kabbarli a personal memoir of Daisy Bates* Sydney: Angus & Robertson.

Kuper, Adam 1973. *Anthropologists and anthropology the British school 1922–72* London. Allen Lane, The Penguin Press.

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——— 1975. Letter: 'Radcliffe-Brown'. *Times Lit Suppl* 3 January, 1975, p. 14

Salter, Elizabeth 1972. *Daisy Bates.* Sydney Angus & Robertson

Death by suffocation

SIR,

In a letter to this journal which unfortunately has gone unchallenged, T. O. Beidelman (*Man* (N.S) 4, 290–1) addresses the problem of the Nuer *gorot* ritual, in which an ox is killed by suffocation. He suggests that Evans-Pritchard's (1956: 217) uncertainty about the symbolic meaning of the ritual can be resolved by his own analysis In this brief communication we hope to show that although the mentor somewhat restricted his

explication, it was Beidelman who unnecessarily confused the matter by raising extraneous issues. Here we suggest an interpretation of the *gorot* ritual which derives from and supports Evans-Pritchard's and Lienhardt's (1961) analyses of Nilotic symbolism

The single *gorot* ceremony which Evans-Pritchard observed was performed for the benefit of a female who had been 'espoused early in life, probably before the commencement of the menses' (Evans-Pritchard 1956: 217). Nuer stated that the purpose of the ritual was to ensure that a child would be born of the union in spite of the unusual circumstances of the betrothal (1956 218). Beidelman surmises that the ritual performed on this occasion involves the 'sacrifice' of an ox, as this is ostensibly another Nuer ritual which serves to 're-establish ordinary conditions' (Beidelman 1969 290). Thus the sacrificed ox serves to remove the 'taint' and 'polluting aspects' of the marriage Beidelman concludes the note by raising the question of the meaning of stuffing grass into the anus of the ox and contends that we must have further knowledge of Dinka cosmology before we can begin to interpret the Nuer data.

Our first argument with Beidelman concerns his reference to the *gorot* ceremony as a sacrifice According to the Nuer ethnographer, the *gorot* ritual 'should not be regarded as a sacrifice at all, at any rate in the usual sense, for the beast does not seem to have been consecrated and there was no invocation' (Evans-Pritchard 1956. 217) Thus, it may be typical of Nuer rituals in general, but not of sacrifice. It is unclear why Beidelman should choose to ignore this information and instead, proceed with an unnecessarily idiosyncratic interpretation of the ritual as a sacrifice

The *gorot* ceremony is different from Nuer sacrifice in a number of significant ways In the typical sacrifice the blood of the consecrated ox symbolically removes the danger which preceded the ritual. For example, in the ceremony to purify an incestuous union the blood of the sacrificial ox carries the sin into the ground. The beast is usually cut in half and the left half of the animal is placed in the bush. 'When an ox is sacrificed there is the idea that the evil goes into the ox and then into the earth with its blood' (Evans-Pritchard 1956 221, 1951*a* 116). Sacrifice also includes phrases of consecration and an invocation to a spirit. Shed human blood is associated with similar polluting qualities (Burton 1974: 29–30), though menstrual 'blood' is believed to be necessary for procreation (Evans-Pritchard 1956: 212–14) and therefore it has some positive symbolic meaning. This also accords with Mandari ideas on the same issue (Buxton 1973: 212). Beidelman suggests that the 'taint' of the unusual marital union is 'mastered by being consumed as flesh'. There is no explicit reference in the published ethnography on the Nuer which suggests that this is typical of *any* Nuer ritual. It is the blood which soaks into the ground which removes the evil eradicated by sacrifice This point has important implications for our understanding of the *gorot* ritual. It is typical of sacrifices which have been described for the participants to shave their heads and affect special bodily ornaments. However, a glance at a photograph (Evans-Pritchard 1956, facing p. 68) of the ritual in question indicates that here this is not the case.

We would argue that because the ox in the *gorot* ceremony is not consecrated, is not the object of an invocation and does not shed blood, the ritual is *not* concerned with removing any 'taint'. There is no evil represented in the ritual. In true sacrificial acts, the ox's blood shields (*gang*) a Nuer from potential or actual symbolic danger. Death by suffocation would indicate that the ritual is *not*, as Beidelman suggests, concerned with the symbolic separation of temporarily confused categories, as no blood is shed.

With this in mind we continue with an analysis of the ritual in the manner which Evans-Pritchard suggested. Specifically, the *gorot* ceremony 'has more the appearance of what we might better describe as a magical rite' (1956 217)

The photograph of the *gorot* ritual mentioned above depicts forcing grass into the orifices of the ox. Unlike Nuer sacrifices, in which there is a preference as to which side the animal should fall, in this instance it is actually positioned on its left side Evans-Pritchard (1956 233–6) discussed in some detail the nature of Nuer symbolism as it is resolved by the complementarity of dual symbolic classifications. Thus for example a man may force his wife to the left side of the doorway before he enters the hut (Evans-Pritchard 1956: 234) Although there is no specific reference in the ethnography, we would also suggest that the woman assumes a position to the left of the male during the sexual act. Therefore, there is a symbolic equivalence between the temporarily barren female and the neuter animal This is especially likely as it is a woman diviner who directs the ritual, and women who force the grass into the ox. As a result, the neuter ox represents the temporarily neuter female. Unfortunately detail is lacking on whether it is a male or female who cuts the throat of the ox once it has suffocated.

Evans-Pritchard (1956. 218) observed that

while the ox was lying on its left side the spouses, along with an age mate of each partner 'sat on its flank'. Beidelman interprets this to mean that these individuals 'bring the lower (generative) parts of their bodies into contact with the ox' (1969: 290). However, he pays little attention to the fact that following this act, the participants are fed a mincemeat of the vital organs of the ox which they masticate but later spit out. As there is no blood involved in the killing of the ox, and there is a symbolic equivalence between the neuter wife and the neuter ox, it is reasonable to suggest that the ox represents the physiological state of the woman, i.e, she is without blood

As we know from the ethnography there is a clear relationship between eating and sex among the Nuer (cf Evans-Pritchard 1951*b* 55, 102). Since they spit out the food we may infer that this expresses the fact that intercourse has not been productive of offspring. We would argue that the absence of blood in the ritual to this point symbolises the incomplete character of the union. A complementary interpretation suggests that the act of sitting on the ox with their generative organs in contact with the animal represents the sexual act with the barren woman. Following this theme, the failure of the ox to shed blood indicates that intercourse *has* been successful as the menses has temporarily ceased As a magical act this serves to represent the problem of barrenness and then conception as the ideal state of affairs

After having sat on the ox, and expelled the mincemeat preparation, the participants enter a hut where the diviner smears boiled blood across their mouths Following this the diviner then smears only the spouses with warm butter. It is certainly not difficult to appreciate that this represents the sexual act through the conjunction of the life creating forces of semen and blood

Finally, the fact that the 'husband's people took the meat of the ox home with them' (Evans-Pritchard 1956 218) is not as confusing as Beidelman suggests if we accept that in performing the *gorot* ceremony, Nuer believe that the woman will bear children and therefore become a full member of the household

In Nuer cosmology there is an underlying theme of cooperation between divinity in some form of *kwoth* and man implicit in notions of procreation Nuer say that 'The child was begotten (*dieth*) by man But it is also created (*cak*) by God, so it appears that they think of conception as a product of this combination of human and divine action' (Evans-Pritchard 1956: 156) In this instance it is not through sacrifice, as Beidelman suggests, that this occurs. Indeed, Beidelman does not even mention the obvious procreation theme of the *gorot* ritual which is expressed through blood, breath and grass, as material symbols of this relationship.

In referring to this ritual as having magical rather than sacrificial characteristics, Evans-Pritchard perceived the fundamental distinction between them If he was indeed 'puzzled' by the meaning of the ritual as Beidelman suggests, perhaps it was because the greater part of Nuer cosmology expresses the relationship between man and *kwoth* in a communicative manner

We can summarise our argument by stating that the first phase of the ritual as we have described it represents the 'reality' of the situation. The marriage had been consummated by intercourse (Evans-Pritchard 1956: 217) but without producing offspring This is symbolically expressed by the fact that the ox does not bleed and the meat is not eaten. In the hut of the female diviner the ideal situation is enacted Blood is mixed with warm butter (semen) producing a foetus, represented by the ox's stomach lining and umbilicus The Nuer ritual described here is more typical of what anthropologists have traditionally called sympathetic magic. Unlike Beidelman, we argue that Evans-Pritchard sufficiently appreciated this characteristic of the ritual

We do not wish to be overbearing but feel it necessary to add that in Nuer cosmology sacrifice is associated with the right and males (Evans-Pritchard 1956 236) In contrast, the ritual which is discussed here involves an association between the left and females

Admittedly our analysis is only suggestive, but it is more in accord with the ethnographic facts than Beidelman's

W. Arens
John W. Burton
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Beidelman, T O 1969. The ox and Nuer sacrifice *Man* (N S.) **4**, 290–1

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Palpable nonsense in the conflict of life and death

SIR,

In his review of my *When the golden bough breaks* in *Man* (N.S.) **9**, 320, Edmund Leach complains among many other things that my statement on p. 49 that the 'structuralist interpretation [of myth] makes it possible for the conflict between life and death to be "resolved"' is 'palpable nonsense'. I owe your readers an apology for not having mentioned on that page that this particular interpretation of Lévi-Strauss, whether it is nonsense or not, is derived from some of the numerous writings of Leach Since I failed to acknowledge my debt to Leach, he must be forgiven for not recognising the source of this 'palpable nonsense' I hope you will allow me to correct my negligence and let me provide the following references to Leach's writings from which that particular view of structuralism is taken. E. R. Leach, *Lévi-Strauss in the Garden of Eden*, cyclostyled version, p 6, lines 6–8 from bottom, 'Genesis and myth', *Discovery*, May 1962, p. 31, col 3, last par and p. 35, col 2, middle par. My use of the words 'conflict' and 'resolved' is directly copied from E R Leach, *Lévi-Strauss*, Fontana Modern Masters, 1970, p 75, lines 7 and 8. In fairness to Leach I admit that I substituted 'conflict' for his 'contradiction'. Although even Leach's chickens sometimes come home to roost, he need not feel too embarrassed because he himself very properly derived this 'palpable nonsense' from the penultimate paragraph of Lévi-Strauss's famous paper *The structural study of myth*, first published in the *Journal of American Folklore*, **78**, No 270, 1955 and since then reprinted frequently

Peter Munz
Victoria University of Wellington

SIR,

Please allow me to comment on Peter Munz's letter. It is now a good many years since Burridge pointed out that Lévi-Strauss often uses dialectic in a Hegelian rather than a Marxian sense so that what is ontologically 'contradictory' is not clearly distinguished from what is logically 'contrary' (Burridge 1967: 95). In the same vein, Friedman has recently drawn a vigorous distinction between 'social contradictions' and 'semantic oppositions' (Friedman 1974: 449), but I have never previously heard it suggested that 'contradiction', misused in the sense of 'semantic opposition', was a synonym for 'conflict'

Munz is correct is stating that the references to 'contradiction' in my Fontana paperback (London, 1970 edition, pp. 71, 75) derive from Lévi-Strauss's exposition of his views in 'The structural study of myth' (Lévi-Strauss, 1963: 216, 229). But there is no confusion in either of these works between 'contradiction', in the sense of paradoxical implication, and 'binary opposition', let alone 'conflict'.

'Life' and 'Death' are binary opposites It *is* just as nonsensical to write of 'the resolution of the conflict between Life and Death' as it would be to write of the resolution of the conflict between black and white, or of the conflict between plus and minus.

Edmund Leach
University of Cambridge

Burridge, K. O. L. 1967 Lévi-Strauss and myth. In *The structural study of myth and totemism* (ed.) E R Leach. London Tavistock.

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Physical Anthropology

BASU, AMITABHA *et al.* (eds). *Physical anthropology and its extending horizons* (S. S. Sarkar Memorial Volume). x, 234 pp., illus., plates, maps, tables, bibliogr. Calcutta: Orient Longman, 1973. Rs 60

The volume contains papers by authors from many parts of the world, collectively intended to commemorate S. S. Sarkar, the Indian physical anthropologist who worked in many fields within the discipline. His interests included the racial affinity of prehistoric and contemporary populations, racial admixture, metric, morphological and physiological traits, twin studies, dermatoglyphics and blood groups, as well as demography and cultural anthropology. The book lists some hundred or so works by Sarkar in these and related fields.

Contributions to the volume cover many of the matters which interested Sarkar himself. Major research interests in physical anthropology during the last decade or so are reviewed by F. M. Salzano. Moreover he suggests that in future, inter-disciplinary research will flourish using many of the laboratory techniques and computer facilities now available for obtaining and analysing data. K. A. R. Kennedy writes of the search for fossil man in India, describing the early lithic industries and some of the skeletal remains found at the earliest in Mesolithic and subsequently in later deposits. D. R. Brothwell outlines evidence for osteogenesis imperfecta in early Egypt and N. Wolanski describes some current research in human growth and development. The investigation of human adaptation in Soviet anthropology is outlined by T. I. Alekseeva whilst J. A. Valsik, R. R. Stukovsky and N. Janku consider factors influencing menarche. M. Weninger provides some remarks on methodological problems encountered in the study of finger and palmar dermatoglyphics. The paper by A. K. Roychoudhury on the determination of gene and chromosome frequencies in blood group data is remarkably clearly presented and useful examples of appropriate procedures are given. A paper on twin studies is provided by Drs Chavez and Roberts since, as they write 'There is great scope for twin studies in Indian peoples'. The much admired work of J. Hiernaux in classifying contemporary man in sub-Saharan Africa cannot be presented in full in this volume but his analysis of the affinities of some thirty-two African populations is eminently lucid. W. S. Pollitzer and R. C. Elston outline and compare various methods which have been used to analyse the relative contributions of parental populations to hybrid ones with special reference to the south-east United States. The genetics of caste in India is considered by L. D. Sanghvi whilst R. L. Kirk reviews the distribution of nine serum proteins, two serum enzymes and fifteen red cell enzymes, all known to be polymorphic in Indian and other populations. Lastly D. F. Roberts writes of genetics and demography noting the wealth of numerical information potentially available for genetic studies of human populations and that quantitative models can be fitted to the data so that the forces of genetic evolution can be estimated. Lists of references follow each paper, there is a general index and brief biographical details of each of the contributors.

It is evident that the papers range widely and most physical anthropologists/human biologists should find something of interest in the book. However many of the contributions are but loosely related, a state of affairs which the editors might have remedied by providing brief introductory remarks to each paper. Yet there is a unity in that the contributions in toto constitute a fitting tribute to the memory of one much respected individual.

E. SUNDERLAND

University of Durham

MARTIN, R. D. *et al.* (eds). *Prosimian biology*; proceedings of a meeting of the Research Seminar in Archaeology and Related Subjects . xxii, 983 pp., illus., plates, tables, bibliogrs. London: Duckworth, 1974. £30

Appreciation of the significance of fossil human remains and of associated human cultures, extending back to the earliest phases of man's evolutionary emergence, depends upon an ability to study man's evolutionary history in the context of that of higher Primates. An appreciation of the latter is, in turn, dependent upon an appreciation of the prosimian radiation lasting during the 30 million years or so of the Eocene from which

the earliest monkeys and apes emerged. A knowledge of prosimian biology thus finds its place in the study of man.

Foundations for the study of the Prosimii were laid in the latter half of the nineteenth century and were continued with the work of such anatomists as the late Sir Wilfrid le Gros Clark and Professor F. Wood-Jones in the first half of the twentieth century But, in general, studies of prosimian biology have tended to be eclipsed by studies of the Anthropoidea and it was not until the advent of an upsurge of interest in primatology as an embracing discipline, that the balance was really redressed. This occurred in the early 1960's, following the establishment of Primate Research Centres in the United States.

The present massive volume, based on a symposium held, in April 1972, at the Institute of Archaeology, University of London, shows the extent of the present interest in this aspect of the study of primate biology.

Following a scholarly foreword by the late Dr W C. Osman Hill, the first group of papers deals with prosimian behaviour, and includes a group of ten field studies of behaviour and ecology together with a slightly smaller number of papers dealing with laboratory studies. Various aspects of olfactory communication are dealt with in a further four papers whilst the next section is devoted to the physiology of prosimian behaviour in captivity.

The second and appreciably larger part of the volume deals initially with general studies of prosimian evolution. These are followed by a large group dealing with form and function in the skull and teeth together with a somewhat smaller one directed towards the morphology of the brain The biomechanics of prosimian locomotion are treated in three papers, while the final, and quite extensive section of the volume deals with modern studies of chromosomes and plasma proteins in the Prosimii and of their relationships to the evolution of the group. All these communications to the original symposium were recast and in many cases extended after discussion, and the whole now forms a carefully edited volume of some fifty papers covering all but 1,000 pages.

It would be easy, but invidious, for any reviewer to single out papers as having special merit; there are many that are excellent and all are good. It would be equally invidious to be exceptionally critical of any that relate closely to one's own field of work. But despite the almost overwhelming size of this compendium and the almost equally bewildering spectrum of topics that it embraces, a general theme emerges: modern studies of the biology of the Prosimii support strongly the pattern of relationship implied in the accepted taxonomic groupings.

But there are irregularities, and the many who continue to regard the work of the late Frederic Wood-Jones with affection and respect and have equally vivid memories of his provocative suggestion (based upon certain aspects of cranial morphology) of a special relationship between *Tarsius* and man, must feel some twinge of delight that in, at any rate, just one group of haematological characters analysed in these studies, *Tarsius* again shows a distinct likeness to man.

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Luckett, W P. (ed.). *Reproductive biology of the primates* (Contrib Primatol 3). xii, 288 pp., illus, plates, tables, bibliogrs. Basel: S. Karger, 1974. £18 80

This volume, dealing with primate reproductive biology, develops several aspects of comparative primatology; in particular, reproductive cycles, gonads and germ cells, and placental development and endocrinology. The tone of the entire volume is an evolutionary interpretation from comparative primatological data. The first of the three sections deals with sexual cycles In it, Butler reviews much of the relevant literature of primate reproductive cycles by major category of primate, from Tupaiidea to Hominoidea, and draws evolutionary connexions. The second section, dealing with gonads and germ cells, covers primate ovary morphology, oogenesis in adult prosimians, and spermatozoon biology. Koering suggests that few good ovary system data exist at this time. Anand Kumar points to the unusual characteristics in some prosimians for the persistence of oogenesis into the adult stages of life. In studying primate spermatozoa, Bedford found some distinctions between prosimians and more advanced primates, as well as some between monkeys and hominoids; these being pointed out in some detail. The third section, on placental development and endocrinology, also contains three papers. Luckett, the volume editor, presents a long discussion of placental development with strong evolutionary and taxonomic overtones. Tullner, in his presentation on chorionic gonadotropins, points briefly to an interesting feature that platyrrhine chorionic gonadotropin production is closer to hominoids than are those of catarrhines. The final paper, by Ryan and Hopper, discusses placental biosynthesis and metabolism of steroid hormones. A common problem, stressed throughout the volume, is the lack of adequate comparative data (many species having been inadequately

studied, if at all). The volume is well referenced and contains many helpful photographic aids. RICHARD A. V. DIENER
University of New Mexico

HIERNAUX, JEAN. *The people of Africa* (Peoples of the World Ser.) xiv, 217 pp., maps, tables, bibliogr. London: Weidenfeld & Nicolson, 1974. £5

This is a good book of its kind, the third of a series on the peoples of the world edited by Sonia Cole; she writes a preface Its author is a well known Africanist with considerable experience in the field.

Counting by chapters (and roughly by pages), his text is in two halves, a general and a special. In an Introduction of some four pages he gives a concise account of the mechanisms of biological evolution; and reminds us rightly that 'evolution is a matter of groups, not of individuals'.

The first half begins with a well-written sketch of the past and present geography of Africa, the stage on which his actors play or have played. We are then given the presumed history that leads us from the hominids to African *Homo sapiens*, whose continent is taken to have been the true and only Eden. A south wind blows us suddenly to the Supra-Saharan zone, whose peoples Hiernaux regards (rather illogically) as immigrants from Asia rather than as returned emigrants, and these are described very briefly. So ends the first quarter.

The rest of the book is devoted to the autochthonous sub-Saharans, who are for the author *les africains proprement dits*. Following a short general account of them that stresses their wide variation, he returns to the factors of biological diversity—phenotypic and genotypic, and then discusses the three modes of classification: biological, cultural and linguistic. From these he proceeds to a fourfold classification of sub-Saharans: Khoisan, Pygmy and Pygmoid; 'Elongated Africans' and Nilotes; the peoples of the Western Sudan and of the Guinea rain forest; and those of Madagascar.

His conclusion is that physical environment has been the paramount factor in determining the wide variation over all sub-Saharan Africa, geographical barriers preventing any considerable admixture from elsewhere.

The book is not for the general reader but for those who already have some knowledge of anthropometric and genetic detail, the latter particularly. It has a valuable list of 235 references to the literature.

It is well produced with few misprints, none serious. But how could a man of the Latin culture ever allow *senso strictu* to remain twice on one page (p. 3)? M. A. MACCONAILL
University College, Cork

Archaeology

JANSEN, GERARD & J.-G. GAUTHIER. *Ancient art of the Northern Cameroons: Sao and Fali* (Anthrop. Pub.). 36 pp., 93 illus., bibliogr. Netherlands: Oosterhout, 1973

It was in 1936 that Marcel Griaule, accompanied by one of his young students, Jean-Paul Lebeuf, undertook the first scientific investigation of the archaeological sites in Cameroon south of Lake Chad. Since then, Professor Lebeuf's repeated expeditions have revealed, besides innumerable burials, a large corpus of terracotta human heads, busts and full figures, animal figurines, and bronzes, ranging in date from the ninth century A.D. to modern times These he has discussed at length, notably in *La civilisation du Tchad* (1950), in collaboration with A. Masson Detourbet, and *Archéologie tchadienne* (1962).

The present volume is in no way comparable with Lebeuf's publications. It is an album of field photographs (environment, physical types, etc.) and of photographs of the Chad material in the Bordeaux and La Rochelle collections. As these are largely unpublished they should be of great interest, but the quality of the photographs themselves and/or the printing is often so poor that it obscures detail and sadly reduces any documentary value they may have.

The introduction by Dr Gauthier (in English and French) is minimal and speculative, as are the notes to the plates For example, plate 33 shows a human head looking upwards in a typical Sao pose, but is interpreted as representing a gorilla, and this is used as evidence that gorillas were once to be found in Northern Cameroon Be this as it may, no gorilla ever sported a bee-hive coiffure.

J B. DONNE
London

ZUBROW, EZRA B. W. *Population, contact, and climate in the New Mexican pueblos* (Anthrop. Pap. Univ. Arizona 24). vii, 87 pp., illus., tables, bibliogr. Tucson: Univ. of Arizona Press, 1974. $4 95

Archaeological, ecological and demographic foci are the interwoven themes of this brief but ambitious study. As Zubrow indicates in his opening remarks, '... this study is not concerned with culture as an undifferentiated totality, but with aspects of culture as they are involved with the process of adaptation' (p. 1). The author proposes to '... isolate and examine in detail a series of population and environmental variables for the New Mexican pueblos [utilizing] organizational and technological considerations as well as environmental and population factors' (p. 2).

The chief value of the monograph lies in its series of excellent aerial photographs

(pp. 27–63), two for each of the nineteen pueblos discussed. We first view the pueblo in its general environs and then are brought in for a close-up of the residential area. Even here, however, there are problems: no compass is indicated and north may appear in any direction (Isleta, for example, is 'upside-down'—for no apparent reason). A second difficulty has broader ramifications for the utility of the study as a whole Zubrow provides us (table 2) with his adjusted population figures for the New Mexico pueblos, 1599–1963 and tells us that he has arrived at these figures through the utilisation of a variety of techniques and data, one of which was as follows:

> In order to arrive at an independent estimate of the population of each pueblo [in 1950], I utilized 1950 aerial photographs and ground maps . . [and] after calculating the total number of rooms per pueblo, I divided the population by the number of rooms (p. 16)

Now, when one looks at the first photographs in the sequence, Santa Ana Pueblo, one sees a small village, relatively compact, which contains according to Zubrow's calculations an estimated 932 people, the Bureau of Indian Affairs, in its 1963 census, lists only 366 residents Further, as Leslie White (1942, *The pueblo of Santa Ana, New Mexico*) points out, the Santanas had abandoned the old pueblo—except during the winter months—as early as 1890 By 1950 the situation had intensified to the extent that Santanas returned to the old pueblo only a few times a year for ceremonial purposes, living the rest of the time in the three settlements which make up their new home, the village of Ranchitos, some 10 miles away (cf, White, pp. 39–41), One would expect, given the nature of Zubrow's study, that such ethnographic data would be important Yet the photograph of the environs of Santa Ana does not show the Ranchitos village and the author does not deal with or even make reference to any of the discrepancies which I have noted It should be emphasised that the Santa Ana situation is not unique—for the pueblos themselves or in terms of Zubrow's superficial treatment.

Granted this is a publication based on his M.A. thesis (which is not really any excuse); granted, too, that he has, in some 40 pages of text attempted to present a broad view of a large topic, one still regrets the precipitous nature of this publication As is too often the case with some of the more recent archaeological work which is being done he casts his net so broadly that large amounts of necessary data are by-passed or elude him.

There are several apparent inconsistencies: table 2 gives a total population in 1660 of 22,250 and aside from questioning the accuracy of these figures I wonder why he shows, in figure 4 a population high in 1680 of 34,500? Why does he conclude (p. 79) that the migratory response was highest during the Anglo period (post-1850) when, again, fig. 4 shows that between 1680 and 1760 the population suffered its greatest decline, dropping to approximately 7,000? The ethnohistorical data would show that this period was a time of great movement, enforced for the most part by raiding Athabascan-speakers and the Spanish amalgamation mentioned earlier.

To summarise briefly my major objections: He does not explain his selection and exclusion of certain materials sufficiently (e.g., why use Bancroft and Hodge for admittedly unreliable data, and ignore Bodine's 1972 study 'Acculturation processes and population dynamics' in Ortiz, *New perspectives on the pueblos*), his simulation model needs more discussion in order to resolve the conflicts which exist between the results which he derives from it and the ethnohistorical and ethnographic data which seem to contradict them, he must treat in a far more sophisticated fashion the concepts of migration and urbanisation; he should support such statements as, '. . [there is] a weakening of the traditional social structure' (p 77); and he must demonstrate that 'decreasing dependence on agriculture' (resumably in terms of actual acreage farmed) really means something in relation to his concern with population dynamics and migration since my own research indicates that, as time spent in agricultural pursuits diminishes quantitatively, the activity's ideational significance qualitatively increases

In short, the inherently strong potential of his analytical model and problem have been vitiated by a work which is too thin, too hastily prepared, and insufficiently researched. Zubrow sums up his conclusions in three brief paragraphs of some 300 words, it is obvious that he could do little more, given the study as presented

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WEBB, WILLIAM S. & CHARLES E. SNOW. *The Adena people;* with a chapter on Adena pottery and a foreword to the new edition by James B. Griffin. xx, 369 pp., illus., tables, bibliogr. Knoxville: Univ. of Tennessee Press, 1974. $10.75

This book is a handsome new edition of one of the major classics in the field of New World archaeology, first published in 1945. The edition carries a detailed, candid and very useful Foreword by Professor James B. Griffith of the University of Michigan.

The meticulous collection, classification and systematic description of a vast amount of data which the volume represents remain impressive as an example of the patience, thoroughness and painstaking detail characteristic of the central tradition of American archaeology. This ensures that despite very important developments in nearly all aspects of archaeological technique since the first appearance of this study, it remains a vital source of information for all who wish to learn about groups of early aboriginal inhabitants of the Ohio River Valley, West Virginia and Kentucky. No less it is of continuing value for those endeavouring to extend the frontier of modern knowledge concerning the Adena people The quality of a culture is to be assessed not least by reference to the nature of its funerary customs, and *The Adena people* is primarily though not exclusively an examination of the complex, sometimes baffling, at times touching evidence of the burial practices of the Adena, especially in Kentucky.

The re-publication of this monument of archaeological scholarship, enhanced in value by the excellent Foreword, and produced in a sturdy yet elegant format worthy of the standing of the work, is thus particularly welcome. As Professor Griffith reminds us, archaeologists will for a long time to come explore and analyse the treasure-house of data contained in *The Adena people* in order to develop their own interpretation of those data beyond that suggested by Webb and Snow, so eminent in their generation, on the basis of their own fundamental researches which in many respects can hardly be superseded The bibliography is comprehensive

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BAUDEZ, CLAUDE F & PIERRE BECQUELIN
Archéologie de Los Naranjos, Honduras (Etud. mésoam. 2). 438 pp., illus, plates, bibliogr Mexico: Mission Archéologique et Ethnologique Française au Mexique, 1973 $23

Los Naranjos, at the north of Lake Yojoa, central Honduras, lies some 80 km east of the sixteenth-century boundary of Maya territory. The Yojoa region was almost certainly Lenca-speaking then and presumably in earlier times. Linguistic affiliations of Lenca are still uncertain; relationship to Maya appear at best remote (E. Wyllys Andrews disagrees). Of Lenca culture very little is known, but it was not advanced.

The site, reported by Doris Stone in 1934, was visited next by Blom and Yde and then by Strong, Kidder and Paul who established that Yojoa polychrome overlay pre-classic monochrome.

Baudez and Becquelin in 1967–69 established this sequence: Jaral (800–400 B.C.) discerning in that period Olmec influences, perhaps better regarded as generalised Middle Formative; Eden I and II, the first, Late Formative corresponding to Playa de los Muertos, the second, characterised by Ulua bichrome, contemporary with early Classic at Copan, the C-14 dates (only three) give the formidable span of 1,000 years for combined I and II; Yojoa, marked by Yojoa polychrome of Strong, Kidder and Paul and generally correlated with Tepeu I and II, but our authors for unstated reasons extend the period to A.D. 950, and Rio Blanco (A.D. 950–1250), early post-Classic, found only in one group at Los Naranjos. A Tohil plumbate jar and a 'Mixtec' bipod and long-handled pottery brazier in a burial suggest sharper dating around A D 1000–1100 and associated Las Vegas polychrome is similarly cross-dated. Research may confirm a shortened Rio Blanco period.

The imposing site comprises several groups of structures on hilly ground scattered over a large area. Group compactness is absent and architecture is largely confined to substructures The group of post-Classic occupation contains two ball courts.

Sculpture is confined to rare, near-naked human (and simian?) figures in the round with bent knees, and a serpent head. The authors recognise Olmec influences in the former; to this reviewer, as to Strong, Kidder and Paul, the associations are with lower Central America, for instance, with statues from Subtiaba, small supporting figures on Costa Rican stone stools, and such late survivals as Veraguan gold figures

Past students of central Honduras have perhaps been over-impressed by the potency of nearby Maya culture; Baudez and Becquelin are concerned to set Yojoa culture in a Central American context, yet, looking over their shoulders at the Maya and even more distant cultures of Middle America, they constantly cite parallels of doubtful significance in the pottery of such sites as Uaxactun, Bilbao and Chiapa de Corzo.

Although Los Naranjos is the largest site in Central Honduras, architecture and artefacts are far from impressive and it might be difficult to defend the authors' classification as a ceremonial centre Seemingly, such places could go their own ways uninfluenced by culturally advanced Copan, not far west, just as Connemara was little swayed by Georgian Dublin.

J. ERIC S. THOMPSON

Saffron Walden

ANCIENT CEYLON. Journal of the Archaeological Survey of Ceylon. 1 and 2, 2 vols., illus., plates, maps, tables, bibliogr. Colombo: Commissioner of Archaeology, 1972

SOLHEIM, WILHELM G. II and S. DERANIYAGALA. *Archaeological survey to investigate south-east Asian prehistoric presence in Ceylon* (Ancient Ceylon, occ. Pap. 1). 40 pp., illus., plates, maps, tables, bibliogr. Colombo Commissioner of Archaeology, 1972. Rs. 12

Many scholars will welcome this new journal, whose aims are set out by the Commissioner of Archaeology, R. H. de Silva, in his foreword to No. 1. The journal is intended to serve as an outlet for the studies of members of the various branches of archaeology united under the department and, at the same time, to serve and appeal to a wider group than specialists. That these two aims are realised in the first two issues without compromising the quality of the journal is a measure of the competence of the individual contributions and the intrinsic interest of the materials they treat.

Ancient Ceylon no 1 has an added value in that three of its papers give a comprehensive review of the state of knowledge on their particular field, thus serving as a foundation for later work These are 'Prehistoric Ceylon—a summary in 1968' (S Deraniyagala); 'The evolution of the technique of Sinhalese wall painting and comparison with Indian painting methods' (R H de Silva), and 'A corpus of pottery forms found in Ceylon' (A Gunasekera, P L. Prematilleke and R. Silva). The other papers take up the study of individual sites and objects rescue archaeology at Kivulegama Vihara (W. B. Marcus Fernando), the medieval architecture in wood of a village rest house (L. K. Karunaratne), the decipherment and translation of the Dakkina Vihara stone inscription (S. Karunaratne), a study in atypical iconography (M. H. Sirisoma) and a survey of Budugala site (L. A. Adithiya) It is good to see photogrammetry and aerial survey accorded their rightful place as valued partners of archaeology (R. Silva and A. D. N. Fernando), as well as a promising note on the general characteristics of the Sanskrit and (more rarely) Pali stone inscriptions of Ceylon (M. Dias).

Ancient Ceylon 2 contains three site reports on the survey and sampling of an early historical, conch shell industrial site at Kollan Kanatta, Vilpattu (N–W coast), now threatened by marine erosion (S. Deraniyagala) pp. 1–17; on the stratigraphical correlations of the Bellan Palassa mesolithic site with freshly excavated skeletal, lithic and ceramic deposits (S. Deraniyagala and K. A. R. Kennedy) pp. 18–47; and finally a full and exemplary report on the excavation and subsequent analysis of the Gedige area of the citadel at Anurādhapura (S. Deraniyagala) pp. 48–169.

The outstanding feature of the two issues of *Ancient Ceylon* lies in the care and consistency with which methods and terminology are defined and then implemented throughout. It is this quality that makes the individual contributions, on widely differing subjects, useful research tools for one another; the same quality ensures their lasting value on a more than local level, making them accessible and relevant to those working on related problems elsewhere. This will be particularly true for those concerned with the archaeology of India and south-east Asia. In the work of S. Deraniyagala, who is most fully represented in these two issues, it is clear that, by taking pains over his definitions—with an eye to recent research not only in Ceylon—he makes great gains in the clarity and succinctness of all that follows. This is no less true of de Silva, Silva, Gunasekera, Prematilleke and the others

These standards suffered something of a decline in Part I of the *Occasional Paper* No. 1 (W. G. Solheim II). Author of many fruitful and provocative hypotheses, Solheim's work in Ceylon was 'to investigate the possible presence and influence of Southeast Asian peoples in Ceylon during prehistoric times' (foreword). Deciding that the lithic materials bore no resemblance to those of south-east Asia from 8,000 B.C. to 1 B C, which he took as his chronological period, Solheim concentrated his attention on ceramics, aided by Miss Mei Mei Burke. He pursued his study of this important subject on the east coast of Ceylon and mentions in his report the presence of incised pottery and paddled sherds and refers often to 'all rim types' being present in a test pit or 'some rim types' only being present. There are three photographs of sherds and a broken numerical series attached to some tiny, poorly drawn figures of rim and lid-rim types. It is unfortunate that there is no statement on the total pottery typology, on the methods he used to distinguish pottery types or what results he obtained from the application of these methods. The average level of pottery detail conveyed in the text is represented by the following passage:

> A kind of pottery was recovered in considerable quantity (48% of total number) from the test pit at the west end of the tunnel, which was only rarely represented in other sites we examined. This is a red and black pottery which is however not the same as the red and black pottery associated with burial jars (p. 7).

Solheim concluded that the results of his

investigations largely negated the hypothesis that peoples of south-east Asia might have had important contacts with Ceylon in the prehistoric period. While admiring the breadth of vision, one might, on the basis of his report, conclude that justice was done neither to the hypothesis nor to his Ceylonese materials.

It remained for Deraniyagala (Part II), to set the material record straight by drawing on the taxonomy of Ceylonese pottery already established by Gunasekera, Prematilleke and Silva, to specify what changes he introduces for the present materials and why. There are full pottery drawings, tables of characteristics, stratigraphical correlates in tabular form, useful maps and a discussion of geological background and faunal associations for each site.

Criticisms are minor and few. Deraniyagala's occasional use of the term 'pre-christian', when he means time B C.; and the need for a long-term policy for the journal regarding metric or non-metric units of measurement. Hints of a piecemeal approach to this problem are not satisfactory, or in keeping with the otherwise admirable standards of the journal

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HEEKEREN, H R. VAN. *The stone age of Indonesia* (Verh. K. Inst Taal-. and- Volkenk. 61). xx, 247 pp., illus., plates, maps, tables, bibliogr. The Hague. Martinus Nijhoff, 1972 Dfl. 72

This is a revised and expanded edition of the author's 1957 book which, together with its companion volume *The Bronze-Iron Age of Indonesia* (1958), provided concise and factual summaries of the various discoveries thought to belong to the prehistoric, pre-Indianising period, in the archipelago Van Heekeren, was, in a sense, rendering an account of the period of Dutch stewardship after the political troubles of the time had forced him to abandon his field research in Indonesia. His approach then was descriptive and typological and if the reader gained only a confused impression of patterns of cultural and economic development in Indonesia the fault was hardly the author's whose aim was to record before it was too late what had been done. Twelve years later van Heekeren returned to Indonesia and, encouraged by the progress which had been made by his former pupil R. P. Soejono and his colleagues, was able to revise and augment by nearly one third, the volume devoted to the Stone Age.

There are two main sections, the one devoted to the environment, fossils and artefacts of the Cenozoic, the other to the post-glacial period, and within these sections, different chapters discuss the various sorts of evidence relating to each of the Lower, early Middle, late Middle, and Upper Pleistocene of Java, the Pleistocene in Sulawesi, a post-glacial mesolithic stage, and the neolithic cultures. The structure is clearer than in the first edition and many new data are incorporated including a report on the first excavation of a neolithic settlement at Kendeng Lembu by Soejono. But the continuing lack of well-dated regional sequences and the great diversity of cultural patterns in Indonesia make any orderly classification by uniform chronological stages too restrictive, as the author readily admits.

Concerning the Pleistocene period, there have been a few new finds, but no basic changes in our knowledge during the past twenty years, and the new potassium argon dates for the Putjangan formation published by Jacob (*Antiquity* **46**: 148, 1972) came too late for inclusion, for 1969 seems to have been van Heekeren's date of completion In retrospect, this seems unfortunate for that year saw the start of new programmes of excavation by Indonesian and foreign scholars and the first publication of C–14 dates from controlled excavations in cave deposits of the post-glacial period. Restricting his subject to the political state of Indonesia also means that van Heekeren has excluded from consideration the considerable results achieved in Sarawak and Sabah by Harrisson, and by Fox and others in the Philippines, results which are very relevant to the dating and interpretation of Indonesian finds.

The author does, however, try to incorporate the preliminary results from excavations by Sørenson, Solheim and Gorman in Thailand which have revealed new dimensions in southeast Asian prehistory, in order to provide an interpretative framework for the yet poorly known finds attributed to a neolithic stage in Indonesia. This book represents van Heekeren's last publication before his sad death in September 1974 and provides a suitable fulfilment of nearly fifty years involvement in Indonesian prehistory. And it is heartening to know that a revised edition of his *Bronze-Iron Age of Indonesia*, nearly ready on his death, will be completed by his former colleagues in Jakarta.

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Social Anthropology

LÉVI-STRAUSS, CLAUDE. *Anthropologie structurale deux*. 450 pp., diagrs., bibliogr. Paris: Plon, 1973

Like its predecessor *Anthropologie structurale* this book is a collection of papers by Lévi-Strauss but published for the most part since

the earlier anthology. During that time of course the reputation of their author has grown tremendously in the English speaking world and acquaintance with his work, once rare, is universal. These two facts mean that it is unlikely that the publication of this book in French will be of quite the same significance as was the case with *Anthropologie structurale*. The reason is simply that most of the papers included here are available in English either in excellent translations as is the case for example with 'The scope of anthropology', 'The Story of Asdiwal', 'Four Winebago myths', 'Race and history' and for several others, or that they were originally published in English as is the case for 'The work of the Bureau of American Ethnology and its teachings' and 'On manipulated sociological models' Nonetheless the gathering together of articles which are not always easily available and the overall pattern which emerges from such a retrospective collection is very valuable It should also be noted that several of the papers have been revised although as far as I could judge always in minor ways

The book is divided in four sections. The first groups together five papers which are tributes to the work of earlier writers and which place French structuralism in the context of the history of thought The first of these is the superb inaugural lecture which Lévi-Strauss delivered on taking up the chair of Social Anthropology at the Collège de France. After having stated what he owes to Durkheim, Mauss, Radcliffe-Brown, Boas, Marx, De Saussure and others, Lévi-Strauss sets out the importance of studying cultures as distinct from ours as possible and the main features of structuralism This is surely the best introduction to the subject and it gives the lie to Lévi-Strauss's reputation for obscurity. There follows a tribute to Rousseau who is depicted by Lévi-Strauss as the founder of anthropology because he replaces Descartes's method which consisted of understanding self in relation to the universe with a method which tries to understand self by his place in nature not directly but via the apprehension of human variety. Then follows a tribute to Durkheim concentrating on the reversal in Durkheim's attitude towards the value of ethnography. The stress on ethnography is again taken up in the chapter on the Bureau of American Ethnology. The section ends with a review of teaching on the religion of preliterate people at the École Pratique Des Hautes Études and a statement of his views on the structural study of myth.

The next section on social organisation consists of answers to criticism. The first is a comment on D. Maybury-Lewis's attack on Lévi-Strauss's famous article on dual organisation. The controversy is significant as it underlines a major theme in all the book; that structure is not the ordering of cultural concepts but an underlying principle which produces this ordering. Maybury-Lewis criticises Lévi-Strauss's manipulations of structures as inappropriate to anthropology. Lévi-Strauss, in answer, criticises empiricism, which according to him cannot explain and leads to endless particularism As an alternative he advocates the use of the notion of transformation, which dominates the book; by this, dissimilar empirical systems are revealed to be variations on an underlying theme. Following this article is one which is the most recently written in the book; it is an answer to criticism of the notion of the atom of kinship formulated by Luc de Heusch Basically the argument is similar to the previous one—that structure is not to be found on the surface. However, it involves Lévi-Strauss in a fascinating and thought-provoking analysis of Mundugomor and Lele kinship categories

The third section is entitled, 'Myth and ritual' but in fact deals almost exclusively with myth. It opens with a discussion of the work of the leading Russian formalist literary critic. This was an early post-revolutionary school which stressed the formal structure of such things as folk tales stressing their recurrence in various guises. Lévi-Strauss is here answering the suggestion made by Mrs Pirkova-Jakobsson that he is really continuing in this earlier tradition, not directly since he was not acquainted with the work of the Russian, but indirectly via the intermediacy of Roman Jakobsson Lévi-Strauss's answer is that although an indirect influence may be present, there is a great difference between on the one hand analysing form simply for its own sake and ignoring content and context and on the other structuralism which sees significance in the interplay between the three elements The next chapters in this section are all studies of myth transformations (usually drawn from north America) of a kind so familiar and so famous that they need no further comment here. The section ends with an essay in a similar vein entitled 'How myths die' If the reader is expecting an answer to the vexing question of how to decide when two myths are *not* transformations one of another he will be disappointed. Rather surprisingly according to Lévi-Strauss myths die when they change functions becoming romanesque tales or merely charters (the contrast with Malinowski here is fascinating)

The fourth section entitled 'Humanism and humanities' is something of a residual category It contains pages on the place of anthropology in the history of humanism, remarks on modern art and on economic development. It is remarkable that on all these topics Lévi-Strauss succeeds in being exciting and

thought-provoking. The section is however dominated by a reprinting, with modifications, of his pamphlet 'Race and history'. Although the title is misleading this is one of the boldest of Lévi-Strauss's works. His argument is simple: evolution is not dependent on cultures moving towards a similar state but on the contrary is dependent on the presence of variation whether internal to a culture or variation between cultures. It is isolation and uniformity which leads to backwardness.

It is difficult to say anything very general about so varied a collection but if we compare it with the earlier *Anthropologie structurale* two aspects are coming to the fore which were not so evident previously The first is the insistence on a distinction between a surface level and an underlying level which it is the task of structural anthropology to uncover, so that apparently dissimilar empirical systems can be seen as transformations one of another. This being made so clear here, it is not surprising that so many anthropologists are worried at the absence of a clearly and rigorously defined method for plunging from surface to depth. Another consequence of this insistence is that Lévi-Strauss's claim that his method derives from structural linguistics becomes less and less tenable. It perhaps parallels better transformational grammar or even generative semantics which Lévi-Strauss would then have foreshadowed

The second new emphasis is on nature. It was never quite clear where the primacy given to the opposition nature/culture came from, in this book, especially in the pieces on Rousseau and modern art, it is clear that it came from a deep *a priori* notion that man's task, whether it be through science or art, is to put himself in accord with nature from which all truth derives. This near pantheism may be a viable metaphysical notion but I doubt whether it is an anthropological one.

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FIRTH, RAYMOND. *Symbols public and private.* 469 pp., bibliogr. London: Allen & Unwin, 1973. £6·60

This book is intended 'neither as textbook nor as a comprehensive general work' (page 9) Professor Firth's treatment of symbolism is meant to be historical, extensive and popular, but this does not provide a wholly accurate description of the contents of the book. One is reminded once again of how astonishingly well-read the author is, and of how objective he is in the use of his material. For all these good qualities, the book is none the less a disappointment. It has all the defects of its virtues. It is too long, and it tells us too much about too many things of secondary importance. The impression is of an inadequately edited scrap-book, kept up from a time before the first hijacking was even thought of (p. 303), and providing for the literary detective a faithful record of its author's travels from one university campus to another. There is too much padding. 'Dying of tuberculosis in 1793 at the age of 35, Moritz was much concerned ...' also leaves one to wonder about sentence construction. The balance is wrong, too. 'I find it impossible to summarize Creuzer's general treatment of symbolism, but a few of his leading ideas can be indicated' (p. 99). What could be better calculated to make the reader expect a 'funereal parade of yawn-enforcing facts' (to quote from Kingsley Amis's *Lucky Jim*), especially when it is two pages later before Professor Firth finally lets Creuzer go. And this for a book not meant to be a 'comprehensive general work' (p. 9). In contrast to the well-favoured Creuzer, Bateson's later studies of symbolism, in the field of mental disorder, although of 'cardinal importance', are dismissed without any further discussion (p. 162) Arguably, of course, the reader will already know all about Bateson, and next to nothing about Creuzer, but one does begin to suspect that by the time the author had made it from the early 19th to the mid-20th century, he was himself becoming a little weary. In the end one is left remembering chance facts, such as that 'the human head has about 120,000 hairs' (p. 263), or that flags were 'transmitted to Europe probably by the Saracens' (p 330). There is an awful temptation to be wiser than Professor Firth, who does not seem to know, for example, that the flags of Holland and Luxembourg are identical (Chapter 10).

Every now and then there does occur the sort of insight to be expected from Professor Firth· '... charisma [is] the ability to get private symbols publicly accepted' (p. 239). Or right at the end (p. 428) '... a demand for acquiescence in the absolute validity of [a] symbol is a political demand, even when it speaks in the name of a religious system.' But however good the theme of the book, its development quite fails to establish Firth as a name in the study of symbolism, to be compared with Douglas or Turner, or, outside anthropology, with Jung or Bateson This is a pity. Professor Firth's scholarship is impeccable. Without focus, or any recognisable bias, it is simply not memorable.

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EKEH, PETER P. *Social exchange theory· the two traditions.* xvi, 237 pp., bibliogr. London. Heinemann, 1974. £4·50 (cloth), £1·80 (paper)

The book proposes a marriage between Homans and Lévi-Strauss, no less. That may

appear the mere fantasy of an imaginative broker attempting the impossible *coup*; but Dr Ekeh is so expert in his analysis of the American, so sympathetic to the Frenchman, so convincing in his sketch of the lineaments of the progeny, the reader could wish the protagonists had no right to refuse their consent to the union.

The central problem is the need for an exchange theory which explains both the pursuit of valued ends and the creation of value, the creation of social solidarity. Ekeh argues that Homans, if he could abandon his ideological commitment to a conservative theory of justice, can provide the former, Lévi-Strauss, gently brought to admit the existence of *Zweckrationalitat*, can provide the latter. Ekeh persuasively identifies Homans's (and Blau's) dyadic exchange with Lévi-Strauss's restricted exchange; and their indirect exchange with his generalised exchange. There are difficulties, but these are explored rather than brushed aside, with a firm hold on the end in view

In his approach to that end Ekeh conducts a masterly analysis of Homans: he teaches that it is integral to Homans's life and to his notions of justice, exchange, of sociology even, that he should claim Mauss as his sole precursor—having at that time (1958) not only read *Les structures élémentaires de la parenté* but published a critique of it Ekeh reconstructs a Homans without pigeons and decomposes Durkheimian social solidarity into elements which admit individualistic social action

The book is clearly written, often witty, it is a pleasure to read a sociological work which has such a good grasp of anthropological data, and in which acute critical powers are used so creatively It is nicely produced, and the footnotes are where they should be Because Ekeh gives exchange theory only a limited place in sociological theory, we must hope that he will linger for a while he should report on the child.

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SAHLINS, MARSHALL *Stone age economics*. xiv, 348 pp., illus., tables, bibliogr London: Tavistock, 1974 £4·50 (cloth), £1·35 (paper)

Any anthropologist who has postponed reading this book should do so at once; likewise, any non-anthropologist who chances to be reading these words; its implications concern us all.

The book contains six essays. Two together are a new twin birth; three reincarnate existing papers on a higher plane, and one is a familiar ten-year-old presented unchanged. The title of the book—glossed by the introduction—explains the link between them. They have a common aim, 'to perpetuate the possibility of an anthropological economics by a few concrete examples'. Stone Age for three reasons; first, the societies studied are, roughly in the stone age; secondly, substantive economics is as yet rudimentary science, whose 'intellectual weapons are the crudest choppers, capable only of indelicate blows at the objectives and likely soon to crumble against retractory empirical materials' (p 277); thirdly, Sahlins's puzzles are about evolutionary processes and he has no inhibitions about stages and ages; man goes forward and upward Lesson One about primitive economics, we all know, is that economic relations are embedded in a messy tangle of other social relations; beginning with this not unchallenged truism, Sahlins sides loudly with 'substantivism' and against 'formalism'.

The first chapter enlarges an article published in French in 1968. Hunters and gatherers, it turns out, do not normally work very hard, in relation to their wants, they mostly enjoy plenty with no great effort. Two major ideologies seem to need revising. First, in no sense of the word 'essence' is man essentially either a property owner or a worker. Pre-neolithic men had all they wanted, because what little they wanted was there for the taking. The exploitation of man by man (or person by person), and scarce means chasing infinite needs are the invention of civilisation, not part of human nature. Secondly, the techniques of the neolithic revolution did not replace human effort, nor increase productivity per hour They made it possible and worthwhile for people to work harder; they increased productivity by increasing incentives to work. The crucial changes were not changes in efficiency, but in goals and needs defined by new social organisations, which the new revolutionary techniques made possible. And the new needs were not always the producers' own needs, the needs were often those of affines, trading partners, specialists, or political masters. Civilisation created the virtue of industriousness.

The case is elegant and cogent, but is the emphasis right? Affluent perhaps in a special sense. But people still faced storms, droughts, plagues, fights, and with no means of defence except ritual, their own largely ineffectual symbols. They died frequently and they died young. Not that Sahlins wants to equate hunters with the good society; he is only saying they were not 'poor'.

I found the two chapters of the domestic excursion the most exciting. Obviously all societies, except 'modern' ones, rely almost

entirely on the domestic mode of production (DMP). Sahlins argues that such societies normally underproduce in three ways: they are always well below the demographic carrying capacity of their territory; they under-use their labour power; and many households fail to produce enough to meet their own survival needs. The domestic group, he argues, has a small labour force with a single main division of labour—that of sex; it has a simple technology and it has 'finite production objectives'. When these socially defined standards are easily achieved, people make less effort. This leads him to 'Chayanov's rule'—that in any domestic unit the better the ratio of workers to non-workers, the less the workers work. That is, domestic productive intensity is inversely related to productive capacity. Left to itself, the DMP is inclined towards maximum dispersion, domestic units avoid other domestic units—it is inherently centrifugal.

In the next essay, Sahlins argues by the use of data, graphs and calculations that it is possible to set a formal measure on the capacity of a given social system to drive people to work. The DMP is inherently centrifugal and highly inefficient, but kinship and politics provide mechanisms for holding households together, for encouraging surplus, for making sure that stronger households carry weaker ones. Progress is by exploitation. Sahlins is not afraid to speculate, and he sets his readers speculating, but the whole argument is grounded in detailed data.

An essay on Mauss, by an American admirer and critic of British anthropology, written under the eye of Lévi-Strauss himself, promises much, and does not disappoint. Sahlins examines in meticulous detail the text of Elsdon Best which Mauss was using. From this, like Mauss, he claims to produce ideas of general validity. First, embedded in the words of the Maori informant is the common primitive moral principle that advantage or profit must be acknowledged by a return. Not two, but three parties are involved. The benefit which I derive from passing on your gift to me to someone else, belongs to you, the original giver. In other words, exchange motivated by the prospect of subsequent profit is morally wrong. The Shade of Mauss may rejoice in the paradox that, although he misunderstood the unsatisfactory ethnography and his arguments were open to objections, he nevertheless hit on a profound insight.

The fifth chapter we have met before (in ASA 1). It illustrates and extends the notion that gifts are peace, with a detailed classification of the correlation between modes of reciprocity and social distance. And for good measure, we have a hypothesis about the conditions which make for the invention of primitive money—namely an unusual incidence of balanced reciprocity in peripheral social sectors. People use money to move goods about—reciprocally. They do not (until merchants and mercantilism) move goods about in order to make money, non-reciprocally. After such insights, it is niggling to object to 'negative reciprocity' as a nasty neologism (but I do), and a pleasure to remind you of the forty fascinating and exemplary pages of ethnographic examples, with comment, that conclude this chapter.

The final chapter, 'Exchange value and the diplomacy of primitive trade,' is perhaps the most specific. It begins with the need for 'anthropological economics to complete its theory of value with a theory of exchange theory' (p. 277). Sahlins admits that in the system of trade in Melanesia, the formalists are right—exchange rates do respond to supply and demand in spite of some appearances to the contrary. However, in primitive societies there can be no competition between buyers and other buyers, sellers and other sellers, therefore markets, as arenas for competitive price setting, are universally absent from primitive society. Exchange is peace, partners are politically indispensable, and people are generous since exchange is always precarious But wherever unlike is exchanged, norms of commensurability must exist, and it is these which adapt as supply and demand conditions change.

Sahlins is illustrating, yet again, that economic transactions are embedded in and sanctioned by multiplex social relationships so that the economic flexibility of his trading systems depends on the social structure But even so, what goes on does 'reasonably reflect the availability and utility of goods'... 'The conclusion ... for the whole book is that a primitive theory of exchange value is also necessary, and perhaps possible—without saying it yet exists' (p. 314).

This book is outstanding and enjoyable. Its excellence appears to be the fruit of six years of full time research out of twelve years of academic life. The text demands close concentration, and I found it hard both to follow Sahlins and keep up with the speculations he stimulated. Though detailed and technical in places, it is always clear, succinct, and it flowers with memorable sentences It is not tortuous, pompous, obscure, pretentious or axe-grinding. Sahlins seems to think in related spurts, and the themes, problems and insights cross and re-cross in the six separate essays. It is the reverse of a treatise—new ideas, old ideas from new angles, hares started in all directions.

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ROSALDO, MICHELLE ZIMBALIST & LOUISE LAMPHERE (eds). *Woman, culture and society*. xiv, 352 pp., tables, bibliogr. Stanford: Univ. Press; London. Oxford Univ. Press, 1974. £7·25 (cloth), £2·30 (paper)

It is the contention of the editors of this collection of articles that until very recently, western social scientists have followed their own culture's bias in treating women as relatively invisible and uninteresting, being content to describe mainly the activities of men, and that inevitably anthropology, like the other social sciences, has suffered from a failure to develop theoretical perspectives that take account of women. There can be few who would quarrel with this reasoning and little doubt that this book will be widely welcomed as helping to fill what is at present a yawning gap

The sixteen articles are all written by young American women anthropologists whose theoretical interests are as diverse as their geographical ones—the articles draw on fieldwork among the Navajo Indians, communities in Sierra Leone, Nigeria, Cameroons and Tchad, the Sherpas of Nepal, the Minangkabau of Sumatra, and societies in Taiwan, the Philippines and Yugoslavia

Although there is no common approach, inevitably many of the articles attempt to answer, at least in part, some of the basic questions concerning the study of women: what features of all known human societies have permitted them to produce and reproduce an inegalitarian sexual order? Are there societies where women are relatively more equal than others? Why do women accept a subordinate standing? How do women exercise power?

The first three articles are concerned with why it is that social groups, which change radically through time, nevertheless continue to recreate a social order dominated by men Rosaldo argues that an emphasis on women's maternal role leads to a universal opposition between 'domestic' and 'public' domains that is inevitably asymmetrical Ortner's paper also posits a dichotomy—between women and 'nature', and men and 'culture', with the former negatively, and the latter positively evaluated. Chodorow seeks to answer the question by an examination of the differential significance for boys and girls of the socialisation process, which in most societies is allocated to mothers; while the girls follow in their mothers' footsteps, the boys have to reject their mothers at some point, to learn 'masculine' values and behaviour

The second part of the collection is concerned with the nature, implications and extent of female power, with articles by Collier, Lamphere, Stack, Tanner, Wolf, Hoffer and Sanday. These papers emphasise that women, as well as men, are social actors whose own goals and strategies are as vital to the processes of social life as those of men; they thus challenge the conventional sociological viewpoint that women are not only uninteresting, but also unimportant.

The four articles which follow use the method of comparison in order to try and isolate crucial variables which determine women's status—Sanday uses statistical material from a variety of societies, Stack examines evidence with information from four east and south African societies, Leis makes a controlled comparison of two west African societies, and Denich compares pastoral with agricultural people in the Balkans.

The last three papers concentrate upon the way in which ideology is used as a means of keeping women in their place. Bamberger analyses myths of women's former dominance in some South American societies, while Paul and O'Laughlin show how the mystification of women's body processes among the Indians of Guatemala and the Mbum of Tchad creates an aura of 'nature', and therefore of inevitability, to women's secondary status.

The editors suggest that this collection shows that sexual asymmetry means different things in different places—women vary widely from society to society in their social roles and powers, and these variations can be accounted for by particular social and economic factors; however, women's activities are everywhere far more socially significant than anthropologists have previously allowed.

Since this book represents a pioneering effort, it might be expected that it would exhibit many of the deficiencies of a first attempt. In fact, this is far from being the case The articles are of a uniformly high standard, and it would be difficult to single out any as being more instrinsically interesting than the others. Although it is the hope of the authors that their contributions will stimulate much more work in this field, it is plain that this book will stand on its own merits for a long time to come.

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WILLIS, ROY. *Man and beast* (Approaches to Anthropology Series). 143 pp., map, bibliogr. London: Hart-Davis, MacGibbon, 1974. £2·95

This is the first in a new series, 'Approaches to Anthropology,' which 'aims to illustrate the concern of anthropologists to those in other disciplines and to a wider public'. It concludes that 'The distinctive peculiarity of animals is that, being at once close to man and strange to him, both akin to him and un-

alterably not-man, they are able to alternate, as objects of human thought, between the contiguity of the metonymic mode and the distanced, analogical mode of the metaphor. This means that, as symbols, animals have the convenient faculty of representing both existential and normative aspects of human experience, as well as their interrelation; what is beyond society, the ultimate ends of action, and the incorporation of such values in the structure of social perception and relations' (128–9). Such a conclusion seems true but commonplace in its glib generalities. In the narrowness of its approach, the present work fails to provide a clear or comprehensive picture of the ways various cultures symbolise animals and makes little attempt to relate the issues to other fields, not even such obvious ones as psychology, aesthetics, philosophy or literary criticism. Nor does it show how some of these disciplines have contributed to anthropological understanding of how men symbolise. The layman would be better served by a survey of the past research on this topic, a consideration of the basic theories involved, and some suggestions as to what these imply not only for anthropology but for others To do this would be a difficult but exceedingly valuable task involving some of the most interesting figures and issues in cultural anthropology. Unfortunately, the author gives little indication of how long such problems have fascinated researchers (as far back in anthropology as McLennan, Frazer and Smith) and ignores some of the most perceptive and influential researchers currently writing on this subject. The works of Tambiah, Horton, Turner, Rigby, Forge, Munn, Buxton and Bulmer are never mentioned while Leach and Firth are relegated to uninformative citations in one footnote each. None of the work on peoples outside Africa is seriously considered other than by way of a few trendy but superficial references to Lévi-Strauss and a passing reference to Klingender's tedious and unreflective, though richly documented study of animals in Western art.

Willis confines himself to brief and shallow examination of three African societies: the Nuer of the Sudan, the Lele of Zaire, and the Fipa of Tanzania. He characterises each of these in terms of one paramount theriomorphic theme: the Nuer venerate the character of the ox, the Lele the pangolin, and the Fipa the python. Such a view is simple-minded and distortive. The material published on the Fipa is too scant to allow any confident statement about the complex relation between their cosmology and their social organisation. The Lele material, while better, is far less substantial than for the Nuer, yet even when dealing with that superbly reported society, Willis considers only a very small portion of the relevant publications and neglects entirely those aspects which may pose difficulties for his interpretation. Unscholarly treatment of material mars most of the book.

A few examples from the Nuer illustrate some of these weaknesses. Willis maintains that Nuer 'think scientifically and abstractly about wild animals whereas they are too deeply involved with their livestock to think in such a manner about their cattle (14). He associates cattle with culture, stability and society, and wild animals with nature and the external, inessential and accidental (115). It is not at all clear why this alleged preoccupation by Nuer is confined to their cattle when sheep and goats are held in large numbers and probably are vital to Nuer subsistence. Nor does Willis discuss the ritual in which sheep and goats figure. In any case, Willis provides little analysis of Nuer concepts about wild animals, apparently because he has already assumed that they are of relatively little significance. We are certainly not able to learn from him that the elephant, associated with certain female kin, is the only wild animal to have a soul. Nor do we learn that certain other creatures are associated with important spirits or with prophets. Or that pythons are associated with bees and figure in some ritual, even though pythons appear in the non-Nuer sections of the book.

Much of Willis's discussion hangs on his interpretation of Nuer concepts of twins as birds which, by a series of shaky arguments, he sees as the key to understanding Nuer concepts of duality. A few of his points are suggestive, but too much is omitted to give one confidence Why are like-sexed twins less dangerous than cross-sexed ones? Why are ground-loving birds associated with twins if it is the high-flying aspects of birds, as Willis maintains, which is the prime quality making them a quintessential symbol of spirit? In any case the entire topic of birds is poorly covered. Why is the pied-crow associated with leopard-skin priests and used to remove pollution from homicides? Why is it associated with certain air spirits? Why are ostriches associated with an air spirit? Are they even considered birds? Willis has ignored most of the interesting Nuer rituals and beliefs associated with wild creatures. Nuer thought is probably far more complex and perhaps more heterogeneous and inconsistent than this simplistic book suggests.

Much discussion of the Nuer is devoted to relating their social organisation to their symbolisation of animals. Unfortunately, the author often confuses assertion of a relationship for the demonstration of such a connexion. The Nuer are contrasted repeatedly with their neighbours, the Dinka, who are described as less well-organised, less cohesive,

and less socially stable than the Nuer. Their views of animals are also implicitly contrasted. It is not entirely clear how these questionable judgements are reached. Lienhardt's study of Dinka religion is often cited, as are the Nuer's many disparaging stereotypes of the Dinka, recorded by Evans-Pritchard. (Traditional enemies are hardly the best sources for such evaluations.) Unfortunately, none of F. M. Deng's four recent books on the Dinka, two on social and political organisation, two on oral literature and its imagery, is mentioned. Much in these excellent volumes goes against the kinds of characterisations Willis makes, suggesting a society more centralised and cohesive than that of the Nuer. Certainly Nuer and Dinka views of animals could be discussed, and contrasted usefully, for there is now considerable published material.

Finally, it is annoying that a book concerned with animals, should render all scientific terms improperly (genus should be capitalised, species not).

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Barkun, Michael. *Disaster and the millennium.* x, 246 pp, bibliogr. New Haven, London: Yale Univ. Press, 1974. $10.00

This book wobbles in the wake of Norman Cohn and Anthony Wallace in a series of solipsist interpretations which, while not without interest, add little to what has already been said. As is only too common with generalist writers, Barkun relies mainly upon other generalists and takes scant notice of those empirical studies which, carried out over the last fifteen years by both anthropologists and sociologists, have done so much to alter the received interpretations of millenarian-type activities. But then, had Barkun looked a little more closely at such studies he could not have written this book.

The familiar material is parcelled together—it is difficult to say '*intellectually* contained in'—by 'disaster', 'paranoia', 'religious ecstasy', and that other good old portmanteau standby of a sociology which fails to give an accounting, 'rational/irrational'. For Barkun 'disaster' is not, as in fact it is, a *logical* requirement, but a sensible physical occurrence. Consequently, 'disaster' has to be extended into the almost meaningless and comes to mean anything that happens to precede a millenarian movement. As anything other than a generally descriptive term, 'paranoia' is surely outmoded. Further, as we now know, since 'paranoids' are almost invariably correct and accurate in their assessment of their ambience, 'paranoia' begins to look suspiciously like one of those words by which the privileged seek to locate the activities of the deprived in psycho-physiological rather than moral, political or social lesion. With this said, 'religious ecstasy' and 'rational/irrational' become mere appendages, much more clearly the politically whiggish refuges they always were.

It is simply not the case (p. 1) that '... these movements flicker briefly in the wake of disaster'. They often occur in good times, in times getting better, and they do not 'flicker briefly'. The 'sixties in Europe and North America were times of spreading affluence—did Barkun not note the increase in millenarian-type activity? It is not the case (ch. 3) that these movements are more or less reserved to rural conditions and cannot take place in the city. And so on and so on. Simple errors of this kind stud the pages, and the views and thoughts of other contributors to the field are mangled and twisted in the interests of a framework which is wholly inadequate to what actually happens.

I wish I could say that the book was as entertaining as *Chariots of the gods*. Alas! It is not. Imagination and intellectual thrust are dulled by a scholarly apparatus hanging precariously on 'paranoia'. No wonder, then, that the superb work of Johannes Fabian goes unmentioned, that judgements can be delivered on the nature of Melanesian cargo cults without recourse to Peter Lawrence. Without that addiction to 'paranoia' it might all be rather baffling.

A disaster but no millennium.

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Claessen, H. J. M. *Politieke antropologie: ein terreinverkenning* (Terreinverk. cult. Antrop. 1). 154 pp., illus., bibliogr. Assen: Van Gorcum, 1974. Dfl. 18.75

This the first book in a new series covering different themes in social anthropology, and clearly intended to provide short general surveys for Dutch first-year university students. The choice and ordering of the material is admirable, although, in the present climate of social science studies in Holland, it is remarkable how little attention is given to Marxist ideas. Holland has, surprisingly enough, as many teachers and students in anthropology as the United Kingdom, so this new study should not want for readers, even though the price is much too high for a short paper-back. The book is however so directed towards its own limited readership that good as it is, it would have little appeal outside Holland, even in an English translation. This is a pity, since there is in fact no English alternative which is as good.

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LESSNOFF, MICHAEL. *The structure of social science: a philosophical introduction* (Stud. Sociol. 7). 165 pp., bibliogr. London: Allen & Unwin, 1974. £3·60

This is a textbook, written by a lecturer in politics at the University of Glasgow, and it would seem to be directed in the main at first-year students of sociology. It contains six chapters dealing respectively with: physical science, minds and social science, regularities, explanation, functionalism, and value-judgements. The bibliography is useful, and there are subject and name indexes.

The author's progress through the topics is orderly, with special stress laid on the parts concerning physics and statistical method, his coverage of the issues is wide and fair, and his exposition is clear. Naturally, there are points that provoke some dissent (to begin with, the word 'structure' in the title is as sounding brass: it makes a bold ring yet signifies nothing), but the impression conveyed overall is one of thoroughness and reliability. Taken at its own level, the book will presumably be found appropriate to the pedagogic aims it is designed to serve.

Yet any further justification of the enterprise is not self-evident, and it is rather a puzzle that a capable and knowledgeable writer should choose to devote himself to still another textbook of this kind. As the bibliographical references indicate, there are already many publications in this area, including Alan Ryan's quite recent *The philosophy of the social sciences* (1970), and there is no obvious intellectual need for another. Or at least, if there is indeed a continuing demand for introductory texts, perhaps this style of work is no longer the most effective way to meet it.

A characteristic defect of the approach adopted in *The structure of social science* is that it leaves the student at a cold distance from the real work, and the occasional excitement, that makes up the scientific investigation of social facts. It is from the practice of a 'social science', e.g., social anthropology, that philosophical problems arise (whence the place occupied in philosophical commentaries by Evans-Pritchard's writings on the Zande), far more than from the lucubrations of methodologists. The immediacy and the particularity of such an empirical setting, in which the investigator has actual problems to solve, needs to be conveyed to the student if he is to grasp what the problems are problems about. Mere generalities, however sound and carefully formulated they may be, can give only attenuated guidance to students of sociology, and they deprive philosophers of precisely those minutiae that they must have for any serious purpose of conceptual analysis.

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AUSBIDA, G. *Strutture sociali e contenuti culturali.* 286 pp., bibliogr. Milano: Cisalpino-Goliardica. 1974. L. 4,000(paper)

The first chapter of this book proposes that everything, from the humblest crystal to the most complex social organisation, vibrates and is therefore susceptible to scientific analysis. The next few chapters provide a scrappy survey of theories of social evolution, education, communication and social structure. For example: '. . . with modern methods of recording and displacement of communication in time and space roles can continue to operate even while their "animators" are temporarily absent. Which is the same as saying that social structures composed of roles have independent life, even if one may expect that roles vacated for too long may atrophy . . . Of course those roles which are deeply rooted in the past last longer: if the role of Pope should remain empty, it would endure intact for many years, probably' (p. 100). The central chapters use a formula of thermodynamics to analyse social energy (which is manifest in the following categories of activity; moving, structural activities, acquiring prestige, biological activities, technology, generation, self-expression, vibration); and in the last 100 pages the insights gained are applied to the neolithic revolution, early urbanisation, the migration of Indo-Europeans, social solidarity, and the future—including the prospected acculturation of monkeys.

The book is a phenomenon: an evolutionary theory without a single reference to Marx; a thermodynamic theory which cites Durkheim's *Rules*, a book about structure defined as 'a net of roles' which can be drawn to show their disposition in space, whose author acknowledges the encouragement of Lévi-Strauss. It is ignorant, complacent and lightweight.

J. DAVIS
University of Kent, Canterbury

OPPONG, CHRISTINE (ed.). *Domestic rights and duties in southern Ghana* (Legon Family Res. Pap. 1). vi, 382 pp., tables, bibliogr. Legon: Institute of African Studies, University of Ghana, 1974

This useful collection of essays edited by Christine Oppong provides ample reassurance that kinship studies in Ghana are alive and well. It also testifies to the influence of the two eminent scholars who have put Ghanaian kinship systems so prominently on the anthropological map. The first part of the book devoted to Akan studies bears a decidedly Fortesian imprint; the third part on comparative studies indicates the influence of Jack Goody, and throughout the book there is a pronounced dependence on surveys and statistical methods—even in the studies by

lawyers and specialists in drama and literature. Adopting a mildly dialectical tone, Fortes and Goody introduce sections of the book, the former stressing the moral and jural significance of kinship and the latter sticking to his familiar material view. In a rather awkward epilogue, Simon Ottenberg throws in his lot with Goody. Some contributions are inventive and illuminating, notably Hardiman's paper on the role of women in Akan communities, Oppong's account of the economic aspects of elite conjugal relationships and Bame's view of domestic morals through the plays of the strolling 'concert parties'. However it is rather disappointing to find that the issues pursued are generally very familiar. The 'matrilineal puzzle' runs through the Akan studies and it is disconcerting to find a parallel 'patrilineal puzzle' preoccupying the writers on Ewe kinship This is a rapidly expanding and soundly Ghanaian area of study; Fiawoo's account of parallel cousin marriage and Nukunya's discussion of bilateral elements are certainly interesting but it is a pity that Ewe research should not have thrown up some more lively theoretical problems. The papers are undoubtedly an inter-disciplinary collection, but there is not much evidence of real collaboration No one takes up Ansre's efforts at a comparison of Ewe and Akan kin terms, or Fletcher's illuminating analysis of house structures. Are Ghanaian studies at risk of becoming too parochial? There is not a whisper of Lévi-Strauss, which will not please one particular type of anthropologist However, this book must be warmly welcomed as the first in a promised series of 'Legon Family Research Papers'. It is to be hoped that when further papers have appeared Dr Oppong will publish a judicious selection of them in more accessible form

A. F. ROBERTSON

University of Cambridge

MACLEAN, UNA. *Magical medicine a Nigerian case study*. 167 pp., bibliogr. Harmondsworth Penguin, 1974. 50p

This book was originally published in hardback in 1971, and is now re-issued (without the pictures or any corrections) in the Pelican Health and Medicine series. The 'magical medicine' is that of the Yoruba in the town of Ibadan. Written by a lecturer in social medicine, the book is mainly for people in the medical services there. Though the subject is important, an anthropologist would not, I think, find this account particularly useful.

There is a certain old-fashioned style to the text (Dahomey for example is called 'neighbouring French territory'), a style which I found slightly patronising but the first edition's blurb called 'amusing'. Phrases like 'a very convincing performance', 'cheerful charlatan', 'relatively sophisticated concepts' indicate the author's own standpoint. It is not surprising then that though much emphasis is placed on concepts, we are told almost nothing of the symbols and systematic thought behind Yoruba medicine: for example there is no account of how a Yoruba analyses his body and its workings. Similarly, the author is interested primarily in patients' behaviour, yet the only account of such behaviour is from questionnaires administered by students to heads of households and to schoolchildren. Further, although I agree with Dr Maclean that 'the enduring value of African medicine lies not in its materials but in its methods and the concepts that underlie them', the pharmacology of at least some of the vast Yoruba (let alone African) materia medica could have been discussed in detail. For not all Yoruba medicine is 'magical'. And where, too, are the bone-setters?

But the medical people for whom this book is intended will, I am sure, find it useful and entertaining, and it will, as Dr Maclean hopes, dispel some European preconceptions about African 'witch doctors'. I am only sorry it does not do full justice to Yoruba thought.

MURRAY LAST

Salisbury

DAMANE, M & P. B. SANDERS (eds, trans). *Lithoko· Sotho praise poems* (Oxf. Libr Afr. Lit). xvi, 289 pp., map, bibliogr. Oxford Clarendon Press, London: Oxford Univ. Press, 1974. £6·50

The existence of a tradition of lengthy and complex panegyric poetry by Southern African Bantu poets has been known for many years to specialists in this region and, in a general way, is increasingly coming to the notice of students of comparative oral literature. Until recently, however, it has not been easy for English-speaking readers who are not specialists in this region to gain access to this poetry or to have the opportunity to appreciate its artistry and poetic effectiveness.

The Oxford Library of African Literature has recently been serving us well in making this poetry available to a wider public. A whole series of volumes has appeared over the last ten years: I. Schapera's *Praise poems of Tswana chiefs*, 1965; Trevor Cope's *Izibongo: Zulu praise poems* 1968, perhaps the treatment by M F. Morris of a similar poetic genre from a not too far distant area in his *The heroic recitations of the Bahima of Ankole*, 1964 (all these three volumes have original texts, translations, substantial introductions and, for the most part, copious notes). Most recently have appeared two volumes on Sotho praise poems—D. P. Kunene's analytic discussion of

poetic style and structure in *Heroic poetry of the Basotho*, 1971, and finally the present volume.

The scholarly and attractive volume by Damane and Sanders is a worthy addition to the series, and usefully complements Kunene's more analytic treatment. In this collection, two Sotho historians—one of them (Damane) also a linguist—have presented fully annotated translations of the Sotho texts earlier collected and published by Mangoaela and Lerotholi (plus a few others). The result is a volume of praise poems in translation, accompanied by very full notes based on the authors' research travelling through Lesotho and consulting men accepted as authorities on the poems. It would, ideally, have been useful to have had the original texts included too; but since these are available in published form, this omission is not unreasonable (the few texts not previously published are printed in an Appendix). The introduction gives a broad view of the historical and cultural background, and there is a particularly good section on the poets themselves and on the occasions of composition and performance—aspects often omitted in collections of this kind.

The editors stress the military associations of Sotho praise poetry and this is well borne out by the warlike tone of many of the poems they give—like this passage about the hero in one of Masopha's praises:

> As the battle caught light and burned,
> And became almost a fiery blaze,
> In went the falcon of Mokhachane,
> In went the falcon, the bird of prey,
> This bird of prey of my master.
> As it went in its eyes were glaring,
> Its teeth were bared in its open mouth,
> Its teeth were bared in its gaping mouth;
> As it entered it spoke to the Hawks,
> Saying to the dogs: 'Devour them, they're there!'

Altogether this is a most useful volume which can be recommended both to specialists (whether in the geographical region or the study of oral literature) and to all those with a general interest in the poetic expression of the human imagination and its role in society.

RUTH FINNEGAN

Open University

JOURNAL OF SOUTHERN AFRICAN STUDIES. Vol 1, No. 1. 136 pp. London: Oxford Univ Press, 1974 £4 p.a.

A journal devoted to southern African studies is welcome not only because of current uncertainties in this part of the continent, but because southern Africa has epitomised opposing views on race relations for more than a century. The intention of its editors is to draw material from students of all the social sciences and 'the natural sciences in so far as they relate to the human condition'. Its initial number is promising.

In the field of economics Martin Legassick criticises from a marxist viewpoint the alleged liberalisation of South African policy with the development of secondary industry, and Charles Harvey discusses the significance of British investment and the question of what influence the presence of British firms has (or their withdrawal could have) towards the amelioration of the conditions of African labour. History is represented by a reinterpretation of the development of mining in Rhodesia (I. R. Phimister), literature by an examination of the racial attitudes expressed in the novels of Sarah Gertrude Millin (Michael Wade). There is news of conferences on southern African studies, and of the foundation of a centre for post-graduate work at the university of York, where a library of archival material and current government publications is being built up.

The plum for anthropologists is John Comaroff's article on Tshidi (Tswana) chiefs in the 'homeland' of Bophutatswana. At the moment when Africans further north were almost unanimously rejecting 'indirect rule', the Bantu Authorities Act resuscitated it for South Africa. Comaroff shows how the apparently clear rules of succession used to be questioned after the event if a chief was unpopular, a way of denying legitimacy which is no longer available when his position is guaranteed by external authority. As elsewhere, the rigidity that has to accompany inclusion in a formal national constitution is killing chiefship, and the Tshidi regret this, for they still value it as the symbol of their cultural identity. Moreover, tribal gatherings used to provide a permitted forum for political discussion and a means for nationalist leaders to recruit followers. The control that goes with recognition stifles this kind of debate. There is irony in the recollection that thirty years ago progressive opinion pronounced that chiefship was a thing of the past; it seems that there will always be a new place to observe it.

LUCY MAIR

London

MYERHOFF, BARBARA G. *Peyote hunt the sacred journey of the Huichol Indians* (Symbol, Myth, & Ritual Ser.) 285 pp., illus, bibliogr. Ithaca, London: Cornell Univ. Press, 1974. £7·50

The Huichol Indians of Mexico, who live in small settlements in the Sierra Madre Occidental and in the cities of that region, are among the Mexican Indian groups that still make religious use of peyote. Small groups of

Huichol pilgrims undertake a ritualised, arduous peyote quest, travelling 170 miles to a sacred area north of San Luis Potosi to acquire a year's supply of peyote, although alternative sources are nearer at hand. Myerhoff presents an interpretation of the ritual hunt, based on her fieldwork in the 1960's. Drawing principally on Victor Turner's approach, she views the journey as a 'unification of opposites', a symbolic return to the golden age of unity *Communitas* is sought through ritual reversals and in other ways; contradictions between the Huichol's every-day life of agricultural work and the freedom of the hunt are resolved; status differences of shamans and followers, men and women, and young and old are dissolved. The ritual expresses the symbolic unit of deer, maize, and peyote, all of which stand for the Huichol themselves and for their ideal life. Based both on observation and participation, and on translation of a distinguished shaman's explanations, the work is valuable for an understanding of the Huichol and of Mexican peyotism.

Myerhoff takes some pains to explain why the symbolic emphasis on deer, hunting, and the free male world of hunting is so marked, even though today the Huichol hunt rarely and do not depend on game Her explanation rests on a doubtful piece of historical reconstruction. In her view the Huichol are migrants from the San Luis Potosi region, and ultimately from somewhere near the Pueblo Indians of the southwestern United States. They were originally desert hunters, who began to depend on maize horticulture 'relatively recently'—that is, perhaps a thousand years ago (pp. 56–8). She believes that the ritual hunt reflects their incomplete transition from hunting and gathering to horticulture. Yet the Huichol resemble many other tribal horticulturalists of Mexico and the American Southwest, who depended on both horticulture and hunting during a period of one or more millennia. As in these other groups, Huichol ritual reflects this mixed dependency—for maize, too, is prominent in the ritual, as Myerhoff notes. As in other instances, Huichol hunting has much decreased in importance, probably in the past fifty or one hundred years. In the reviewer's opinion, it is plausible and probable that the ritual emphasis on hunting reflects the recent importance of hunting in a mixed economy, rather than a remote past of entire dependency on hunting and gathering (cf. Alfonso Ortiz, *The Tewa world*, for one of several such cases). As for Huichol resemblances to the Pueblos, their generic resemblance to other Mexican and southwestern horticulturalists is also evident (cf. Ralph L. Beals, *The comparative ethnology of northern Mexico before 1750*). The reviewer is sympathetic towards historical reconstruction, but in this case there is a need for more systematic methods and a less selective use of comparative evidence.

Although one could wish for a description of Huichol peyote ceremonies at home, between peyote hunts, the work stands as an interesting interpretation and a fine record of an important ritual.

David F. Aberle
University of British Columbia

Hughes, Charles C. *Eskimo boyhood: an autobiography in psycho-social perspective.* 429 pp., illus., bibliogr. Lexington: Univ. Press of Kentucky, 1974. $12.75

This autobiography is based upon a Saint Lawrence Islander's essay, in the English language, written mainly while hospitalised for tuberculosis while in his twenties. Hughes respects the privacy of the actors by changing the names throughout the autobiography, calling his main subject Nathan. However, when the reviewer was on Saint Lawrence Island, Nathan's true identity was readily perceived and he was shown the book (Nathan had no knowledge of the publication of his essay, and in fact has not seen or heard from Hughes in twenty years). He showed great interest in the material and was pleased it had been published; his tape-recorded comments were utilised by the reviewer in assessing the merits of the work (and a copy has been sent to Hughes).

The book is highly readable, the first section (1–380) resembling an adventure story for boys and being in simple language. The final section (381–429) consists of a psycho-social analysis of the material couched in psychiatric terms, and therein lies a dilemma the ideal readership of the second section is liable to be irked by the tone and pace of the first, and vice versa. Providing one can comfortably reconcile the anomaly, the two sections are complementary and each is well done. For instance, Hughes is good at reading significant anthropological meaning into the boy's simple statements such as that the children present at a meal may be there in place of a beloved deceased relative (114). Hughes relates this to reincarnation beliefs.

Hughes' role analysis is tripartite: (i) the apprentice hunter; (ii) the schoolboy; and (iii) the son. It is thorough and informative, but, seeing that the book covers ten years of the subject's life (it takes until page 391 to discover this fact), more consideration should have been given to that role which relates a male Eskimo adult to the rapidly changing White-dominated society of the nearby mainland. Hughes probes such a relationship only in connexion with the role of the schoolboy.

One tends to doubt that 'Behavioural alternatives are nonexistent for Nathan; the "occupational" structure is a mono-valued one' (400). The assertion is challenged by the fact that, unbeknown to Hughes, Nathan later went back to high school for several years as an adult and entered commerce as an accountant. As native crafts teaching aide in a high school, he more recently escorted a class from the mainland in order to demonstrate native boatbuilding skills. In all fairness it should be added that Nathan is again an ivory-carver.

Contradictions occasionally occur. The book's main fault is the amount of generalisation from minutiae. In his concluding analysis Hughes makes much of the well-known Eskimo hostility-avoidance (396), failing, however, to distinguish between indigenous norms and the effects of zealous Christianisation. Nathan's admission that 'the poor bird, hanging by its nose, wildly fluttering to free himself ... bothered me very much in my heart' (129) is probably more an example of Christian morality than of Eskimo values. Such admissions may have been for the benefit of the Christian reader, for whom Nathan must surely have known the essay was ultimately destined. As he himself stated in his 1974 interview with the reviewer, in subsistence matters the Eskimo has little choice and such an incident is relatively morality-free.

Although Nathan was a church-goer, Hughes nowhere explains the youth's moral verbalisations in terms of an acquired Christian morality. Is it not possible that Nathan was influenced by his choice of language, his English surely being that of the Christian teacher and the Christian minister? Hughes, who includes Sapir in his bibliography, surprisingly ignores the language-thought relationship pointed out by Sapir and by Whorf, merely stating that 'the life story was written (in English) at my request' (page 3—Hughes's parentheses).

THOMAS F. JOHNSTON

University of Alaska

ARCHER, W. G. *The hill of flutes: life, love and poetry in tribal India · a portrait of the Santals.* 375 pp., plates, bibliogr. London · Allen & Unwin, 1974. £5·95

The author of this well produced and lavishly illustrated volume belongs to the dwindling band of British scholars who held administrative positions in pre-Independence India, and in addition to bearing heavy official responsibilities developed a deep insight into the cultural and social life of the people in their charge. At a time when it has become fashionable to denigrate the achievements of colonial rule, it is salutary to read a book such as *The hill of flutes*. The penetrating understanding of the mentality and values of the Santals, whom Archer got to know when he was Deputy Commissioner at Dumka, is clear proof of the emotional and intellectual involvement with local populations experienced by many British members of the Indian Civil Service. If the quality of government depends on the sympathy between rulers and ruled the administration of men such as W. G. Archer, W. V. Grigson, J. H. Hutton and J. P. Mills will certainly stand up to the judgement of history. Their achievements, obvious to anyone who saw them in action, are evidence of the value of an anthropological approach to practical tasks, and of the benefits both anthropology and administration can derive from the merging of the two interests. Nowadays few anthropologists have the opportunity of staying for several years among the people they study, and hence there are few studies of an intimacy matching that of Archer's account of the Santals.

The hill of flutes is not a conventional ethnographic description of a section of the Santals, a people more than two million strong, but a picture of Santal life derived both from their own poetry and the author's observation. The poems and songs, which form an essential part of the book, are the key to the understanding of the Santals' emotions, and Archer uses them most skilfully in illustrating the human relations which are the central theme of his study. With the exception of Verrier Elwin, no other anthropologist dealing with an Indian tribe has given as detailed and vivid an account of the interaction between parents and children, men and women, and indeed between the individual and village society. The rules governing interpersonal relations emerge clearly from the wealth of case-histories, and though in practice some deviation from the ideal modes of conduct is tolerated, the sanctions at the disposal of tribal society are adequate to deal with all breaches of the moral code. Particularly interesting is the custom of *bitlaha*, a ceremonial demonstration against unrepentant offenders staged by an enormous crowd drawn from several villages The climax of this demonstration is the invasion and defilement of the culprit's courtyard. The aim of this ritual, which has many elements of sexual symbolism, is to shame culprits into submission, and the Santals themselves regard it as a hunt in which the offenders themselves are the quarry. Having observed many *bitlaha* Archer is in a position to describe them in detail and analyse also the attitude of the participants. By exposing the culprits to defilement and public ridicule this ritual is the most dreaded of all sanctions, and one by

which the tribe asserts itself as the supreme body regulating the conduct of its members.

A large part of the book is devoted to detailed descriptions of sexual attitudes and customs, and poetry as well as myths is quoted to elucidate the Santals' view of sex. They emerge as a people of tremendous vitality and *joie-de-vivre*, and their lack of inhibitions in discussing sexual subjects would seem to have made it possible to gather very detailed information on topics treated far less fully in the majority of Indian anthropological writings.

The gift for artistic appreciation, on which W. G. Archer's reputation as one of the foremost critics and interpreters of Indian visual arts is based, has enabled him to render Santal poetry into elegant and yet simple English, entirely appropriate to the genius of a preliterate and yet profoundly artistic people. The Santals are fortunate in their chronicler just as Archer was fortunate to spend some of the best years of his life among so attractive and amiable a people.

C. VON FURER-HAIMENDORF

SOAS, London

FÜRER-HAIMENDORF, CHRISTOPH VON (ed.). *Contributions to the anthropology of Nepal*; proceedings of a symposium ... [vi], 260 pp., illus., maps, tables, bibliogr. Warminster: Aris & Phillips, 1974. £6

Like population, anthropology in Nepal is growing exponentially. This multi-ethnic collection describes 'work in progress' and is consequently very uneven in approach and quality. Shamanism is the dominant theme. Allen describes the 'ritual journey' among the Rai of east Nepal in a perceptive piece of ethnography. Hitchcock analyses a shaman's song from the Magars of central Nepal and sees it in ambiguities found in generalised marriage exchange, reference to sexual ambiguity and evidence of the shaman's ambiguous status. Gaborieau analyses songs in western Nepal and concludes that they reflect a real tension between an older style of arranged marriage and more recent 'love' relationships. Höfer writes generally on spirit possession, asking why such possession is a specialist activity in the Himalayas while it is open to the laity in India. He then describes shamanism among the Tamangs, applying linguistic models to the shamanistic texts. Michl describes shamanism among the Chantel (copper miners). Sharma contributes to the ethnography on the 'Divinities of the Karnali Basin in Western Nepal'. Greenwold investigates the problem of whether 'monkhood' is an inherent characteristic of Buddhism and by what standards Newar 'Buddhists' are to be judged. The ethnographic description of a Thakali clan ritual by Jest complements the film shown at the symposium.

Two papers are concerned with kinship and caste. Aziz argues that the stereotype of agnatic descent in Tibet is mistaken; Tibetan society was effectively bilateral, based on households and kindreds. Caplan tests hypotheses of Leach and Dumont concerning the significance and prevalence of caste endogamy by looking at inter-caste marriages in a west Nepalese town. It is the only paper to study the modern sector of Nepal and to use simple statistics. Two other papers discuss economic change. The editor discusses the effects of changing patterns of trade and increased monetisation, particularly the closing of the Tibetan salt trade, on the high-altitude Bhotia people. Messerschmidt and Gurung compare the innovatory techniques of Gurung and Thakali 'big men' and attempt a historical reconstruction of one legendary figure. The other three contributions consist of rather broad ethnographic accounts. Fournier describes the Sunwar and points to their complex religious life. Frank outlines an attempt to count the distribution of ethnic groups, particularly the Tamangs. Oppitz summarises his work, published in German in 1968, on the clan history of the Sherpas and reconsiders problems concerning the boundaries between myth and history.

For those interested in shamanism and in the Tibeto-Burman field this is a useful work. It omits many important areas, for example politics and power, agricultural economics and the major effects of economic development, formal demography and medicine, social control and administration. Yet it provides a good cross-section of some of the major fieldwork recently completed in the area. It is doubtful whether, in the present form, much of the material is of more than local interest. But there are clearly going to be some important contributions in the future based on the work reported in a preliminary way here. The editor is to be congratulated for having achieved the difficult task of international co-operation which led to the conference and to this volume.

ALAN MACFARLANE

University of Cambridge

PASTERNAK, BURTON. *Kinship and community in two Chinese villages*. xvi, 174 pp., illus., plates, maps, tables, bibliogr. Stanford Univ. Press; London: Oxford Univ. Press, 1972. £4·25

In combining his two village studies from southern Taiwan into one monograph, Pasternak has produced a lively if somewhat awkward book. To read the ethnography of

the Hakka village of Tatieh, for instance, one would have to find the relevant pieces of the various chapters on agriculture, kinship, and village integration, each of which compares Tatieh with Chungshe, a Hokkien village in the Chianan Plain. What is lost in ethnographic clarity is perhaps gained in polemical force, but a synoptic lay-out might have provided the reader with an option of reading the two studies seriatim or interwoven.

The book brings together information and argument from a series of papers already published in various places, and it provides a well-written, well-integrated, and thoroughly interesting comparative treatise that attempts to explain the differences it discovers. Briefly, Pasternak finds that Chungshe exhibits the strong and pervading agnatic lineage organisation one would expect to find in a southern Chinese village, while the inhabitants of Tatieh stress kinship far less and organise their collective activities through non-kin and even community-wide associations

At first glance, the comparison looks far too uncontrolled to provide useful insights into Chinese rural social organisation, but Pasternak marshals the evidence carefully and examines four possible explanations for the difference Urbanisation and the Japanese occupation can be discounted because their influences have been equivalent on both villages. Ethnicity is a little more difficult to reject as an explanation, but Pasternak makes a strong case against it by pointing out that some Hakka villages (such as Chong Village in the New Territories of Hong Kong studied by Jean Pratt) closely resemble Chungshe, while some Hokkien villages (such as Hsing Hsing and K'un Shen studied by Bernard Gallin and Norma Diamond respectively) are much more like Tatieh.

This leaves the nature of settlement as the most powerful explanation, and here the argument questions one of Maurice Freedman's suggestions that Chinese lineages are a response to frontier conditions necessitating co-operation in defence and opening new land. Pasternak suggests that quite the opposite is the case: lineages appear in those places where settled conditions permit fission into agnatic groups, while the contingencies of the frontier require village solidarity that cuts across or embraces all agnatic groupings

From Pasternak's evidence, Tatieh (the Hakka village) appears to exhibit social organisation typical of *overseas* Chinese communities, while Chungshe (Hokkien) looks like a village indigenous to southeastern China. Principles of clanship—along with locality and interest—are more important in Tatieh than the agnatic kinship ties of lineage (Pasternak should read Meyer Fortes!). Furthermore, the Hakka in Tatieh came later than the Hokkien, they were ethnically foreign to both Hokkien and aborigine settlements, and they maintained individual links with lineages in their mainland provenance (p. 133), despite over two centuries in Taiwan. It would be interesting to discover if Tatieh Hakka and Chungshe Hokkien differ in their view of Taiwan's relationship with metropolitan China; do they see themselves as in China itself or overseas?

The absence of Chinese characters is disappointing and difficult to understand, given the growing list of titles in the sociology of China that Stanford University Press is publishing. Some of Pasternak's translations seem whimsical: 'happiness master' for *fu-chu* (p. 110), 'Make Prosperous Limited Shares Corporation' (p. 100) for ... what? It is absurd to refer to the period prior to the large-scale irrigation scheme on the Chianan Plain as 'pre-Chianan', a phrase that will not be found in the author's earlier article on the subject.

W. E Willmott

University of Canterbury

Stevens, Frank *Aborigines in the Northern Territory cattle industry*, with drawings by Bruce Petty (Aborig. Aust. Soc. 11). xviii, 227 pp, illus, tables, maps, bibliogr. Canberra: Australian National University Press, 1974. £5·30

Frank Stevens is probably best known for his editorship of the excellent three-volume collection, *Racism: the Australian experience* (1971). The present book reports on a rather limited survey—a series of semi-structured interviews with employers of Aboriginal labour in the cattle industry, carried out primarily in 1965. This was a crucial time for race relations in the Northern Territory, the 1965 Arbitration Case allowed Aborigines at last to obtain for the first time the right to equal wages. This was also the beginning of industrial action by Aborigines which led to the strikes at Wave Hill and surrounding areas, and to the Gurindji land claim.

The book is clearly written and well-organised, and provides a mine of information on the often confused and contradictory attitudes of white cattle-people in the Territory. One reason for Stevens's success is doubtless that he is not an anthropologist, and therefore does not consider it beyond his professional duties to study the whites, whose presence, life-styles and values are an integral part of the current Aboriginal world. As he himself notes, the contribution of the anthropologists has not been a helpful one. The advocates of employers' groups in the 1965 Arbitration Case made much of anthro-

pological evidence (as indeed did Justice Woodward in the Yirrkala Land Rights Case) and in both instances this evidence was decidedly in favour of white interests and against the Aborigines'.

Cases such as this highlight the extreme difficulties of anthropologists working in rapidly changing circumstances, where statements published as part of academic study may be taken out of context, manipulated, and presented as conclusive evidence to anthropologically untrained decision-makers. But Stevens's criticisms go further than this, I think unjustifiably so. He concludes his preface with an attack on Elkin, 'the doyen of Australian anthropologists', and makes the decidedly heavy assertion that 'the erroneous basis of the Elkin analysis [of race relations] was, in fact, one of the major causes for the failure of Aboriginal policy in North Australia' (p xii). This seems a remarkable contention; it is unlikely that the influence of any one anthropologist on such a complex socio-economic situation could be so enormous, and when one considers the many pleas Elkin made for sweeping changes as early as 1934, all of course ignored, it is hard to see how the blame for the whole sorry situation can be laid at his door. (Consider for instance his 'Anthropology and the future of the Australian Aborigines', *Oceania* September 1934, where he pointed out that Aborigines had the same capacity for change as anyone else, but that they needed autonomy and some participation in decisions for any social change to be meaningful or effective Further, he advocated that any administration should have powers of control over the actions of frontier pioneers, as well as over employment and conditions of Aborigines in the outback)

Finally, there is wide agreement these days that the anthropologist/fieldworker has at least some obligation to protect the interests of the people he is working with, or at least not publish material which will cause them loss, hardship, distress etc This is fine when the people are poor, oppressed, or in other ways minority group 'victims' of a powerful majority. But where does the obligation lie when research is conducted among the oppressors rather than the oppressed? The great strength of Stevens's study is that it deals with whites in power situations. Reading between the lines of Stevens's book it seems he gained the co-operation of station managers by at least implying a sympathy to their cause yet it is perfectly obvious from the text where his real sympathies lie, who is the exploiter and who the exploited. To what extent is it ethical to protect the interests of poor informants but condemn the interests of rich ones? As anthropology becomes more 'relevant' such questions will become more pressing, and will have to be answered.

ANNETTE HAMILTON

Macquarie University

BLOK, ANTON. *The Mafia of a Sicilian village, 1860–1960: a study of violent peasant entrepreneurs*; with a foreword by Charles Tilley (Pavilion Ser.). xxxiv, 293 pp., illus., plates, maps, tables, bibliogr. Oxford: Basil Blackwell, 1974. £5·25

Professor Blok has written an unusual and valuable study. It is unusual since the book is concerned with the fate of a social category of persons for the century ending one year before the actual fieldwork and archival research began. Evans-Pritchard for the Sanusi, and M. G. Smith for the Hausa Emirates both made this form of historical research an honourable one in anthropology. It is simply that today anthropologists are embracing history so ardently that we can expect many monographs in the style of Anton Blok's, one only hopes that they will match him in clarity and interest.

Through the judicious combination of archival research with observing and interviewing the right people, Blok can reconstruct the emergence of a particular social configuration, and then account for its relative strength or weakness in successive periods. As we approach living memory, the technique becomes closer to the extended case history employed by Manchester-trained fieldworkers and the ethnographic detail richer. But at no point is Blok attempting the portrait of a village, and there is scarcely a word about the community between 1961–67, when Blok was doing his 30-months fieldwork in Sicily In this quite basic way, he has reversed the usual emphasis of much recent social anthropology. I hope he will in the future redress the current imbalance, but what he has done so far must be warmly welcomed.

Mafiosi are power brokers who arose when the Bourbon state sought to curb the post-feudal land-owning aristocracy and encouraged peasant emancipation. But this state did not manage the successful monopoly of force in the Sicilian hinterlands, and the rising aspirations of the peasantry were held in check by the leaseholders and estate guards (*gabellotti* and *campieri*), themselves mobile peasants whose interests were most directly threatened, in alliance with the major latifundists, and local notables. Violence was their main instrument of control, and power relations a key factor in analysis.

Blok thus depends chiefly on an approach through modes of production (*latifundia*, peasant tenures, and other agrarian occupations) taken with the rise and fall of specific classes and interest groups. The local situation

is always interpreted in the context of its changing relation with the encompassing nation-state, the growth of representative politics, the penetration of market forces and more recently, the international market for Sicilian labour migrants.

It is worth noting the conceptual eclecticism of Blok's analysis: although prefaced with tantalising extracts from Marx and Barrington Moore, the author draws freely from F. G. Bailey (the broker model, the encapsulating state, leaders, followers and resources) Norbert Elias (the process of state-formation) Eric Wolf (approaches to patron-client relations, and peasant rebellions) and others. This range of sources does not offend me, but it is worth noting since there are those who insist that, for example, Marx and Bailey cannot be both right at the same time—indeed such a view is increasingly the topic of journal articles. The book is none the worse for rejecting a single pure vocabulary, except where one feels authors are cited merely to establish anthropological credentials; but given Blok's departure from existing 'village study' orthodoxies, he may have been wise to take those precautions.

It is refreshing to read another Mediterranean monograph which makes only three references to the otherwise ubiquitous notion of 'honour'—Sicilians talk more of men being 'respected', which in plain talk means *feared* Thus a whole onion skin of cultural obscurity is shed. I doubt if the way Blok has identified his sources will satisfy historians, and indeed even by the much less demanding standards of most anthropologists, it would have been helpful to know all through the book which kinds of sources were being used (oral or written) and when. But this is a small point, against the very real achievements of the author.

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General

IVIMY, JOHN. *The sphinx and the megaliths.* x, 208 pp., illus., maps. London: Turnstone Books, 1974. £2·95

In this book the author, an English civil servant, stresses a theoretical combination of reincarnation and diffusion in presenting his thesis that the world's first civilisation was founded in the fifth millennium B.C. in the Euphrates Valley by a pre-Sumerian group headed by a unique leader—a demigod who possessed extraordinary mathematical skills, acquired in another incarnation when he sojourned with the 'Divine Intelligence' (p. 99) in a purely mathematical universe.

Groups of men then set sail for far-flung places, taking with them the knowledge that had been given them by their founder.

Members of this 'dynastic race' (p. 30)—distinguished by their superior physique and intelligence—settled in Egypt where they consequently built the Great Pyramid—an edifice designed for calendrical purposes and as an astronomical laboratory.

Members of the Egyptian priesthood then journeyed to Salisbury Plain in order to build Stonehenge—a replica of the Great Pyramid in that it, too, was designed for scientific purposes.

Gradually through the centuries this abstruse knowledge was lost, only to reemerge again in the architectural design of the Mormon Temple at Salt Lake City, Utah—a structure built under the direction of Brigham Young who acquired his abilities in a previous incarnation.

If this book could be dismissed simply as an entertaining historical novel, then the author's excursions into fantasy could be attributed to literary imagination.

But alas, one suspects that because of the sophistication of his mathematical presentations, he is addressing a scientific audience.

If this is so, then his premisses are subject to the same rigorous scrutiny one would accord any other scientific writer.

If Stonehenge were built by an Egyptian priesthood, intellectually superior to the local inhabitants, why do we not find physical evidence of their presence in England?

Also, the fact that Stonehenge is not the only nor the oldest megalithic structure in England and adjacent areas suggests a somewhat widespread tradition, continuous over a period of time. His outrageous explanation for this phenomenon is that these other structures were built by immigrants from the Euphrates Valley.

Finally, he reluctantly admits that Renfrew's revision of radiocarbon dating for Europe may make 'Stonehenge I contemporary with, if not slightly earlier than the early pyramid builders in Egypt' (p. 82) thus negating one of his basic premisses—diffusion.

Fortunately, or unfortunately, as the case may be, his other premiss, reincarnation, is untestable and lies in the realm of 'pure speculation' (p. 2) as he himself admits.

The most damaging aspect of this book, however, is its similarity to Erich von Daniken's, *Chariots of the gods*? Here is the same appeal to racism, bigotry and the quasi-scientific intellect.

To paraphrase King Solomon, there seems to be no end to the making of such books.

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SEALE, MORRIS S. *The desert Bible: nomadic tribal culture and Old Testament interpretation.* x, 223 pp. London: Weidenfeld & Nicolson, 1974. £3·95

The dust-cover describes the author as 'an orientalist, teacher and missionary who has spent forty years in Syria and Lebanon. He has made a special study of Arabic and Islamics, Sufism, Hebrew and Old Testament literature and related theological and philological subjects'. On the evidence presented here, however, his reading has lain within a very restricted field.

His basic thesis is that English translators and commentators of the Old Testament have taken inadequate account of the fact that 'the Hebrews began as a nomadic people' and have 'generally speaking, failed to study Old Testament literature in the light of the cognate language and literature of the ancient Arabians, themselves a Semitic people of desert origin'. The book ends with suggestions for 'fifty odd corrections' to the accepted text based on an examination of such parallels.

Professional anthropologists with an interest in this field are not likely to be greatly impressed Nomadic, urban and rural populations have been living in symbiotic relationship to one another in the vicinity of Palestine for the past 10,000 years and there is no basis for Dr Seale's apparent assumption that the 'original' texts of the Old Testament represented a kind of folk history of an exclusively nomadic society.

If the English and American Christian scholars mentioned by Seale have paid only marginal attention to Arabic-Nomadic parallels of Hebrew tradition this can hardly be said of Biblical commentators in general. For example, the bibliography appended to Julian Morgenstern's *Rites of birth, marriage, death and kindred occasions among the Semites* (Hebrew Union College Press/Cincinnati, 1966) runs to about 400 items and contains scores of books and articles which are concerned with parallels for Old Testament texts of precisely this kind. Indeed Robertson-Smith's two major classics, *Kinship and marriage in ancient Arabia* and *Lectures on the religion of the Semites* were anchored around this very theme. Neither Robertson-Smith nor Morgenstern is mentioned in Seale's pages!

Most of Seale's textual adjustments are quite trivial, several seem fanciful, and at least one (the amended translation of Hebrew *rekeb* in Isaiah 21:7 from 'chariot' to 'troop') already appears in the Revised Standard Version of the English Bible of 1885! Biblical specialists will not be embarrassed.

The whole technique is of dubious merit. For example, in the main text of the book (p. 181) Seale devotes a single paragraph to commentary on the prohibition against shaving the forelocks in mourning in Deuteronomy 14:1, and mentions one, not very relevant, Arabic cross-reference. The comparable text and notes in Morgenstern (pp. 105–6) refer to twenty-one 'authorities' in English, German, French, and Latin besides cross-references to various Arabic and Hebrew sources, including six other references in the Old Testament itself. Even so, neither Seale nor Morgenstern is able to provide convincing evidence as to why or whether ancient Hebrews or ancient Arabs did or did not shave their forelocks in mourning!

In other words, this book is very lightweight of its kind, and the kind is, anthropologically speaking, very old-fashioned and unrewarding.

EDMUND R. LEACH
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LA BARRE, WESTON. *The ghost dance: origins of religion.* xviii, 677 pp. London: Methuen, 1972. £6·95

The Ghost Dance was a religious movement among North American Indians in their defeat and despair in the closing decades of the nineteenth century. The Indians who were caught up in the movement believed that if they performed the correct rituals their dead kinsmen would come back to life, the white men who swarmed about them like locusts would disappear, and their cultures would once again be valid.

This book gets its title not because it is about the Ghost Dance—only one chapter is devoted to it, with passing references elsewhere—but because Weston La Barre says that it was through reading James Mooney's classic study of the Ghost Dance that he gained an understanding of the fundamental nature of religion. The subtitle would have been a far more appropriate title because the book is in fact an anthropological and psychological enquiry into the origin of religion. It is a big book, running in excess of 600 pages, and this represents only about one-third of the original manuscript. It is impressively documented with references in several languages from many fields of study, including psychoanalysis, human biology, social and cultural anthropology, theology, comparative religion, history, and classical literature It is lofty in tone, polysyllabic in vocabulary. It is obviously the product of a lifetime of reading and research.

But for all these outward signs of scholarship the book will be a disappointment to most contemporary social anthropologists. In several respects this work resembles anthropological inquiries into the origin and evolution of religion which were written fifty or more years ago. Like the anthropologists of that era, La Barre's emphasis is decidedly on the bizarre—cannibalism, scalp-taking, head-

hunting, hallucinogenic drugs, human sacrifice, odd sexual practices, and so on. He draws heavily on the ethnology of the American Indians not on any theoretical or methodological grounds, but simply because they are examples of mankind that he happens to know about—savages are grist for the anthropologist's mill. Moreover, he sees the American Indians as 'Mesolithic fossils' which can be used to reconstruct Judaeo-Christian prehistory, implying that savages are not so important in themselves but rather as means for discovering our own past.

His explanation of the origin of religion is straightforwardly psychoanalytic. Religion is a projection of man's experience in the nuclear family and of man's 'neoteny'—i.e., man is intrinsically infantile, constitutionally unwilling to accept reality. God is only a shaman's dream about his father; religion is a group dream or nightmare; god is a function of man's psychobiology. La Barre's main addition to Freud's theory of religion is that each religion is the outgrowth of a 'crisis cult' or Ghost Dance. That is, when a society is faced with a problem which surpasses its technical means, men regress as infants to the palliative of a defence mechanism—a 'frantized, autistic solution'—i.e., they devise a religion. It is through their mothers that men become magicians, it is through their fathers that men come to believe in gods. Magic differs from religion in that the former is psychologically more infantile than the latter.

Freud has been called the high priest of the individual, but La Barre accords the individual an even higher place than did Freud in proposing that only those theories propounded by individuals are worthy of our attention. He dismisses schools of thought with a wave of the hand. 'Whatever a crowd does is uncritical and therefore suspect, and, if not potentially dangerous, at least intellectually stultifying' (p. 283). If a man is kind and good, it is his individual life-experience that makes him that way, and not the moral teachings of any group to which he might belong. A man in a sect or congregation, or presumably in any kind of group, lets others think for him.

La Barre is also more determinedly hostile towards religion than was Freud. Religious beliefs are nonsense; theology is nonsense; metaphysical questions are merely quibbles about 'linguistic behavior'. Religion is obsessional neurosis writ large; only through the rigorous application of scientific method can a rational individual escape its clutches. He is especially down on Christianity: '. . . the New Testament is explicitly a hopeless ghost-dance tangle of conflicting ethnologies and culture heroes over which Christians can argue endlessly' (p 609). Paul was a neurotic homosexual, and Christianity is the legacy of his personal sickness.

Like Freud, La Barre not only uses his theory to explain everything he chooses to explain (e g, Nazism was a nativistic cult; Mao's Red Guards exhibited 'cult elements'), but also like Freud his theory has a built-in defence against disproof. Namely, he contends that anthropologists who are uncomfortable with characterisations of their own or other religions in terms of psychopathology are either naive or are themselves victims of some neurosis which prevents them from seeing the truth. (Perhaps it is just their neoteny coming out) Although this book is primarily about the world views of other cultures, one senses that La Barre's commitment to his own world view—psychoanalytic theory—is so strong that he has never ventured much outside it. If he has, it doesn't show.

CHARLES HUDSON

University of Georgia

Erratum

Man (N S) 9, 338, 'Bornu' should read 'Borgu'

BOOKS RECEIVED FOR REVIEW IN *MAN* JANUARY–MARCH 1975

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THE JOURNAL OF THE ROYAL ANTHROPOLOGICAL INSTITUTE

NEW SERIES, VOL. 10, NO. 3 SEPTEMBER 1975

CONTENTS

INDEX OF BOOK REVIEWS

POLYTHETIC CLASSIFICATION: CONVERGENCE AND CONSEQUENCES

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The conventional definition of a conceptual class is that its members must possess certain properties in common. Vygotsky and Wittgenstein have shown that this definition is unrealistic and logically unnecessary The resultant recognition of classificatory concepts formed by family resemblances has recently led to a revision of anthropological analyses of kinship and of belief statements The present article reports the discovery that, by a remarkable convergence of ideas in the past decade, family resemblance predicates had already been adduced in certain natural sciences under the term 'polythetic classification'. The methodological and experimental results of this approach are set out, and a variety of consequences for social anthropology are drawn from them A main conclusion is that comparative studies *carried out in the stock classificatory terms of anthropology are subverted by the realisation* that they refer not to common features but to polythetic classes of social facts It is suggested that effective comparison may nevertheless be practicable by reliance on a purely formal terminology of analytical concepts, and it is envisaged that these may permit the determination of basic predicates in the study of human affairs

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It is hazardous to think that a coordination of words . . . can have much resemblance to the universe.

J. L. BORGES

I

'A perfect intelligence would not confine itself to one order of thought, but would simultaneously regard a group of objects as classified in all the ways of which they are capable' (Jevons 1874, 2: 349).

This dizzying postulate, by virtue of its very extremity, is a commonplace in theology; the capacity is central to that 'respectful chaos of unimaginable superlatives' by which men refer to God (Borges 1965: 147). Yet for all its unsettling power the notion still proffers a false security, for it implies that a finite sense can be given to the prospect of 'all' the ways there are of classifying things, whereas the truth instead is that the description of reality is in principle inexhaustible: 'Of any two things whatever, there is some respect in which they can be said to resemble each other and not to resemble some third thing' (Hampshire 1959: 31). This is so even on the traditional premiss asserted by Jevons: 'Of every class, so far as it is correctly formed, the great principle of substitution is true, and whatever we know of one object in a class we also know of the other objects, so far as identity has been detected between them' (1874, 2: 345); in other words, 'a class must be defined by the invariable presence of certain common properties' (412). Logically, however, there is no such necessity; and once it is admitted that the common-feature definition of a class need not be the only possible method, the hypothetical

number of 'ways' in which things can be classified is multiplied over and over again.

Now we know experimentally from Vygotsky that classificatory concepts are not in practice formed by children in the way traditionally supposed in formal logic (Vygotsky 1962, ch. 5); and we have been shown analytically by Wittgenstein that verbal concepts are commonly not constructed on that pattern either (Wittgenstein 1953; 1958). Instead, classes can be composed by means of what Vygotsky calls complex thinking: specifically, in a 'chain complex' the definitive attribute keeps changing from one link to the next; there is no consistency in the type of bonds, and the variable meaning is carried over from one item in a class to the next with 'no central significance,' no 'nucleus' (1962: 64). In a remarkable parallel, Wittgenstein, writing in the same period as Vygotsky, resorted to the image of a rope (later, in the *Philosophical investigations*, a thread) in order to convey the same constitution of a concept: 'the rope consists of fibres, but it does not get its strength from any fibre that runs through it from one end to another, but from the fact that there is a vast number of fibres overlapping' (1958: 87). Among the members of such a class there is a complex network of similarities overlapping and criss-crossing; sometimes overall similarities, sometimes similarities of detail. These features Wittgenstein termed, in a since famous phrase, 'family resemblances.'[1] Thus by an intriguing convergence of psychological and philosophical analyses, reported independently[2] in Russia and England respectively in 1934, the traditional common-feature definition of a class was demonstrated to be both empirically and formally defective. What Jevons had called 'the great principle of substitution' no longer held as a universal principle of classification, and a class could therefore no longer be defined necessarily by the invariable presence of certain common attributes. Since the members of a class composed by sporadic resemblances[3] were not assumed to be identical in any respect, it was no longer true that what was known of one member of a class was thereby known of the other members. What Jevons had described as a 'correctly formed' class was correct only by convention, and in numerous circumstances, as both Vygotsky and Wittgenstein proved, the convention was factually incorrect.

II

This conceptual revolution, after the millennial hegemony of the formal logic of the schools, might well have been expected to have consequences for a wider range of empirical disciplines. How far it actually did so, in one scientific undertaking or another, is a matter yet to be determined by an historian of ideas. It certainly had implications for social anthropology, however, and in 1970 an attempt was made to draw some of these out so far as they affected the analysis of kinship and marriage (Needham 1971*b*; repr. in Needham 1974*a*, ch. 1). A point worth making about that revisionary exercise is that the approach was not deductive, but proceeded instead by way of explanatory difficulties that were commonly encountered in the empirical practice of anthropological comparison.

Among the topics dealt with, a paradigm case was presented by the concept of 'descent,' a notion which in both descriptive and comparative studies had led, I contended, to typological confusion. I proposed therefore the disintegration of this

speciously univocal notion, in favour of formal criteria that represented logical possibilities. The result was the discrimination of six elementary modes in which rights could be transmitted from one generation to the next. An important aspect of this procedure, in connexion with the theme of the present essay, is that an insufficiently discriminative taxonomic concept was replaced by a set of criteria which might be matched only sporadically, and in highly various combinations, by the jural institutions of real societies.

A consequence of conceiving descent systems in this way, I then argued, was that among a number of societies compared in any formal respect there would not be presumed to exist any empirical feature common to all: 'in other words ... they may not compose a class in the conventional sense' (1974*a*: 49). This was illustrated by a simple comparison (as in the present table 1) of three hypothetical

TABLE 1 Serial likenesses among descent systems

A	*p*,	*q*,	*r*				
B			*r*,	*s*,	*t*		
C					*t*,	*u*,	*v*

societies (A, B, C), each constituted by three features (*p*, ... , *v*).[4] Let features *r* and *t* be each a type of right transmitted in a given formal mode, e.g., mode 1 ($m \rightarrow m$), male to male (cf. Needham 1974*a*: 47). There is then a resemblance, *r*, between societies A and B, and another, *t*, between B and C, but none between A and C. Yet in ordinary anthropological practice these three societies could all be classed together as 'patrilineal.' 'A crucial misdirection can thus be given to our thought by the uncritical employment of the received idea as to what a class is' (50).

I have recapitulated this example in some detail in order to provide for a comparison with others that are to follow below, and it is on the paradigm case of descent systems that I wish to focus particular attention. But I should not allow it to be thought that it is only in the analysis of kinship and marriage that there are serious disadvantages for social anthropology in subscribing to a conventional but unrealistic idea of how a class is formed. On this score, let me just allude briefly to a far larger and quite fundamental issue, namely our conception of human nature.

In the development of anthropology the essential capacities of man, the intellectual and psychic resources that make up a common nature in mankind, have largely been taken for granted. In this century, it is true, there has been much prominent debate concerning the question whether all men reasoned alike, but at least it was never denied that reasoning was a universal human capacity. For the rest, the tacit presumption was that such capacities were well known, in advance of specific ethnographic comparison, and also that they were already adequately discriminated by European languages. Prominent among these components of human nature was the capacity for belief, and it has been a standard and unquestioned feature of ethnographic works that they ascribed to their subjects, i.e., to peoples belonging to quite disparate cultural and linguistic traditions, the common mental capacity of believing. It occurred to me, however, that this was in fact highly problematical, especially since philosophers in even our own tradition appeared increasingly baffled in their attempts to determine just what belief

consisted in. I therefore undertook an investigation of this problem (Needham 1972), from a comparative standpoint, and came to the conclusion that (to be very cursory) the supposed capacity for belief consisted in no more than the custom, in a particular linguistic tradition, of making statements about belief.

In the course of the argument that led to this conclusion, a decisive step was the analysis of the verbal concept of belief after the fashion demonstrated by Wittgenstein in his examination of capacities and states of mind such as seeing, comparing, being guided, feeling, expecting, and so on (cf. Needham 1972: 116–19). In these investigations Wittgenstein showed again and again that a verbal concept which was taken to denote a distinct capacity or inner state actually referred instead to a range of phenomena (experiences, utterances, actions) that were linked only sporadically into a class by their family resemblances. In each case there was no common feature among the phenomenal instances making up the class; and in any particular case, even if a common feature could be discerned, there would remain the question why that feature should be the capacity in question (cf. Needham 1972: 121).

The outcome, then, was that a distinct capacity for belief had been attributed to human nature on the basis of an incorrect inference from the uses of a verbal concept; and that the crucial and underlying mistake was the uncritical acceptance of a traditional definition (e.g., that of Jevons: 'the invariable presence of certain common properties') of the composition of a conceptual class. It is the recognition instead of a class composed by sporadic resemblances that makes the methodological connexion between the analysis of descent and the analysis of belief, between the comparative study of jural institutions and of human capacities. The present essay is intended to establish yet more firmly the taxonomic principle in question, and to indicate certain general and inescapable consequences of applying the principle in social anthropology.

III

When I wrote my 'Remarks' I was inspired directly by the writings of Wittgenstein, and I thought I was making an original application of his ideas to the practice of an empirical discipline. Certainly the venture was an independent one, and original within social anthropology, but it proved not to have been the first exercise of the kind The encouragement to be had from this fact may be underlined if I state the circumstances of this realisation.

At the University of California, where I read my critique of kinship and marriage to a graduate class in 1971, it was mentioned to me that in zoology also a taxonomy existed in which classes of creatures were grouped by what were in fact family resemblances. Such classes, I was told, were termed 'polythetic.'[5] So there were natural scientists, it appeared, who in their own field had already broken away from the scholastic tradition in the classification of the phenomena they studied. This was very interesting, and a subsequent investigation thoroughly confirmed an instructive convergence of taxonomic method. I present here the main points established, firstly in the hope that the prior recognition of polythetic classes, on the part of different sciences, may induce social anthropologists to take the more seriously the relevance of the taxonomic principle to the study of social facts. In the second place, the convergence in question seems to me to deserve notice for the sake of its intrinsic significance in the history of ideas.[6]

The starting point is the work of the great French botanist Michel Adanson, who proposed that a member of a class of plants did not need to possess all the defining features of the class, and that a deviant specimen did not need to be assigned to a separate class (Adanson 1763, 1: cliv sqq.).[7] 'The important point he made was that creatures should be grouped together on the greatest number of features in common, and there is no justification for deciding *a priori* on the relative importance of characters in making a natural taxonomy' (Sneath 1962: 292).

In classical taxonomy, nevertheless, taxa were in fact usually described by specifying features that were absolutely diagnostic for the class of things considered. 'Linnaeus constantly revised his differentiae when the characters of newly discovered species showed that a character of a previously known taxon was no longer exclusive to that taxon' (Mayr 1969: 82), and this was a recurrent difficulty encountered generally by adherents to the common-feature principle of classification. But after 1859, and under the influence of the theory of evolution by natural selection, the definition of a class changed: 'the definition of the logicians—"individuals sharing common characters"—was replaced by "members of a group having descended from a common ancestor"' (Mayr 1969: 83). It was then no longer necessary to stipulate that the members of a taxon should invariably possess the definitive features in common; for 'there is no assurance that any given character of an ancestor will persist in all its descendants' (Sokal & Sneath 1963: 217)

In the eighteenth and nineteenth centuries, therefore, the grounds for a revision of taxonomic premisses were already well laid, but it was not until past the middle of the twentieth century that the development with which we are concerned was brought to a point of achievement. Sokal and Sneath write (1963: 13):

> Biologists owe a debt of gratitude to Beckner (1959) for the first clear enunciation known to us of one important concept of natural taxa, a concept which Beckner calls 'polytypic.'

This concept, which significantly Beckner remarks is 'not restricted to biological theory' (Beckner 1959 21), is contrasted with that of 'monotypic' classification, from which we may best approach its meaning. 'Monotypic' is a concept 'defined by reference to a property which is necessary and sufficient for membership in its extension' (23), i.e., it is equivalent to the traditional common-feature definition of a class. A 'polytypic' class, on the other hand, is formally defined (22) as follows:

> A class is ordinarily defined by reference to a set of properties which are both necessary and sufficient (by stipulation) for membership in the class It is possible, however, to define a group K in terms of a set G of properties $f_1, f_2, \ldots, f_n$ in a different manner Suppose we have an aggregation of individuals (we shall not as yet call them a class) such that:
>
> 1) Each one possesses a large (but unspecified) number of the properties in G.
> 2) Each f in G is possessed by large numbers of these individuals and
> 3) No f in G is possessed by every individual in the aggregate.
>
> By the terms of (3), no f is necessary for membership in this aggregate; and nothing has been said to warrant or rule out the possibility that some f in G is sufficient for membership in the aggregate.

Beckner goes on to state that a class is polytypic if the first two conditions are met, and 'fully' polytypic if condition (3) is also fulfilled. If the number of individuals is 'large,' all the members of K will resemble one another, though they will

not resemble one another in respect to a given *f*. 'If the n [the number] is very large, it would be possible to arrange the members of K along a line in such a way that each individual resembles his nearest neighbors very closely and his furthest neighbors less closely. The members near the extremes would resemble each other hardly at all, e.g., they might have none of the f's in G in common' (22–3). In the case of a fully polytypic class, 'no f is universally distributed in the class' (24).

A consequence of a polytypic definition of classes is that 'there will always be the possibility of borderline cases. . . . Indeed, it is an essential aspect of polytypic classes' (24). Furthermore, Beckner continues (25):

> In a sense, the whole point of polytypic . . definition is to avoid commiting oneself to a necessarily arbitrary delimitation of a class before a theoretically adequate definition can be found. . . . [It] leaves the borderline between K and non-K indeterminate where there is no theoretical reason for drawing the borderline at a particular point Polytypic concepts are sufficiently justified if only on the grounds of scientific economy. new knowledge can be utilized in applying polytypic concepts without the necessity of modifying their definitions.

As an example of the application of this concept, Beckner later cites 'escape reactions,' such as heading for the rocks, withdrawing into a shell, running, and so on. It would be very difficult, he says, to attempt to specify a set of characteristics, other than the functional one, which all these responses, and no others, possess. 'The class of escape reactions . . . is fully polytypic with respect to those features of behavior observable in the single response, and with respect to movements in relation to any environmental coordinate system, e.g., movements toward or away from particular things' (122–3).

Here, then, we have a clear formal statement of the taxonomic principle that I was only later to advocate for social anthropology. But the great interest of the conceptual advance thus made is much augmented by a circumstance that I have not mentioned In his commentary on the concept of a 'polytypic' class, Beckner makes a philosophical allusion (23) that presents a new aspect of the convergence of thought that I wish to register:

> Wittgenstein has emphasized the importance that concepts of this logical character assume in ordinary language, especially in that small segment of ordinary language that contains the semantical concepts of 'meaning,' 'referring,' 'description,' etc He points out that all the members of such classes have a 'family resemblance' to one another; he does not suggest a general term for classes of this kind

Beckner does not cite any particular work by Wittgenstein, though it is most likely to have been the *Philosophical investigations* (1953). Nor does he say what part, if any, Wittgenstein's conception had in the formulation of his own statement of the principle, and on this point we may be satisfied enough just to note the reference.

The concept of a polytypic class was afterwards taken up by Simpson in his standard work on principles of animal taxonomy (Simpson 1961). There he first expounds the principles of hierarchy and key classification, and then presents by contrast an account of polytypic grouping, described as a method which results in the formation of a taxon in which each member has a majority of the total attributes. 'This is a perfectly definite, taxonomically valid and meaningful procedure that involves *no* characters in common . . .' (42). As a hypothetical example

of the method, he groups six individuals according to seven attributes (*a*, . . . , *g*) into two species, as in the present table 2.

TABLE 2. Polytypic grouping (Simpson)

Individuals:	1	2	3	4	5	6
	a	a	b	a	a	b
Attributes:	b	b	c	e	e	f
	c	d	d	f	g	g
	Species I			Species II		

The classification is adequate for definition, in that each species has a majority of the attributes. It is adequate also for diagnosis: 'In Species I, each member has at least one of the attributes *c*, *d*, neither of which occurs in Species II, and in Species II each member has at least one of the attributes *e*, *f*, *g*, none of which occurs in Species I' (42). Simpson refers in this connexion to Beckner's formulation of the principle. In his discussion he adds the significant points, by comparison with traditional taxonomies, that 'Species II has no "differentia" and . . . the two [species], although to us they seem obviously related, have no scholastic or Linnaean "genus"' (43). Later in his text he gives an account of Beckner's views on polytypy, as demonstration of the fact that 'taxa may be recognized and defined by balances or chains of resemblances regardless of characters in common and without having abstractable archetypes' (94–5). In this phrase there are three terms of special interest: balance, with its statistical implications; chain, with its figurative correspondence with Vygotsky's idiom and its formal parallel with Wittgenstein's overlapping fibres in a rope; and the idea of an abstractable archetype, as the metaphorical paradigm of traditional (monotypic) classification.

The designation of the principle, next, was called into question by Sneath (1962). In considering Beckner's distinction between monotypic and polytypic classification, he raises the objection that these terms have other meanings that are already well established in systematics (291). As better alternatives he proposes 'monothetic' and 'polythetic' (Gr. *mono*, one; *poly*, many; *thetos*, arrangement). These terms have since been generally adopted in the taxonomies of various sciences (cf. Sokal & Sneath 1963: 13). Sneath glosses the notion of polythetic with a reference to phenetic groups which are 'composed of organisms with the highest overall similarity, and this means that no single feature is either essential to group membership or is sufficient to make an organism a member of the group' (291). This method of grouping is described as 'Adansonian classification' (292), which rightly confirms that the polythetic principle is by no means a modernity in natural science, let alone a novelty contrived in modern social anthropology. But Sneath at the same time makes the significant operational point that 'Adanson's ideas could not show their value until the statistical procedures which they required could be handled by modern electronic computing machines . . .' (292). It is only in recent years, some two centuries after Adanson's enunciation of the taxonomic principle, that this has at last been made possible and that the idea of a polythetic class has been applied to full effect. This has been done most notably in numerical taxonomy, where one marked advantage over monothetic classification is, Sneath claims, that

'numerical taxonomies . . . are unlikely to need radical revision, since, being polythetic, they are not liable to the catastrophic effects of finding that a character of presumed crucial importance is after all useless' (325).

Yet it was soon shown that a polythetic classification did not necessarily compel the revision of a monothetic classification of the same materials. Lockhart and Hartman, in a report on a test of method in quantitative bacterial taxonomy (1963), begin by defining polythetic groups as those in which 'no property is necessarily possessed by all individuals in the group, and no organism necessarily has all the properties generally characteristic of members of its group.' In consequence, 'any given organism may score mathematically as being equally similar (though in different respects) to two or more other individuals which, in turn, are quite dissimilar to each other' (68). Thus polythetic groups are 'not mutually exclusive' and are 'not theoretically analogous to the hierarchical taxa of present classifications' (69). Nevertheless, a computerised analysis of fifty bacterial strains, classed by sixty properties, first by polythetic grouping and then again by a monothetic grouping, produced little difference in the results. 'Rather curiously, our monothetic groupings were nearly identical to those obtained on a polythetic basis. It is conceivable, though it seems quite unlikely, that this apparent identity is coincidental and unique to the material used. . . .' These findings, which are illustrated by two remarkably congruent tree-diagrams (71, fig. 1, 73, fig. 2), suggest to the experimenters that 'the polythetic groups of organisms occurring in nature have a monothetic core of common properties' (76). This would entail, in their opinion, the allaying of fears that 'it might one day be necessary to abolish present classifications and to replace them with taxonomies and diagnostic schemes which somehow would take into account the multidimensional polythetic arrangements of organisms which actually seem to occur in nature' (77)

Within the same year as this critical test, Sokal and Sneath published a definitive statement of the principle of polythetic classification, in their comparative and methodological textbook on numerical taxonomy (Sokal & Sneath 1963). They first establish the principle of monothetic classification: 'the ruling idea of monothetic groups is that they are formed by rigid and successive logical divisions so that the possession of a unique set of features is both sufficient and necessary for membership in the group thus defined', they are called monothetic, it is explained, 'because the defining features are unique' (13). 'A polythetic arrangement, on the other hand, places together organisms that have the greatest number of shared features, and no single feature is either essential to group membership or is sufficient to make an organism a member of the group' (14). For a formal expression of this concept, the authors declare that they cannot do better than to quote Beckner's definition (see above). They add, however, the operational qualification that natural taxa are usually not fully polythetic, since one can usually find some characters common to all members of a taxon: 'it is possible that they are never fully polythetic because there may be some characters (or genes) which are identical in all members of a given taxon.' All the same, they conclude, 'we must regard a taxon operationally as being possibly fully polythetic, since we cannot be sure that we have observed any characters that are common to all members' (14).

Sokal and Sneath supply as a formal illustration the scheme reproduced here as table 3. Here, 'the class of 1 + 2 + 3 + 4 is polythetic (and in this instance is also

TABLE 3. Polythetic arrangement (Sokal & Sneath)

	Individuals					
	1	2	3	4	5	6
Characters	A		A	A		
	B	B	B			
	C	C		C		
		D	D	D		
					F	F
					G	G
					H	H

fully polythetic, since no one character is found in all the four individuals). . . Individuals 5 and 6, however, form a monothetic group' (15).

One of the difficulties of Beckner's definition, Sokal and Sneath add, is that in natural taxa we do commonly find features that are not possessed by large numbers of members of the class. 'Furthermore, we cannot test whether any given *f* [feature, character] is possessed by large numbers of the class before we have made the class, and therefore we cannot decide whether to admit this *f* into the set *G*' (15). But this difficulty, they state, can be avoided by defining membership of the class in terms of common (or shared) attributes. Further on the score of method, they add that polythetic groups can themselves be arranged polythetically to give higher polythetic groups, 'as is done in building a hierarchy in a natural system.' As for the consequences of adopting a polythetic taxonomic principle, the advantages of polythetic groups are that 'they are "natural", have a high content of information, and are useful for many purposes', the disadvantages are that 'they may partly overlap one another (so that hierarchies and keys are less easy to make than with monothetic groups) and that they are not perfectly suited for any single purpose' (15). By way of a general reminder, the authors later stress that 'when . . . groups are polythetic ones, we must bear in mind that it is never certain, but only more or less probable, that a member possesses any given feature' (171).

Finally, for the purposes of the present survey, we may notice the cast more recently given to the principle by Mayr, citing Beckner and Simpson, in his textbook on systematic zoology (1969). 'Taxa characterized by a set of characters of which each member has a majority are called *polythetic* taxa. . . No single feature is essential for membership in a polythetically defined taxon nor is any feature sufficient for such membership' (83). In fact, as he continues, many zoological taxa 'are based on a combination of characters, and frequently not a single one of these characters is present in all members of the taxon . . .' (88). Mayr's glossary defines 'polythetic' as: 'Of taxa, in which each member has a majority of a set of characters' (409). In this formulation of the principle, then, the stress is laid on the possession by each member of a class of a simple majority of the defining features, without, that is, the assignment of decisive weight to any of them.

IV

We see therefore that polythetic classification, far from being a unique innovation in current social anthropology, is a recognised taxonomic principle in a range of natural sciences.

In the adoption of this principle two circumstances appear to have been crucial: the promulgation in 1859 of the theory of evolution, when natural history challenged the logicians' method of classifying species; and the development, a hundred years later, of electronic computers which made it practicable at last to put into scientific effect the precept enunciated by Adanson in 1763. In the outcome, polythetic classification became employed in sciences so various as botany, zoology, biology, and bacteriology, and it carried fundamental implications for each. The empirical application of the method, and the elaboration of the principle, were in the main the work of a period so recent as the nineteen-sixties.[8] In the formal expression of the concept of polythetic classification, a specially significant point is the allusion by Beckner to Wittgenstein's emphasis on concepts of the same logical character in ordinary language. This neatly clinches a noteworthy convergence in the history of ideas, and on this score the main part of my case is made.

Let us now examine some of the consequences for social anthropology that may be derived from this better appreciation of the polythetic principle. This will have to be done in fairly general terms, without a conclusive empirical demonstration of each point of argument, but the practical relevance of the principle to the comparative study of social facts should be evident enough.

I have already entered the caution that, in the study of descent systems in particular, a direct consequence of polythetic classification in anthropology is that 'comparison becomes far more difficult, and on any large and detailed scale perhaps impracticable' (Needham 1974a: 50). In the study of relationship terminologies, similarly, the consequence is that 'while analysis is made more exact, comparison is made more intricate and difficult' (60). More widely, the consequence is the realisation that the social phenomena classed as 'kinship,' etc. 'do not in all cases possess any specific features such as could justify the formulation of general propositions about them' (69); 'there may . . . be something in common, under each general term [e.g., 'marriage,' 'incest'], but not necessarily a definite set of characteristic, specific, or essential features' (70). Operationally, this means also that attempts to find statistical correlations are seriously affected, for these cannot be established without a precisely specified typology of phenomena; but if it is conceded that the social facts in question do not necessarily compose a conventional class of a homogeneous kind, under each type, 'but may instead exhibit an immense array of serial and more complex resemblances,' then the grounds for this method of comparison and explanation are removed (70).

By this account of the matter, the first consequence of the adoption of polythetic classification in social anthropology is that comparative studies, whether morphological or functional or statistical, are rendered more daunting and perhaps even unfeasible. Yet polythetic classes are likely to accommodate better than monothetic the variegation of social phenomena: they have, as Sokal and Sneath put it, a high content of information, and they carry less risk of an arbitrary exclusion of significant features. In other words, it could be said, the polythetic principle is truer to the ethnographic materials. If this is so, then the increased difficulty of comparison is a price that simply has to be paid—if there is some way of meeting it.

This is a point on which social anthropology may have much to learn, by way of explicit method, from the sciences in which the polythetic principle has already

been well recognised. We may find special instruction, in the first place, under the aspect of evolution, since this is so basic to the sciences whose examples we have just considered. I shall therefore deal in some detail with the methodological implications of studying evolutionary materials.

One very striking difference between the materials that a natural scientist has to classify and those which are the concern of the social anthropologist is the presence or absence of evolutionary connexions. It is the factor of common but divergent descent that permits a zoological taxon, for instance, in which not a single one of the definitive characters is present in all members of the taxon (Mayr 1969: 88). This is an acceptable grouping of natural entities, whether animals or plants or bacteria, because lineages of descent can be demonstrated; and this can be done even if intermittently the original characters have changed or fallen away, under the pressures of adaptation, until the descendants bear little resemblance either to one another or to their common ancestor. It is doubtless the factor of phylogenetic descent that encourages some among even the most modern taxonomists to speak of 'natural' groupings of organisms, and to distinguish such aggregates from 'artificial' groupings. I suppose, too, that it is phylogeny that largely accounts for what Lockhart and Hartman isolate as the '*monothetic core of common properties*' in polythetic classes of bacterial strains (1963: 76). We might therefore sum up the situation in the natural sciences by saying that, as intrinsic features of the materials under comparison, common descent composes natural classes while natural selection variegates the members of such classes.

In the comparative study of social facts, however, these conditions do not obtain. It is not without good reason that social anthropologists today pay little if any attention to evolutionary connexions among social forms, to the extent indeed that it is hard to find a modern instance of such an approach.[9] There is, though, one study of the kind, carried out on a scale comparable with that of zoology or bacteriology, in Murdock's *Social Structure* (1949). In his chapter on 'Evolution of social organisation' Murdock propounds five generalisations about 'the normal order of change among the principal elements of social organisation' (221–2), and he applies these generalisations to his sample of 250 societies. He distinguishes eleven 'major types of social structure' defined by rules of descent and types of cousin terminology (224), and these major types are sub-divided by rules of marital residence into a total of forty-seven 'structural subtypes' (324–6). A technique for the reconstruction of antecedent forms is presented (326–31), and each of the societies in the sample is then assigned to a sub-type and thence derived from a series of prior sub-types (332–4). The enterprise is painstaking and detailed, and consistently worked out; but it does not serve to establish a correspondence between natural evolution and social evolution such as could now permit the application in social anthropology of the taxonomic procedures that we have observed in zoology and other natural sciences. I do not refer to defects in the stipulation of those features (e.g., 'cousin terminology') by which the types and sub-types are distinguished, for in principle these could be amended and the evolutionist method remain feasible. What is a graver criticism is that Murdock's evolutionary scheme permits the establishment of such a proliferation of transitional connexions, many of which are reversible, as to prompt doubt concerning its general validity (Needham 1962: 173–80). Certainly there is extraordinarily

little resemblance between the 'evolutionary' connexions among a set of eight societies that I selected from the sample (178, fig. 1) and the phylogenetic trees that can be established in the natural sciences. Another source of doubt concerning the scheme is that by the introduction of an additional discriminating feature (*viz.*, prescriptive alliance), that Murdock did not take into account, it can be shown that a number of societies in the sample can be placed in quite different relative positions from those which they occupy on Murdock's premisses (175). This implies that sociological taxonomy is highly artificial (as in fact it is), and such that the evolution of institutions may be disparately inferred according to the typological features that are more or less arbitrarily decided upon. There is no sign, at any rate, of a correspondence with quasi-natural monothetic clusters of features. Rather, it is the construction of a sociological typology of cultural particulars by monothetic criteria, and these too of the most slender and unreliable kind, that is the chief and irremediable weakness of this evolutionary scheme.

Now Murdock's venture is admirable in its own terms, and in some ways the most impressive evolutionary undertaking in anthropology, but as soon as the polythetic character of the resemblances in question is recognised, the grounds of the taxonomy on which the study rests are quite removed. For example, the Araucanians, Iatmul, Miwok, Nyoro, and Thado are all typed together as 'Normal Omaha' (Murdock 1949: 240), on the basis of patrilineal descent and the terms for cross-cousins (239, 244), whereas the quickest survey of the ethnographic evidence on each society shows that their forms of social classification, not to mention their institutions, are systematically disparate (Needham 1974*a*: 51; cf. Lowie 1917, ch. 5). The concept of 'descent,' moreover, is itself polythetic (47), and so also are the conventional types by which relationship terminologies are classed as 'Omaha,' etc. (47–54, 59–60). When, therefore, the descent systems under comparison are analysed by polythetic criteria, instead of being typed by a few monothetic features (60), the presumed resemblances are reduced or abolished; the comparison is vitiated, and the attempt to work out evolutionary interconnexions is thereby doomed to failure.

This particular outcome does not mean, however, that no form of evolutionary reconstruction is possible in the comparative study of institutions. I have suggested indeed that prescriptive systems tend to change in regular ways (Needham 1967: 46), and it seems quite feasible to establish regular transformations which bear a resemblance to evolutionary modifications in natural species. Admitted, this line of enquiry has not yet been taken very far, and the series of transformations so far proposed is very tentative, but there remains nevertheless some empirical justification for speaking in these regards of the 'evolution' of social forms. Yet it does not at all follow that the taxonomic methods of the natural sciences, e.g., zoology, can therefore be directly taken over in social anthropology. The crucial feature that is missing from the prescriptive systems compared is still that of common origin. If there is a precise structural correspondence between the terminologies of the Tibeto-Burman Garo of north-eastern India and the Malayo-Polynesian Manggarai of eastern Indonesia (Needham 1966: 155, table 4), this is not because these societies are descended from a common ancestral society. Still less is such an evolutionary connexion the explanation of the resemblances among other two-line terminologies in Australia, south India, and the tropical forests of south America.

In fact, the type of evolution at work in these instances is no more than formally similar to that of natural species.

The outcome, in any case, is certainly not much like that in zoology, for example, for whereas a zoological taxon may comprise very dissimilar individuals, the anthropological taxon of prescriptive systems of social classification is so defined that the individual terminologies shall be distinctly alike. The points of resemblance are not cultural particulars but analytical abstractions, prescriptive terminologies are thereby classed monothetically. The resultant class of social facts, defined by a common feature (the invariant relation articulating the categories) is artificial. All the same, the label 'prescriptive' does not denote a class of societies to which the law of substitution can be applied, for in prescriptive alliance systems (conceived as empirical forms of social life) the categories, marriage rules, groupings, and modes of social action are independent variables (Needham 1973: 174). In other words, whereas prescriptive terminologies are defined monothetically, the class of 'natural' societies characterised by jural categories of this type is polythetic. It may be possible to list certain characteristic features of such societies (Needham 1970*a*: 257; cf. Korn 1973: 100–4), but the societies do not compose a class such that 'whatever we know of one object . . . we also know of the other objects.'

Even in this quite promising line of 'evolutionary' investigation, therefore, the parallels with a natural science are superficial. An abstract stipulation occupies the place of a common ancestor; the similarity of social forms has nothing to do with common descent; and the postulated transformations of prescriptive systems appear causally different (Needham 1967: 47) from the evolution of natural species. These essential contrasts make it hardly likely that the methods of the natural sciences, in coping with polythetic classes of evolutionary materials, should be directly transferable into social anthropology

Nevertheless, the polythetic approach in the natural sciences can still seem to offer the promise of a method that might be adapted to the concerns of social anthropology. This is in the practice of numerical taxonomy, and particularly in the method of 'cluster analysis.' We have seen that Adanson's principles demanded statistical operations that were not practicable until the development of electronic computers. Likewise, an obvious difficulty in the project of large-scale comparison of social facts has long been that the great numbers of variables involved made the task prohibitive, and this source of difficulty is very much magnified when the evidences compared are defined polythetically. But in quantitative bacteriology Lockhart and Hartman have managed to compare fifty organisms by reference to sixty properties such as production of indole, colony morphology, and tolerance to sodium chloride (1963. 69–70). This is impressive as it stands, but the researchers add that a certain computer could be programmed to handle as many as 1,500 bacterial strains, each one scored for 200 properties. With figures so high as these we approach the sort of computation that presumably would be called for in the comparison of social facts. If therefore the polythetic classification of bacteria can be coped with by an appropriate computer programme, perhaps the taxonomic principle in itself need no longer be seen as an obstacle to effective large-scale comparison in social anthropology.

Unfortunately, Lockhart and Hartman's experiment does not make the case, for by their method the groups 'are made monothetic by discarding all characters

which vary within them' (Sokal & Sneath 1963: 188). Without going into the technicalities of this procedure so far as bacteriology is concerned, we can at once see that it introduces an arbitrary factor such as in social anthropology we seek to obviate precisely by the adoption of polythetic classification. In the study of social facts what is needed is not a convenient technique for cutting down the number of variables, but a means of accommodating as many as possible. The method of cluster analysis can be justified in the case of Lockhart and Hartman's experiment by the result that the polythetic groupings were nearly identical with the monothetic; but this can be explained by their suggestion that polythetic groups of organisms occurring in nature have a monothetic core of common properties, and such phylogenetic properties do not characterise social facts. We can well follow the bacteriologists in their application of the taxonomic principle, but the evolutionary nature of their materials introduces a difference of kind which prevents us directly taking up their statistical methods.

There are in any case other reasons for not turning to cluster analysis for the purposes of anthropological comparison. Quantitative methods in taxonomy are 'based on the principles that classification is a measure of over-all similarity among organisms and that all properties of organisms are potentially of equal value in creating taxa, so that no *a priori* assumptions need to be made of the relative importance of particular features' (Lockhart & Hartman 1963: 68). Simpson writes of 'balances of resemblances regardless of characters in common' (1961: 94–5); Sneath refers to the 'highest overall similarity' (1962: 291); Mayr defines a polythetic taxon as one in which each member possesses simply a 'majority' of a set of characters (1969: 83, 409). This approach entails a number of difficulties when we conjecture its possible use in the comparative study of social facts.

To begin with, 'it is reasonable to ask whether a definition of a polytypic concept is after all a *definition*, since it is certainly imprecise' (Beckner 1959: 24). In Beckner's formulation, as he points out, 'the vague term "large number" occurs twice in the definition' (24), and there is no rule of method for deciding in general, or for any given context, what is to count as a large number. The same kind of uncertainty attaches to the phrases 'over-all similarity,' 'balances of resemblances,' and a 'majority' of characters. No matter how the definition of a polythetic class is expressed, the difficulty is to know where to draw the line. This problem is not resolved by the admission that 'there will always be the possibility of borderline cases' (Beckner 1959: 24), for the location of the border is itself a function of the degree of numerical preponderance that is thought sufficient, and this in principle is always contestable. In any event, the consequence is that 'it is never certain, but only more or less probable, that a member [of a polythetic group] possesses any given feature' (Sokal & Sneath 1963: 171). A numerical taxonomy, therefore, leaves the social anthropologist in much the same definitional quandary as when he is faced with the question what is to count as an instance of a given institution. For example, in a reassessment of the concept of marriage Rivière has concluded that 'we mislead ourselves by describing with a single term relationships which in different societies have no single feature in common other than that they are concerned with the conceptual roles of male and female' (Rivière 1971: 70). That is to say, 'marriage' is what Wittgenstein called an 'odd-job' word (cf. Needham 1972: 124–5; 1974*a*: 44); or, as we might better say now, it is a poly-

thetic concept. No statistical method or computer programme can decide what is to count as marriage. Concomitantly, to identify a particular social relationship as an instance of marriage, by a polythetic definition, will not demand the presence of any specific feature by which it is possible to decide that it is to count as marriage.

On the other hand, a polythetic conception of marriage, as determined by overall similarities, still does not preclude the risk of leaving out of account some feature that is regarded indigenously as essential to the relationship, or one that might be more relevant to the purpose of the comparison. This is especially likely when what is in question is an evaluation placed on the relationship, e.g., a moral repugnance for divorce, which happens not to be present in a majority of the cases classed together. In general, indeed, human affairs are semantically so very complex that it must be difficult in the extreme (if it is even conceivable) either to stipulate significance as a polythetic feature or to assess the degree of similarity among the meanings or values attached by different civilisations to any kind of institution that is the subject of a comparative proposition.

Another difficulty is exposed by the possibility in natural science of determining features by difference, e.g., by whether or not an organism produces indole or ferments lactose. 'There may be endless arguments as to whether two organisms are similar in that neither ferments lactose, but everyone agrees that they are different if one does so and the other does not' (Lockhart & Hartman 1963: 70, 77). But what in the study of human affairs would permit this decisive treatment? At the level of social facts, as distinguished from analytical abstractions, it will be often impossible and usually dubitable to determine such absolute contrasts. For that matter, though, it is not always so easy even in the natural sciences to determine features so unambiguously. In bacteriology, for example, the Lockhart and Hartman experiment classified organisms by features such as colony morphology and tolerance to NaCl, but these involved relative discriminations and hence a degree of arbitrariness. Three forms of colony were scored, *viz*., punctiform, circular, and 'irregular'; and tolerance to NaCl was scored at four levels as sensitive, weakly resistant, moderately resistant, and strongly resistant (Lockhart & Hartman 1963: 70). These morphological and scalar discriminations are uncertain and at least imprecise, and some are no more dependable than are the criteria often employed in social anthropology. Moreover, even on the assumption that features can be well determined, numerical taxonomy has a disconcerting consequence that must limit or even rule out its application in social anthropology. In a polythetic group, as will be remembered, no property is necessarily possessed by all members in a group, and no individual necessarily has all the properties generally characteristic of members of its group. Hence, in bacteriology, 'any given organism may score mathematically as being equally similar (though in different respects) to two or more other individuals which, in turn, are quite dissimilar to each other' (Lockhart & Hartman 1963: 68). Clearly this is an outcome that would be quite unacceptable in the study of social facts.

Underlying all these difficulties that lie in the way of adapting numerical taxonomy to the polythetic concepts that are none the less called for in social anthropology there is a more fundamental obstacle. In the natural sciences the features by which polythetic classes are defined have generally a real, distinct, and independent character, and they can be clearly stipulated in advance. Such features

are, in zoology, skeletal structure (a definite number of bones functionally arranged in a certain order); in botany, roots, leaves, pistils; in bacteriology, chemical elements, compounds, and their reactions; in many sciences, at a deep level of analysis, molecular structures and the particles of which these are composed. This is a rather rough and ready characterisation, of course, and it becomes less appropriate when ultimately the character of 'fundamental' particles comes into question, but it serves all the same to make a crucial contrast between the natural sciences and social anthropology. In what has been presented here as the most relevant example of taxonomic method, namely quantitative bacteriology, the researchers are in no doubt concerning what is or is not lactose or about whether it is or is not present: it can be exactly defined in advance, and its chemical properties and reactions are known or testable. This kind of certainty about the materials under study (whatever ambiguity may attend the discrimination of forms or the assessment of degrees of resistance, etc.) permits the method of classification by differences: a definite feature can be definitely determined as either present or absent.

But in the realm of social facts this aspect of polythetic classification is hardly to be found. A main reason is that in social anthropology the determination of the constituent features of a polythetic class cannot be carried out by reference to discrete empirical particulars, but entails instead a reliance on further features of the same character which themselves are likewise polythetic. In social life, that is, there are no established phenomena, in the form of isolable social facts for instance, which correspond to the elements and particles in nature. The disparity between the natural sciences and social anthropology, in taxonomic method as in much else, reflects a contrast of kind between natural entities and social facts. This contrast is the most marked when the materials for an anthropological classification are collective representations More generally, in any case, there is no reason that a classificatory technique that is appropriate to one kind of evidence should be applicable to another, and all the less is this so when the evidences in question are contrasted as physical and ideational.

V

In this article I have set on record what strikes me as a remarkable convergence in the history of ideas, and I have briefly considered certain methodological consequences of the discovery that classification by sporadic likenesses is already practised in certain natural sciences.

But the results are not encouraging, for whereas the polythetic methods developed in those sciences are much in advance of those suggested for social anthropology (Needham 1971*b*), both in their formal expression and in their empirical application, there are serious objections to the adoption of such methods into the study of social facts. This setback is not the fault of the taxonomic principle, but has to do with the essential disparity between natural phenomena and collective representations. In social anthropology the idea of a polythetic class does indeed proffer a great rectification of thought and a newly panoptic perspective on human affairs. But at the same time this vantage offers an austere prospect, for amid so many uncertainties as we have glimpsed the one sure outcome is that comparative studies, if they are carried out in empirical terms, will become irresolubly compli-

cated. Polythetic classification therefore tends, by this view, to confirm Evans-Pritchard's aphorism: 'There's only one method in social anthropology, the comparative method—and that's impossible.'[10]

There is a great deal to be said perfectly seriously in support of this quip, but the issue is not quite so decided as it implies. Comparative studies are likely to be defective and unproductive so long as they continue to be carried out within conventional, i.e., monothetic, taxonomies and by reliance on substantive paradigms (Needham 1974*a*: 53, 60). But comparison stands a better and quite different chance of success if it is conducted in formal terms. There have been prominent examples of this kind of approach under the rubrics of 'elementary forms' and 'elementary structures,' and it could yet be pursued further by the formulation of a 'vocabulary of analytical concepts that were appropriate to the phenomena under consideration but would not be merely derived from them' (Needham 1974*a*: 16). Not only are the terms of common English, such as 'kinship' or 'marriage,' worse than unserviceable in this enterprise, but so also are the quasi-technical terms of anthropology such as 'unilineal,' 'Omaha,' 'patrilocal,' and so on. As will be appreciated by this point, a characteristic flaw in such terms is that they have been presumed to denote monothetic classes of social facts, and thus to permit the operation of substitution; whereas in fact their reference is polythetic, so that comparative propositions are perpetually undermined by the awkwardness that the absence of common features precludes the possibility of substitution. But this objection does not attach to relational concepts such as 'symmetry,' 'alternation,' 'transitivity,' 'complementarity,' etc., or to analysis by reference to logical possibilities (Needham 1974*a*: 16, 47; 1974*b*: 39). Here we have formal properties which can be defined in purely formal terms, e.g., in the notation of symbolic logic, without reference to any classes of entities, however the classes may be composed, or to the characteristic empirical features of their members.

Formal analysis of this kind is not merely a methodological postulation, nor just a critical technique in the undermining of received categories of anthropological discourse. In practice, it accurately accommodates social facts, it facilitates systematic analysis, and it makes possible the effective prosecution of comparative studies. In each of these regards this radical style of abstraction evades the disastrous deception of reificatory and monothetic taxonomies, and it renders unnecessary a desperate contention with the alternative hazards of trying to theorise taxonomically about classes of facts that in empirical terms are polythetic.

The formal terms mentioned above, together with others that propose themselves as appropriate to any relational enquiry, have been arrived at largely inductively through their application in the empirical analysis of various types of society. They also happen to accord, however, with certain philosophical conclusions arrived at by Campbell in his study of family resemblance predicates (1965). He argues that, for any given linguistic context, not every predicate can be of this kind; 'it follows that the notion of family resemblance cannot of itself solve the problem of universals', there must be, for any such context, what he calls 'basic predicates' (243). In social anthropology there have been repeated attempts to establish ultimate theoretical notions, relating to social life and collective representations, in morphological or functional or structural terms, or else by resort to universal needs or motives or mental capacities. These ventures have not proved

successful, but the relational concepts that I have adduced are well adapted, I suggest, to the analytical task of taking each case as it comes and to the comparative task of formulating propositions about classes of social facts. There is pragmatic reason to think that such purely formal terms may provide, for certain purposes, the basic predicates that are called for in social anthropology.

The present article also accords in another important regard with the conclusion to Campbell's analysis. Where we wish to make generalisations in the confidence that they admit no exceptions, he writes, defined terms are to be preferred, other things being equal, to family resemblance terms: 'We should not rest content until family resemblance predicates, admittedly intelligible, have been banished from our sciences' (Campbell 1965: 244). This is exactly the aim of my 'Remarks,' to which this essay forms a sequel, though I do not think that such (polythetic) predicates can ever be eliminated from practical description in the field or from academic discourse about ethnographic reports. Where they can deliberately be dispensed with is in the contrivance of a formal theoretical terminology. Studies of prescriptive alliance systems show particularly well that a definition which is designedly monothetic, in combination with analytical concepts which are necessarily of the same character, can advantageously be applied to classes of social facts that are extensively polythetic.

There is a great deal more that could be written about these fundamental topics, and what I have written here is no more than groundwork for the formulation of theoretical issues, both formal and analytical. Also, if the appreciation of polythetic classification is to have its proper effect, it is desirable that more detailed empirical demonstrations should be fully set out. For the present, though, I conclude with two observations that indicate something of the relevance of this theme to ultimate concerns in the understanding of human affairs.

The first is to take up the point that 'our analysis may be guided by the same logical constraints as must have been effective in producing the systems that we study' (Needham 1974*a*: 36, 48; 1974*b*: 39–40). This is particularly clear in the comparative study of descent systems, terminologies, and certain rules of marriage, but there is a wider implication. If we consider that formal terms such as 'symmetry' or 'transitivity' are not peculiar to a particular linguistic and intellectual tradition, but denote properties which must be discriminable (either conceptually or in social practice) by any cultural system of thought, then it follows that the terms are intrinsically appropriate to the study of exotic collective representations. Alternatively, a more speculative notion is that the formal terms denote mental proclivities and constraints which are universal to mankind in the fabrication (deliberate or not) of categories and articulatory relationships. According to either of these conjectures, the kind of theoretical terminology to which I have referred would thus naturally qualify as basic predicates.

The other comment, linked to the former, is to stress the conception of social anthropology that inspires the present essay. The approach is guided by the ambition, inherited from the *Année sociologique* school and directly traceable back to the Enlightenment, to determine what can be called the primary factors of experience by the comparative study of social facts. The facts at issue in the 'Remarks' and in my analysis of belief were words, *viz.*, verbal concepts framed by cultural traditions in the classification of the world, whether this was the

ordering of social life or the ascription of capacities to the human mind. In the analysis of such collective representations, whether they were the quasi-technical generalisations of social anthropologists or the ordinary resources of everyday language, it was found that certain deep miscomprehensions were ultimately the results of the traditional assumption that classificatory concepts were necessarily composed about common definitive features, i.e., that they were monothetic. In each case, however, it was shown that the words in question actually denoted classes composed by family or sporadic resemblances, i.e., that they were polythetic. Now the outcome of analyses of this kind should not be seen as merely a local or technical rectification of European academic argument, but as pointing to a general hazard of language which presumably afflicts men in any tradition when they classify their fellows and their nature. 'In seeking to translate alien concepts... we have to appreciate that the foreign words in question are themselves words that may be in the same state as our own,' so that the speakers of another language, constrained through it by their own collective representations, 'must be assumed to be the victims of just such linguistic defects, traps, and diversions as are we ourselves when we formulate our own thoughts' (Needham 1972: 233). Thus the realisation of the confusions brought about in social anthropology by stock classificatory terms may serve to prepare our understanding in coming to terms with alien concepts which, in a fashion that is similarly unrecognised by those whose modes of thought we want to comprehend, are also polythetic.

NOTES

[1] For a brief commentary on the history of the 'family' metaphor, see Needham (1972: 113 n 4) To the writings cited in that place may be added the specific parallel presented by Nietzsche (1886 25), who referred to the similarity of Indian, Greek, and German philosophising as literally a 'family resemblance' (*Familien-Ähnlichkeit*). We know moreover that Nietzsche, like Wittgenstein, was an admirer of Lichtenberg, a notable predecessor in this train of thought; he judged Lichtenberg's aphorisms to be worth a place in a 'treasury of German prose,' and found them worth reading again and again. It should be remembered, all the same, that *Familienahnlichkeit* was in literary use over a century before Wittgenstein wrote the *Philosophical investigations* Grimm, *Deutsches Wörterbuch* (vol. 3, 1862) glosses the word with the Lat. *gentilis similitudo* (1306, s.v) Mr Thomas Braun has directed me to the statement in the preface (vol. 1, 1854, cols. xxxix–xli), where it is explained that Latin equivalents are supplied as the best means to clarity, and he has kindly given me his opinion also that the phrase is not a literary tag such as would indicate a classical origin of the notion. For a good philosophical analysis of this idea, see Campbell (1965).

[2] In an earlier reference to this convergence (Needham 1972 111–14), I thought I was reporting a discovery, but I afterwards found that I had been anticipated by Stephen Toulmin (1969) I regret my ignorance of Toulmin's essay, which I should have been happy to adduce. Toulmin reports that he read Vygotsky in alternation with Wittgenstein's *Zettel* (1967) and found his head ringing with intellectual echoes: 'the theoretical parallels, the similarities in general attitude, even the tones of voice of the two men were too close to be entirely independent' (71–2). He suggests moreover an indirect connexion between Vygotsky and Wittgenstein through the psychologist Karl Buhler, who is frequently cited by Vygotsky in *Thought and language*, was an acquaintance of Wittgenstein's sister (at whose house Wittgenstein could have met him), and was also 'a major contributor to modern linguistic theory' (72).

[3] On the ambiguity of the idiom of 'family' resemblances, see Needham (1972 112–13) I have previously alluded to such resemblances as 'serial likenesses' (1972 119 n 6; 1974a· 49), but on the whole the qualifier 'sporadic' seems the most apt.

[4] That three features were assigned to each society means that formally speaking the demonstration was not conducted in the simplest possible terms, but the logically redundant features emphasized the point, I thought, and conduced perhaps to an easier assimilation of the principle to empirical instances.

[5] I am very grateful to Mrs Aileen Garsson Baron, then a graduate member of the Department of Anthropology at the Riverside campus of the University of California, as being thus 'the onlie begetter' of this insuing study.

When I returned to Oxford I remembered the term and asked occasionally about it, of biologists and others, but drew blank. The matter then receded from my attention, under the pressure of other concerns, until December 1974, when Dr R. N. Pau, of the Department of Zoology at Oxford, delivered a seminar paper to social anthropologists on the subject of primitivity, and hence taxonomy, in zoology. When I asked him about polythetic classes he very kindly supplied me with two important references (Sokal & Sneath 1963; Mayr 1969) which laid the basis for the present article

[6] On the historical count, I should point out that for the present purpose I think it sufficient to make only a summary survey of certain of the sources. There remain a great many more (see, e.g., the bibliography to Sneath 1962) which in a full reckoning, by an historian of science perhaps, would deserve exploitation. For a short but fundamental statement of the development of taxonomic theory since 1851, see Gilmour (1951)

[7] It is only by way of this reference, I am afraid, that I have belatedly picked up the significance of Bambrough's allusion to 'botanical taxonomists' in his demonstration that five objects may each have four out of five given features and that the missing feature may be different in each case (Bambrough 1961 209–10) The example he gives is, with a slight alteration in designations. (1) *A B C D*, (2) *A B C E*, (3) *A B D E*, (4) *A C D E*, (5) *B C D E*

[8] To stress the temporal continuity in the convergence that I am demonstrating, it may be noted that Mayr's book on systematic zoology was published in 1969 and that the idea of classifying by 'serial likenesses' in social anthropology was proposed to the A.S.A conference on kinship and marriage in the spring of 1970 (cf Needham 1971*a*). The interval between even Beckner's monograph and the delivery of the conference paper was a mere eleven years

[9] Evolutionist explanations, such as were common in the last century, are sharply to be contrasted with historical (or 'diffusionist') reconstructions, the feasibility of which is another matter altogether (Needham 1970*b*: lxv–lxxxi)

[10] He said it to me, and doubtless to others as well, but so far as I know it does not appear in any of his published writings. Cf Evans-Pritchard (1965).

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TRADITIONAL POTTERS OF THE KATHMANDU VALLEY: AN ETHNOARCHAEOLOGICAL STUDY

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This study of surviving traditional potters in Thimi and Bhadgaon just outside Kathmandu is concerned especially with what archaeologists are usually unable to discover i.e. the potter's motivation in making his decisions about the pots he makes and the designs he puts on them. The Newar potters proved both accomplished and articulate The major stimulus to virtually all their decisions was the desire to sell the largest number of pots in a competitive situation hence each potter made the greatest range of shapes with the greatest number of variations in each that he could Other questions studied included how the potters, and their customers, named their pots (largely by function rather than morphology), the interrelationship (both functional and financial) between clay containers and those of wood, basketry and metal, and the degree to which a total archaeological study of a ceramic system could be expected to yield a representative picture either of that system as it once existed or indeed of the total society of which it formed a part

Two communities in the Kathmandu Valley produce virtually all the pottery used both there and in the neighbouring mountain villages. One is Thimi, a village about 4 miles east of Kathmandu, comprising roughly 700 potting households, the other Bhaktapur (also called Bhadgaon) about 10 miles east of Kathmandu is larger, but with only about 300 potting households

Both have access to excellent clay sources. The dark grey *kalımati* is found especially through the centre of the Valley, while small quantities of a second type, *kamero* or *takucho* which is greyish white in colour, diatomaceous, and high in silica content, are found at Thimi. Little wood is available for fuel, and the pots are fired in bonfires of straw covered with ash. The potting families, the *kumar*, like most of the specialist castes in the Valley are predominantly Hindu, and in Thimi at least, all Newar. Households consist of parents and sons with their families, varying from three to ten in number. Much of the potting is done by the men; although there is no rule about this, the women have children to look after or work in the fields. Some cultivation especially of vegetables is carried out by the potting families, but garden produce is not usually sold for cash as with the unspecialised village families. Pottery is carried to Kathmandu by the boys or men of the family on *banghi* sticks for sale in the market.

The present study resulted from limited fieldwork in August 1972, and was aimed at investigating the relationship between the potter, his repertoire and his market. In any traditional pottery situation the potter on the one hand has a basic pool of skills—the local ceramic tradition—to which he is bound either more or less rigidly according to the nature of the mechanism of skill transmission. The customer on the other hand equally has a range of needs, conditioned primarily by his environment and mode of subsistence, together with additional cultural and social

Man (N.S.) 10, 370–386.

factors, within which he too has the opportunity for personal choice in certain areas. This choice he may exercise in varying degrees. Between the two—the product of the shifting balance between them—is the pottery repertoire.

So often for the archaeologist this is all he has, backed up by a general idea of the environment, considerable ignorance of the social and cultural supra-environment, a general idea of the ceramic tradition, extreme ignorance of the mode of skill transmission and fair knowledge of the customers' needs as based on economic factors decreasing rapidly in cultural and spiritual areas. The present study was one of a series by the writer concerned with the exploration of these relationships in different contemporary ceramic situations in which information of considerable interest to the archaeologist is still available.

I

The pottery made by the Thimi and Bhadgaon potters is far more varied in form, decoration and technique than that found in most parts of India. Functional variation of shape is considerable; pots can be painted, pattern-burnished, stamped, rouletted, incised, sprinkled with mica or elaborately carved, while techniques include double firing to produce black wares as well as the usual forms of wheel-throwing, beating and scraping.

The repertoire is dominated by the common household containers for liquids and solids, i e. the *ghyampo*, *ghagro* and *ghaiñto*, as well as the common cooking pot of a form well known in north India, the *hāndi*. These are made in enormous quantity and with considerable minor (or secondary functional) variation. In addition a wide range of more specialised vessels is made for the making and drinking of rice beer and *rakshi*, for the transport and storage of milk products, for the storage of pickles and roasting maize, for household worship, for smoking and for a variety of miscellaneous domestic needs. These forms are listed below and illustrated on figs. 1–4.[1]

1. *Common domestic jars*

The commonest domestic vessels are the *ghyampo* (pl. 4), a very large container used for both grain and rice beer storage; the *ghagro* (fig. 1,4), a smaller vessel usually with gently rounded base for carrying and storing water, often similar in shape to the standard brass household water pot for those who can afford it, the *ghagro* (fig 3, 16); the *ghaiñto* (fig 1, 3), again usually slighter smaller, more often with a flat base, and used for both liquid and dry goods; and the *hāndi*, a wide-mouthed cooking-pot for both boiling and roasting (fig 1, 2).

The range of size for each class, as reported, is given below:

	Smallest	*Largest*	*Usual*
Ghyampo	2 and a half pati	30–35 pati	20 (80 litres)
Ghagro	2 pati	5 pati	3 (12 litres)
Ghaiñto	1 mana	12 mana	½–1 (2–4 litres)

(1 pati = roughly 4 litres = 8 mana)

It is apparent that there is a considerable overlap between the classes although the main grouping is clear. Overall shape as well as rim detail and decoration also vary within each class to such a degree that within-class variation can be as

Newari name	*Nepali name*	*Function*	*Ware*
FIGURE 1—Clay vessels			
1. Khayata tañ tankegu*	—	Fowl fountain	Red burnished
2. Bhājañ*	Hāndi	Cooking pot	Buff
3. Ghañ*	Ghaiñto	Small store jar	Buff
4. Chaghañ*	Ghagro	Water pot	Buff
FIGURE 2—Clay vessels			
1 Thēki (2 mana)*	Theki	Milk pot	Red burnished
2. Thēki (1 mana)*	Theki	Milk pot	Red burnished
3. Thēki (large)*	Theki	Lassi pot	Red washed
4 Bhyagañ*	Bhinoto	Domestic bowl	Coarse
5. Anti	Anti	Decanter	Brown-black burnished
6. Gulloo	Doobli	Rice spirit pot	Brown-black burnished
FIGURE 3—Clay and brass vessels			
1. Twaduwa*	—	Lamp	Buff
2. Gompacha*	—	Offering pot	Buff
3. Apacha*	Apacha	Death ceremony pot	Buff
4. Palla*	Palla	Offering saucer	Buff
5. —	—	Clay stamp for pot decoration	Buff
6. Maka*	Maka	Brazier	Buff
7 Chilan	Chilan	Tobacco holder	Brown-black burnished
8. Chilan	Chilan	Tobacco holder	Buff
9. Maka*	Maka	Brazier	Buff
10. Pato	Pato	Stopper	Buff
11 Pato	Pato	Stopper	Buff
12 Acharkulo	Acharkulo	Pickle jar	Brown-black burnished
13. Tañpha*	—	Drinking vessel	Brass
14. Kharuwa	Kharuwa	Drinking vessel	Brass
15. Khāshi } Jānsi }*	Kharkulo	Large cooking pot	Brass
FIGURE 4—Brass vessels			
1. Foshi	Foshi	Pan especially for distilling	Brass
2. Bata	Bata	Large cooking and serving pan	Brass
3. Kopara	Kopara	Chamber pot	Brass
4. Ghañ	Ghagri	Water pot	Brass
5. Kasauri*	Kasauri	Cooking pot (dhall)	Brass
6 Tasaula	Tasaula	Cooking pot (cheaper)	Brass
7. Pyudani*	Fika dani	Spittoon	Brass
8. Amkhora	Amkhora	Drinking vessel	Brass
9 Kasauri*	Kasauri	Rice cooking pot	Brass
10 Jwāla Nakon*	Nakon	Mirror used in washing	Brass
11. Surkunda	Surkunda	Lamp with Ganesh and snakes	Brass
12. Kalañ	Kalañ	Carrier for ritual vessels	Brass
13. Twaduwa*	Twaduwa	Lamp	Brass

* Many of the names of these lists were most kindly checked for me subsequently by Dr M. R. Allen of the Anthropology Department, University of Sydney. Those marked with an asterisk are confirmed Newar words.

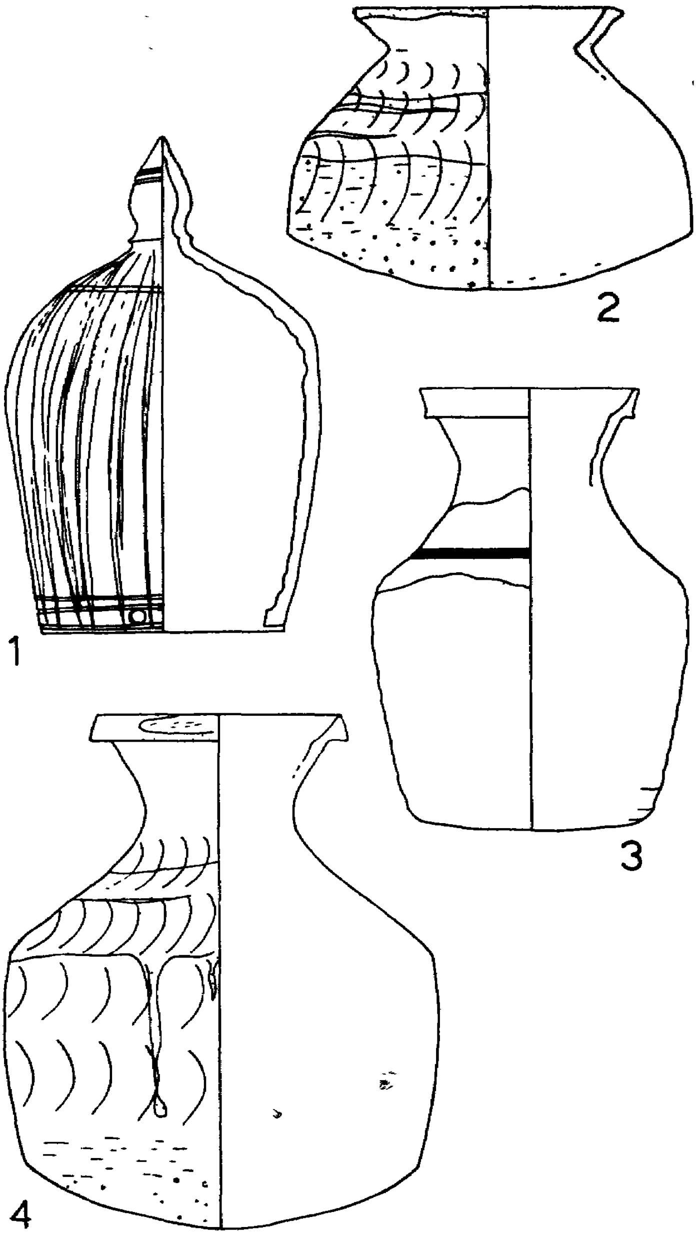


FIGURE 1

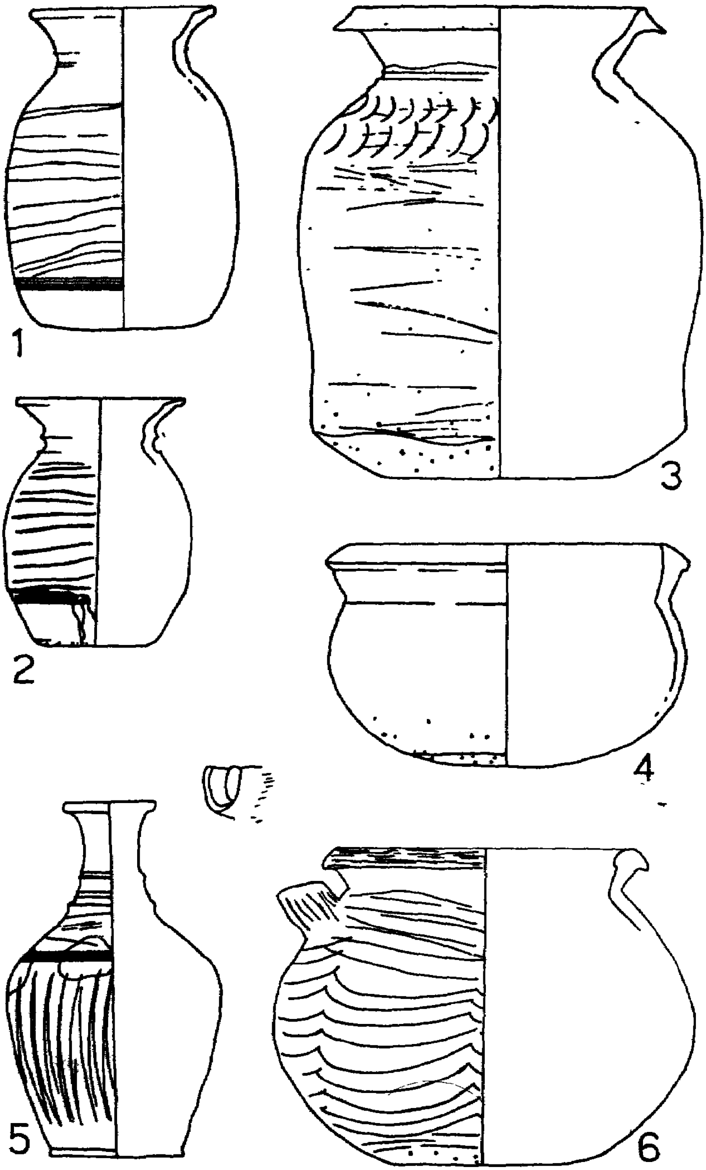


FIGURE 2

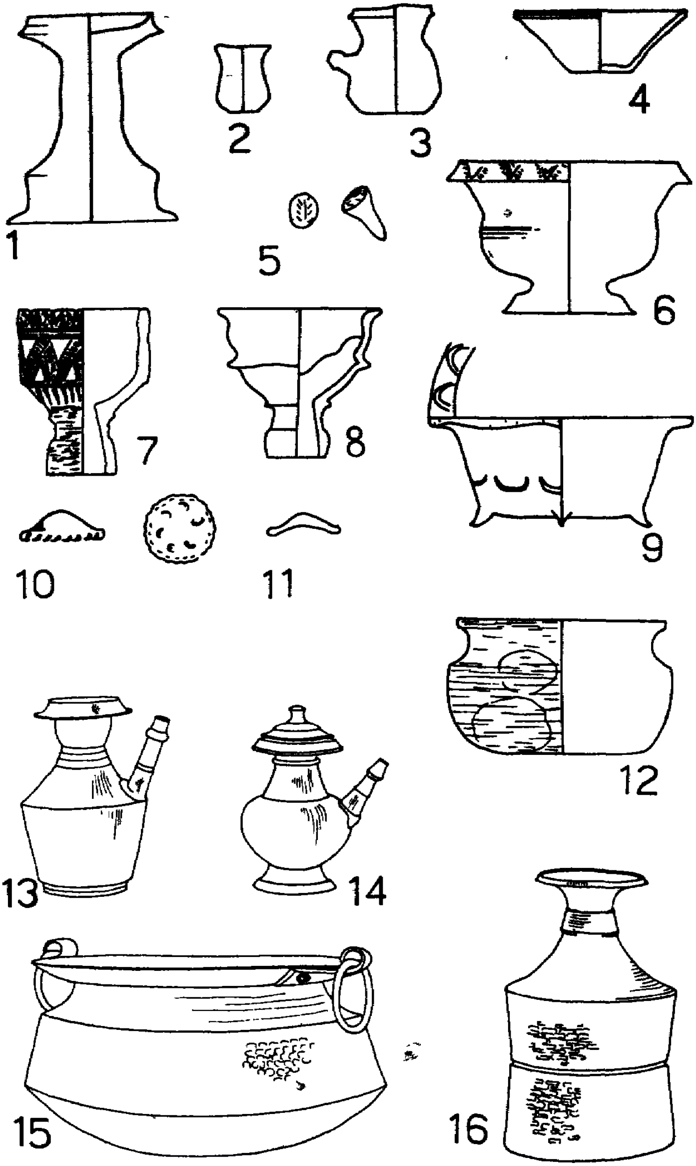


FIGURE 3

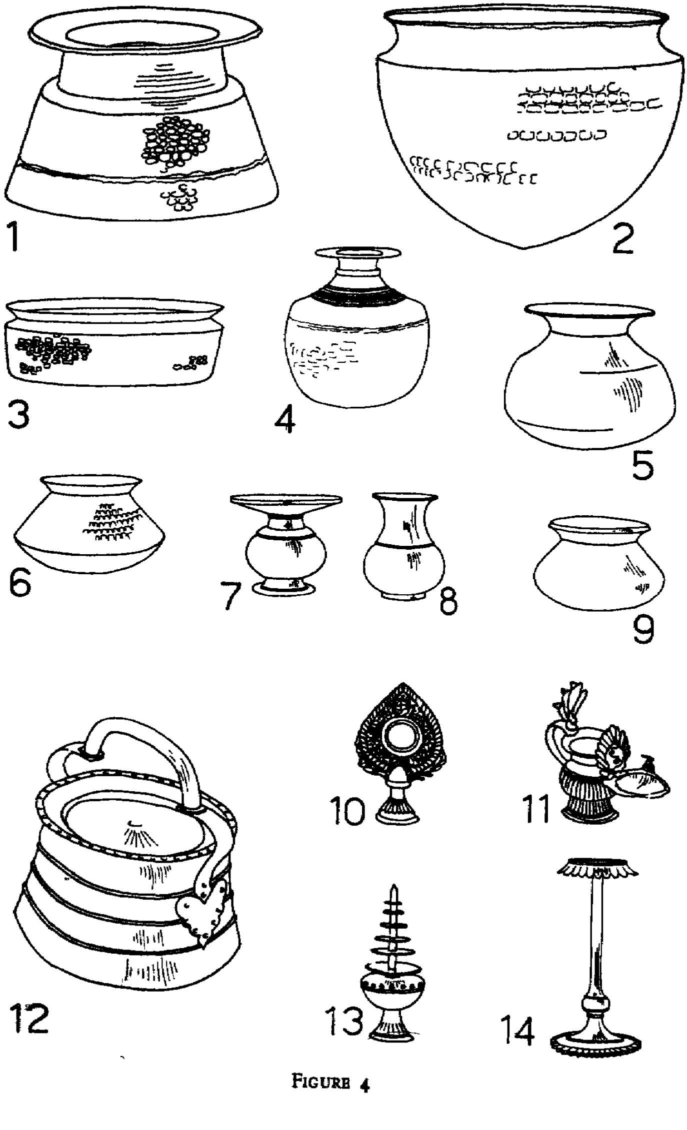


FIGURE 4

great or greater than inter-class variation, according to the potter's assessment of fashion trends of the moment. Round or flat base, angular or rounded shoulder, and double, ledged, undercut and bevelled rims can occur on either *ghagro* or *ghaiñto*. The fact remains that there is a recognisable ideal template of each which both potters and consumers are aware of, even while they are affirming as either *ghaiñto* or *ghagro* a vessel demonstrably different in almost all features from what they themselves describe as the ideal type.

The *hāndi* comes in two sizes, and is one of the most standardised forms. The valley people use it especially for roasting buckwheat, subsequently pounded into flour, the mountain people more commonly for boiling.

All these shapes are first started on the wheel and successively beaten out to their final shape, the smaller ones requiring three stages of beating, the large *ghyampo* requiring no less than five All are buff and porous when completed with bands of matt red slip as decoration; the large *ghyampo* and the *ghagro* sometimes also have applied rows of stamps (made with a small clay stamp) coloured white with chalk.

These are the general-purpose household containers, and it is these, especially the last three, which are made and sold in the largest quantities, both in the villages to local and distant purchasers, and in the urban markets. Obviously the very large *ghyampo* are difficult to transport, so that fewer are sold over a distance.

2 *Containers for milk products*

The properties of clay are considered particularly suited to a variety of uses for milk. Very common is the *thẽki* of either 1 or 2 mana, i e. half or one litre (fig 2, 1–2). These have a sack-shaped body, constricted neck and wide rim, and are used mostly for bringing milk into the town for selling. They can be re-used indefinitely provided they are thoroughly washed. A significant feature of them is that they are bright red-slipped, and are partly burnished Sometimes they have black patches on them from uneven firing—said by the potters to be the result of 'burning', when the wind catches one side of the bonfire causing it to burn too hot and fast.

Another vessel is the *dhau bhyāgañ* like a porridge plate, used to set curd. These again come in varying sizes, with diameter according to need. The common ones for sale are about 20 cm across. A third pot is the *lassi* jar, or large *thẽki* (fig 2, 3), a heavy, exceptionally well-made pot with flaring neck and bevelled rim, into which curd and water are put to make *lassi* The largest can be 20 mana, and they range down to 4. The large ones, being thick, are said to be very strong, and are comparatively expensive (3 rps) They are coated with a dull brown-red wash, but not burnished, on the stated grounds that the butter fat will produce the same effect as burnishing, i.e. decreased porosity. In all these forms clay is specifically said to have a beneficial flavour. Butter however is not put into clay, since the best butter is firmly asserted to come only from *dharharpe*, wood pots.

3. *Rice beer preparation, storage and drinking vessels.*

Rice beer is prepared from fermented rice in large *ghyampo* in which it is also stored. If taken to the fields or drunk in the home it is often poured from a heavy wide-spouted jug, with a strap handle to assist in pouring. These jugs called *swoña* (pl. 3, 3) are like the majority of vessels used for alcoholic liquids in the

Kathmandu Valley, black and highly burnished, often with ornate pattern-burnished designs as described below in the *rakshi* pots. Both spout and handle forms distinguish the beer jugs from the *rakshi* decanters which have a finer spout with small pouring hole, and small or no handle (pl. 3, 1, 2). The traditional drinking vessel is the small saucer or *sali* (*dabaka*), the more recent form the *glas*, or beaker.

4. Rakshi *preparation and drinking vessels*

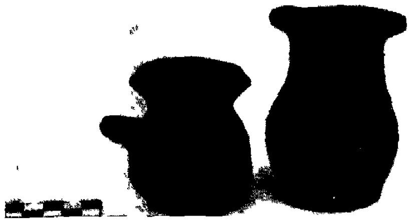
A considerable proportion of rice beer is not drunk in its primary state but is distilled to make *rakshi*, an essential part of all Newar ceremonies and festivals. A number of specialised pots are used in making, storing and drinking *rakshi*, and it is perhaps noticeable that they are mostly distinguished by certain unusual characteristics. They are most desirably black rather than buff or red, and to achieve this they are fired twice—first normally, then for a short time in a very smoky fire. This is said to make them very strong, so that they last well—and the best *rakshi* is considered to be that which is stored in an old black *ghaiñto*. Secondly, they are usually elaborately burnished, and may even be carved and fluted.

For distilling a set of four pots is required—a brass or copper *foshi* (fig 4, 1) half filled with rice beer, which is set immediately above the fire, with a large clay pot called a *petassi* above it with holes pierced in its base so that the vapourising beer can rise through it to condense against the underside of a third large pot on top of the *petassi*—this time another copper one, a wide shallow *bata* (fig 4, 2) or *chaubbata* filled with water. The fourth pot is a clay one called a *gulloo* (fig 2, 6) of a distinctive spouted shape in dark hand-burnished clay, which is placed within the *petassi* to catch the condensing drips Again it is traditional that the best *rakshi* is made in an old black well-burnished *gulloo*. To make the best *rakshi* the process is continued for 15–30 minutes and the water changed twice (pl. 7)

When made, the spirit is stored in a *ghaiñto* (pl. 3, 5 and 6), if possible a burnished black one At festival time the spirit is dispensed from a variety of elegant plain narrow-necked carafes or narrow-spouted decanters called *anti* (fig 2, 5: pl. 3, 1 and 2), poured through a narrow hole from a height to give maximum aeration. These are demonstrably imitative in shape and ornament of traditional gold, silver and copper examples of apparently Sino-Tibetan association. The potters' explanation of the desirability of burnishing on these pots is that it makes them less porous so that they do not soak up all the wine.

5. *Pickle jars*

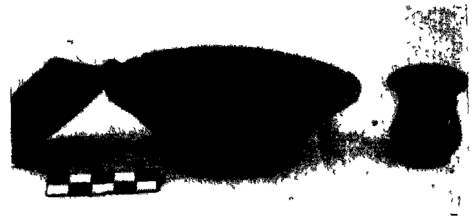
Associated with the pottery connected with alcoholic beverages is the very interesting and varied range of pots made for pickle storage and serving, the *acharkuli* (fig 3, 12: pl. 2, 3–6). They also are usually either black or red burnished, sometimes pattern burnished, and very nicely made, with maximum attention given to the need to exclude air and cut down on evaporation through the pot walls. Many kinds of pickle are made, from mangoes, lemons, cucumbers and *lopshi*, and the potters affirm that even a soft pickle like lemon or *lopshi* can be kept up to a year in one of these pots. Clay is considered essential as a container for the best flavoured pickles. Shapes of the pickle pots are also interesting in that, like the *rakshi* decanters, they exhibit considerable individual variation.



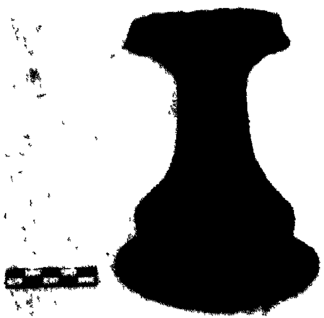
1 *Kulncha* (right), *Apacha* (left)



2 *Dhupouro*



3 *Palla* (left), *Sali* (centre) and *Gompacha*



4 Lamp

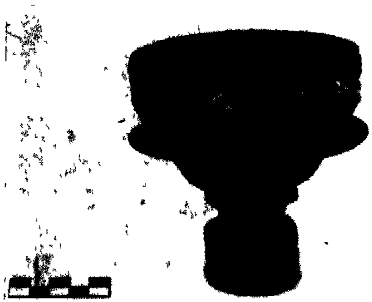


5 *Saraswati Dhara*

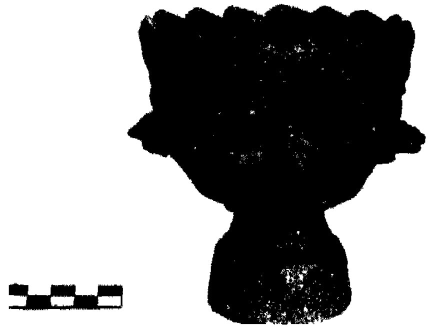


6 *Dhupouro*

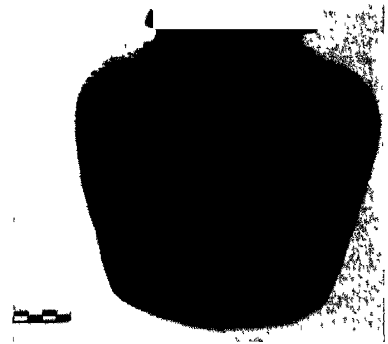
PLATE 1 Ritual vessels.



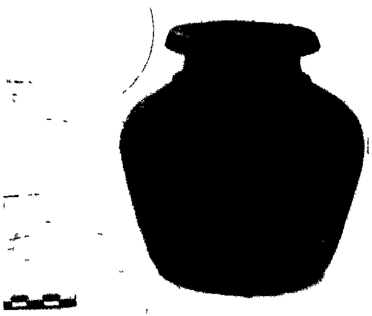
1 *Chilan*



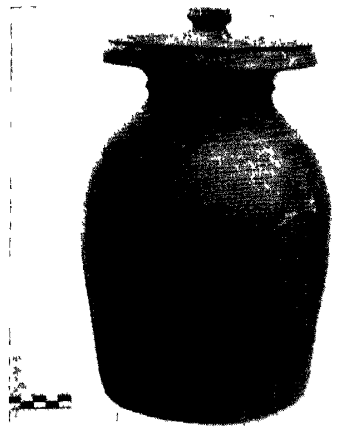
2 *Chilan*



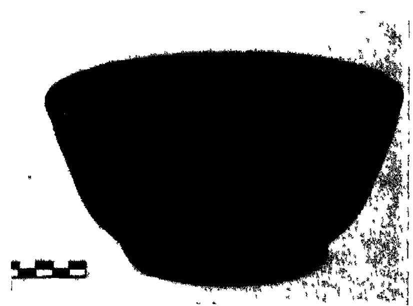
3 *Acharkulo* (pickle pot)



4 *Acharkulo*



5 *Acharkulo*



6 *Acharkulo*

PLATE 2



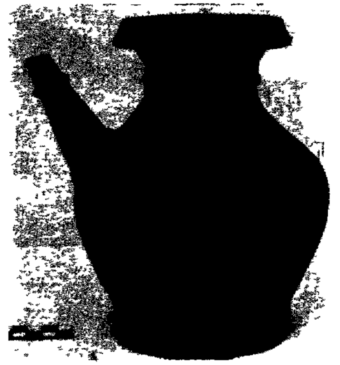
1 *Anti*



2 *Anti*



3 *Swoña*



4 *Tañpha*



5 *Ghaiñto*



6 *Ghaiñto*

PLATE 3.



PLATE 4 *Ghyampo* stacked against wall in Bhadgaon with spouted *gulloo* in front *Ghañto* are drying on straw in foreground

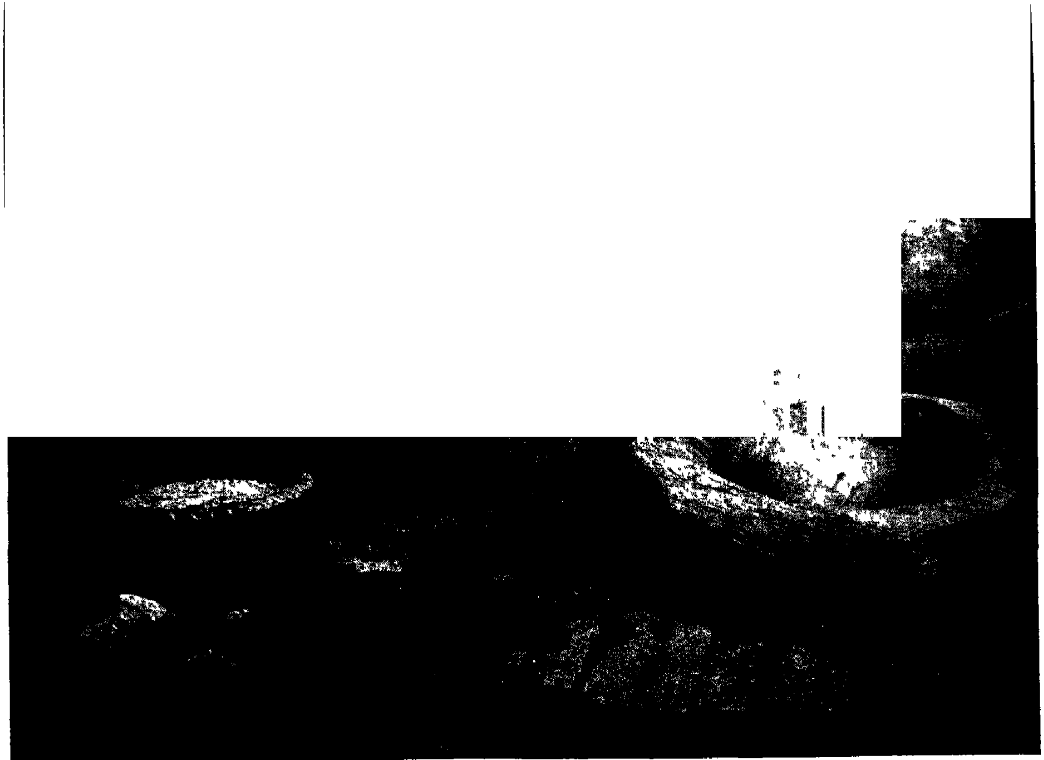


PLATE 5 Potter's equipment at *Thimi*—wooden base (right) on which pots are worked and mould for large bowls, knobbed anvils and a wooden beater (centre), and bowls containing sand, ash and water (left) used in beating process At extreme left is the first stage of a *ghyampo* with completely formed rim, and body as yet a solid lump of clay

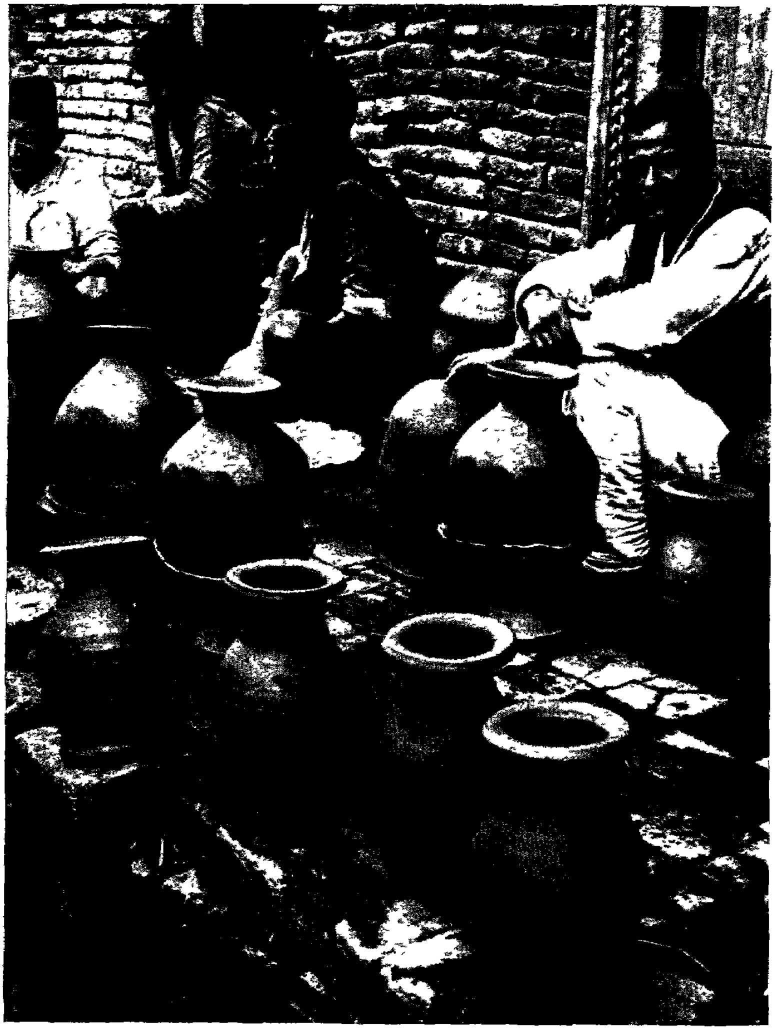


PLATE 6. Potters of *Thimi* Stage 1 of the *ghagro* (foreground) is flat-based with thick finger-ridged body. Stage 2 shows the body thinned by the beater and the base rounded Note variety of rim forms.

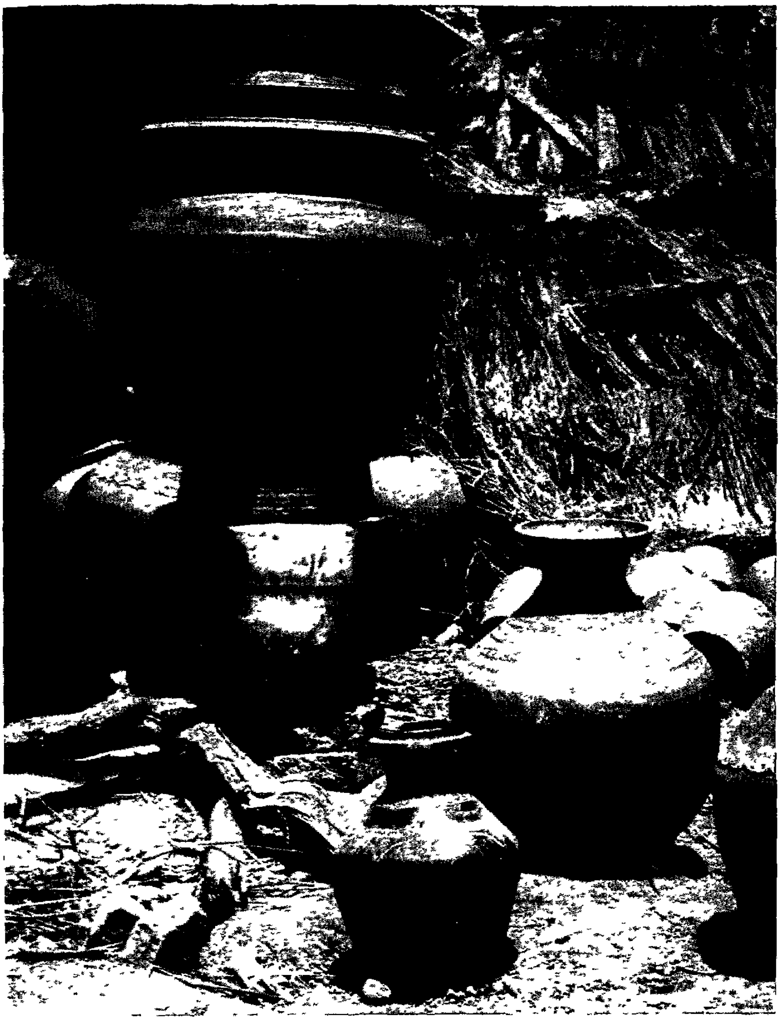


PLATE 7 Distilling *rakshi* The fire is set below the bottom vessel (*foshi*), on which is *petassi* (clay) containing the *gulloo* inside it The copper *chaubbata* is on top containing water. There is rice beer in the *foshi*, and the *gulloo* collects the condensed *rakshi* In the foreground is a decorated *ghaiñto*



PLATE 8. Baskets on sale in Patan

6. *Miscellaneous domestic vessels*

Whenever financially possible rice is boiled in a brass *kasauri* (fig. 4, 5 and 9) or the cheaper *tasaula* (fig. 4, 6), the *hāndi* being kept predominantly for maize or buckwheat roasting and flour-making. The *bhinoto* (fig. 2, 4) is a rounded bowl used for cooked foods and as a general container—potters use it secondarily for water or mica beside them at work. Several forms of small brazier, *maka* (fig. 3, 6 and 9) are made in the same shape as the small four-legged bamboo baskets, and there is also a small closed oven. A much larger bowl, the *athañ*, is used for washing clothes. An interesting universal form to find here in the Kathmandu Valley is the fowl fountain (*Khayata tañ tankegu*) (fig. 1, 1) consisting of an inverted bell chamber in a saucer. Two varieties at least are made, with pointed top or loop-handle, all in burnished red ware, heavy and well made. Finally another universal series, the range of flower pots and flower vases, is commonly decorated by finger indentations around the rim. Both straight-sided and globular forms of flower pot are found, the latter with wavy indented rim.

7 *Smoking*

Two main forms of clay funnels are used by the male villagers in smoking, which they do frequently as a form of relaxation. A cone-shaped form about 10 cm long is known as a *chilim* and is commonly used for hashish although it can also be used for plain tobacco. Hashish is normally smoked by the older men sitting around in a circle of fifteen to twenty near the temple, and is considered relaxing after a day's heavy work in the fields. The larger *chilan* (fig. 3, 7 and 8) and *pato* (fig. 3, 10 and 11) is always used for tobacco, never hashish, and is attached by a wooden or silver pipe to a water pot, which has a mouth-piece attached. Tobacco is placed in the bowl, the *pato* placed above it, and charcoal packed over the *pato*. The *chilan* come in several styles, one with heavily micaceous clay which gives the appearance of gold-dust and applied stamped designs, another in dark burnished clay with triangular cutout designs reminiscent of traditional wood carving.

8. *Household worship*

A variety of small and humble vessels are used in household worship. The *palla* (pl. 1, 3) is a tiny saucer used to burn mustard oil or ghee with a cotton wick. The *dhupouro* (pl. 1, 2 and 6) is a handled bowl used for burning incense powder in worship. There is a small saucer lamp on a pedestal (fig. 3, 1) and tiny pots for offering a grain of rice and water (*gompacha*). All of these cost only a few paise each and are accessible even to the very poor for worship.

9. *Temple worship*

For the more hazardous situation of carrying offerings to the temple metal is preferred whenever possible, notably for lamps (*surkunda*) (fig. 4, 11), mirrors (*nakon*) (fig. 4, 10) and the *kalañ* (fig. 4, 12) a brass lidded and handled bucket in which the small clay pots for presentation are carried.

10. *Special ceremonial occasions*

In addition to the standard daily pots in use, certain shapes are made for special festivals or family occasions. The *kulincha* (or *kalash*) is a small pot which is placed at the four corners of a new house containing water, rice, peepal leaves and flowers

for good fortune. The wealthy use copper ones. The *apacha* is a very small spouted pot given to the Brahmin who performs death rites, thirteen days later (pl. 1, 1). The *saraswati dhara* (pl. 1, 5) is an interesting pot with many holes in the body. Straws 6–8 cm long are stuck into these, so that when water is poured into the pot it pours out through all the straws. It is used in celebration of the goddess Saraswati. It is a characteristic of many of these ritual vessels that they are thin and usually carelessly made. Moreover all are made in the buff clay–red paint tradition rather than the black burnished one; one informant commented that no burnished pot could be given to a Brahmin in connexion with a death.

II

The potters and their techniques

A preliminary study only of the ceramic tradition among these potters was possible, since it was both restricted in time and confined to one part of the year. Obviously such a study is necessary for any estimate of the range of choices open to the potter. It also incidentally offers a promising approach towards adding a historical dimension to the contemporary picture; the distribution of specific techniques, at least in south Asia, has so far proved a more reliable indicator of the past movements of potters and their families than most stylistic and functional features. B. Saraswati's India-wide study (1964) has made a welcome beginning in this area, and has also listed the most useful technical processes for comparison.

The wheel. The wheel used by the potters especially in Bhadgaon is the solid (or block) pivoted wheel, turning about a socket set in the ground. In Thimi truck wheels with rubber tyres are also used.

The clays. The body clay used by the potters comes from fields in and around the village. The best clay comes from the deeper layers, is dark grey in colour, and is seasoned for a year before use. It is then prepared by 'fighting' or kneading, and Ram Gopal of Thimi said that for four *lassi* pots of 20 *mana* each he would have to fight the clay for one hour. Yellow clay for the slip comes from the mountains nearby, two days' walk away, and one potter said that to bring back as much as he could carry was about six years' supply. This is called *lancha*. After the application of one coat the pot will be yellow after one firing, with two coats red after one firing. It is only applied over the top and sides—i.e. where it will show. Also from the mountains comes the *pangra*, a shiny seed about the size of a large walnut used for burnishing, which the potters consider their lucky amulet.

Forming. The rim and side of the pot are formed on the wheel. For large pots in this stage the sides are extremely thick, forming a solid lump: this lump will be beaten in a series of processes designed to thin out the sides and strengthen the clay. The beating is done with square-section wooden beaters against a small knobbed anvil in a wooden block base, and when a beating stage is completed the pots are stood in an upturned broken pot. Micaceous sand is used to prevent sticking (pl. 5). Each stage of beating produces a specific change of shape, and the crescentic paddle marks form a regular series of rows around base and shoulder. In fact they are removed at each stage by careful scraping with an old sherd; often small particles

become trapped in this process and drag characteristic small scratches horizontally around the pot—a sure sign of the technique.

Decorating. After beating is complete comes the decorating, which may be burnishing, carving with a sharp implement, or stamping with a clay stamp (fig. 3, 5). The tobacco pipes of Thimi have elaborate carved decoration, while *ghagros* and *ghyampos* of Thimi, as well as the red tobacco pipes can be stamped with rosettes or geometric designs, the latter being then covered with chalk which fires white.

Firing. Firing can take place in either small or large bonfires. Ram Gopal preferred to keep all his pots for a massive bonfire in December covering an area 12 m square in an open field but the potters of Bhadgaon had both small and medium sized bonfires once or twice a week from September onwards as required. These small fires were conical in form, kindled under a series of straw-roofed lean-to structures. All are fuelled with straw, the usual arrangement being in a square, the pots around the edge with mouths facing inwards. Straw alone is placed in the middle. No additional straw is added during the firing which lasts three days altogether, and the amount is carefully calculated so that when it is consumed the pots are done. The whole fire is covered with ash throughout. After one firing the pots are red If they are required to be blackened they are left in with additional straw underneath, a little straw between and extra ash on top to make a very smoky fire The ash is extremely important in all firings and is kept from one year to the next. Sometimes red pots come out with black patches, and this will be on the hottest side in the first firing, where wind has shifted the ash.

Rituals The potters' year ends with the monsoon (July-September) and there is no firing during the heavy rains Their main ritual occurs during *Dussehra* (*Dasain*) in October, when sacrifices of chicken or duck include blood smeared on the wheels, and this can also be repeated again during the year if anything goes wrong with the firing

It is clear that in the Kathmandu Valley the potters have a very wide range of technical skills, that they consciously select from them, and that they are particularly adventurous in the way they use them. There is no cultural pressure against innovation, in fact the reverse, and they appear to cater to an interested and discerning public. A noticeable characteristic was the potters' own awareness of their own skills, and their ability both to describe and to analyse them.

A comparison with Indian techniques reveals only a general relationship with north India of about half the features compared; however documentation is still at a most general level. The beating method is that called by Saraswati the North-west method, with closed base. The system of open fire is common to the western Himalaya, much of central India and Rajasthan. The use of the knobbed anvil is slightly more restricted, covering the western Himalaya, Punjab, Rajasthan, UP and Bihar. At present there are no comparative dates for the red and black burnished wares with their often elaborate decoration.

III

The major point that emerges from this brief investigation is that the dominant factor affecting the potters' output is the need to sell their pots in a competitive

situation. All those questioned maintained that they made every type of pot rather than specialising in a few specific shapes because then they could sell more pots. Similarly the minor variations in details of profile, rim form and decoration on the basic pottery repertoire were consciously designed to attract the widest range of customers. Not only can this be seen in items in which personal taste might be expected to dominate—for example, in tobacco pipes, or beer and *rakshi* decanters —or in more lasting household vessels such as pickle jars, but even in the everyday water pots and big store jars. One potter in Thimi had no less than four different rim forms in the batch of otherwise identical water pots (*ghagro*) he had drying in front of his house. He firmly denied that any of them was stronger, more drip-free or easier to handle than any other. Some customers liked one kind, others another; he wanted to sell to them all Another had square-shouldered *ghagro* alongside the more usual round ones outside his house—a new fashion which some preferred, he said.

Thus there was little discernible variation in the types of pot produced from one potter to the next, or from one village to the other, except in one aspect which was itself interesting. It was generally agreed that the potters of Thimi made more of the common water and beer jars (*ghyampo*, *ghagro* and *ghaiñto*) and the large pierced pot for distilling (*petassi*) because they were closer to the large urban market of Kathmandu. The potters of Bhadgaon produced these shapes in smaller quantity and sold them mostly to the inhabitants of the eastern mountain villages when they came down to the Valley to sell their own wares. The few miles separating these villages, scarcely noticeable with modern transport, appeared to be of considerable significance in a society where all loads are carried by man. It is incidentally noticeable that the same factor of distance cut down to a marked degree the range of pots bought by these mountain villagers. Not only personal taste, but also concept of specialisation in containers were non-significant in their relationship with the potters—they buy only the simplest *ghyampo*, *ghagro* and *hãndi* to take back on their six-day walk.

Again, in the use of decorative techniques, while all potters asserted that they all knew how to do all the techniques—pattern-burnishing, rouletting, incising, stamping, cutting or carving as well as the double-firing for dark pots—they were all equally prepared to admit that some were better at some forms. Several justified this differentiation in potter's skill by related statements concerning a lack of consumer appeal for items which they did not feel able to do well. One potter in Thimi, Ram Gopal, was widely held to be among the best at carving pots in either village, most of the carving and indeed much of the best decorative work being on the black pots. Black smoking pipes are often elaborately carved, the red clay ones more often rouletted or stamped, sometimes mica-dusted. Black *rakshi* and beer decanters are also often carved with gadroons and fluting, and usually pattern-burnished, or burnished to a high lustre. Burnishing also occurs on red pots, such as milk pots, fowl feeders, some *rakshi* decanters and particularly on red pickle jars of various shapes, but is not found on the whole range of buff and red-washed porous cooking and ritual pots. The impression remains that while virtually all the potters—virtually all Newar and all Hindu—are able to make all kinds of pot with all kinds of decoration, there appears to be a marked double strand in the ceramic tradition between, in general, the burnished red and black slip *rakshi* and beer

containers, pickle jars and milk pots, and some of the *chilim* on the one hand, and the matt buff or buff red-washed *hāndi*, water pots and the whole range of ritual pots on the other. The latter group, both by shape and in techniques are clearly related to north India and may be assumed to have been introduced into the Valley from there. The former, obviously at least partly derived from a wood-carving tradition and also closely allied to metal forms, seem to have a different origin. Further work in this area of study seems promising.

A number of other points of interest arose from this study, brief as it was, especially for those concerned with ceramic systems in archaeological contexts.

For example, analysis of the total ceramic repertoire reveals a surprisingly full picture of village life, mostly but not wholly concerned with domestic economy. The size and quantity as well as the shape of the clay vessels found can be revealing of family size or wealth, seasonal activities or social groupings. The large store jars as well as the water jars are made in a full range of sizes, as are the cooking pots, the curd bowls and the *lassi* jars. Clay pots are preferred for pickles, of lemon or cucumber or *lopshi*, and a wide range of shapes, sizes and qualities is available to the family that wants them. Houses are equipped with clay braziers, ovens and fowl feeders if required and flower pots and vases for ornament. Clay pipes for relaxation come in many forms, and are a recognised area in which the customers' tastes are particularly important. In the large number of vessels associated with the making, storage and drinking of *rakshi* there is an interesting distinction between the standardisation of the vessels used in its production, and the range of decorative forms found on the drinking carafes and the storage pots. At the same time both smoking and drinking vessels are sociological indicators, since no strict high caste Hindu uses either. More indicative of ritual life is the comparatively high proportion of vessels made specifically for ceremonies and worship. Tiny offering pots, lamps and *dhupouro* for daily worship are accompanied by special pots for house dedication, and for presentation to the officiating Brahmin after a death in the family. This area of investigation appears to be a particularly interesting one to pursue, in view of the ambivalent religious situation in the Valley. So far Hindu ritual pottery forms a distinctive part of the repertoire. The significance of the purchase and use of these pots remains to be further investigated, as does the possibility of any vessels which may be used primarily by the Buddhist community.

Again, if the average archaeologist were to attempt a conventional classification of this repertoire without the aid of informants it is very soon clear that reliance on morphology of the pots and stylistic variation in rim details and decoration would be of little assistance, especially if the sample should be confined, as so often happens in an archaeological situation to rims and part profiles. On the other hand, a classification which also utilised easily observed details of technique such as the use and nature of slip or burnish or both, the thickness of the walls, the presence of paddle marks, striations caused by scraping with a sherd and the use of double-firing would yield, in this instance at least, a far more significant functional picture. The information given by the potters themselves makes it clear that such details are consciously related to the ultimate use of the pot. Double firing used to produce brownish-black rather than the normal buff or red-slipped wares is thought by most of the potters to give extra strength, while burnishing is consciously applied to vessels in which evaporation of the contents is to be avoided. A combination of

these two is therefore usually used on pots required to have a long life or to preserve precious contents in, e.g. pickle pots (which can preserve their contents up to a year) and *rakshi* containers which are often highly decorated. An unpolished wash is used on *lassi* pots in which butter fat will do the same job of clogging the pores of the clay as burnishing would do. They are made especially heavy with thick walls and of good quality, for constant domestic use. On the other hand, small drinking saucers, offering dishes and pots, and those made as payment are feather light with thin walls, and would not be expected to last for any length of time. Paddle-marks on store jars, water pots and cooking pots, usually partly but not completely obliterated, merely indicate that the vessel is large in comparison to the thickness of its walls, which must then be finished and thinned by beating. The same technique is indicated by the presence of mica specks on the bottom and lower sides of such vessels, for micaceous sand is used freely while the base is being beaten to prevent sticking. One further feature is detectable on these pots which the archaeologist would doubtless use in his classification even if he could not infer its significance—a series of irregular horizontal striations produced when a sherd is scraped round the lower body to remove all trade or paddle marks. Paddle marks are sometimes left visible on the shoulder—no-one had any satisfactory answer why. Neither paddling nor scraping with a sherd appears on the burnished pickle jars or the *lassi* and milk pots (which of course are intentionally left fairly thick).

In fact function remains the dominant aspect of the pot for both potter and consumer. Classificatory terms used by the consumers and potters themselves are predominantly functional, with minor complications. For the most part one vessel type or shape has one function and one name, especially among the ritual vessels. Often a form like the *hāndi* will come in several sizes, to which the same name *hāndi* is applied. In some cases however variation in size itself is significant enough to denote a new type—for example the first explanation given to me of the difference between the three liquid storage jars—*ghyampo*, *ghagro* and *ghaiñto*—was one of size, i.e. in general the *ghyampo* is large, *ghagro* middle-sized and *ghaiñto* small On further questioning it became clear that in terms of actual measurement the three classes could overlap quite considerably whereupon I was told that it was a combination of shape and size; when I pressed further in attempting to identify the precise shape variations concerned, i e., flat or round base, rim forms, nothing could be selected as a permanent and identifying shape characteristic of one or another. However, the fact remains that all agreed instantly on names for any given pot, and I also within a few days could come to the same conclusion. In general the size factor was important, and *ghagros* were more often round-based and *ghaiñtos* flat-based. Finally, sometimes the same name is applied to several pots varying not only in size but also in shape, but with the same overall use, e.g , the name *thēki*, which is given to a graded range of milk pots, and also *lassi* pots of totally different shape but related use. This is also true of personal water and wine pots. The name *anti* is used in precisely the same way as our terms carafe and decanter, to denote a wide range of vessels which can vary widely in shape and decoration, some spouted, some narrow-necked, but all of which are specifically used for pouring small quantities of liquid carefully. *Acharkuli* equally applies to pickle jars which again may vary widely in shape, but which again share certain common characteristics—this time general squatness, fairly small opening and lid, and a burnished exterior.

IV

In conclusion it must be said that while the pottery repertoire of any group obviously has to be studied on its own from the point of view of the ceramic tradition and the potting skills involved, it must equally always be viewed in the context of the total container repertoire. The Kathmandu Valley is no exception. Wood, metals and basketry and strong woven bags complete the variety of containers used, and once these are added the cultural information yielded is considerably increased. Obviously for the archaeologist it is invaluable to find the full range of materials in current use with reasons for preference of any specific material available.

Metal, whether brass, copper, silver or aluminium, is preferred for a wide range of storage, serving and cooking containers for several qualities—its length of life, its versatility in shaping, its strength for large vessels, its cleanliness and its attractive appearance. Some of the range in normal domestic brassware, is illustrated on figs. 3 and 4. Rarer luxury pieces, more ornate decanters (*anti*), *ghaiñto*, *tañpha* and *kharuwa* in silver with brass decoration, or ornate bronze, together with similarly more elaborate ritual vessels—lamps, mirrors—can be still seen in antique shops and collections in Kathmandu. Household wares are in brass of varying quality, the heavy paler variety being the more expensive and desirable. Large domestic vessels, the *bata* and *foshi* and especially the *kharkulo*, are considered particularly auspicious, and are commonly given at weddings. The *kasauri* of heavy quality is used especially for cooking rice, the *tasaula* is a thinner cheaper imitation, often given to the Brahmin as payment. Metal is particularly preferred for the storage of cooked food. The brass *ghagri* is the all-purpose family water pot (fig. 3, 16), the spouted *kharuwa* a personal drinking vessel usually carried around all day (fig. 3, 13 and 14) Alternatively the non-spouted *amkhora* is used as an individual drinking vessel (fig. 4, 8). These are the major general vessels. The *chaubbata* is used specifically in making *rakshi*, while the *surkunda* (lamp) and *kalañ* are used in daily worship. In general metal vessels are preferred for taking to the temples because of the greater wear and tear involved; clay ritual pots take their place in the home. The *kalañ* is especially used to transport offerings to the temple. Other forms—ladles and cooking utensils, spittoon (fig. 4, 7) and chamber pot (fig. 4, 3) are also made It is notable that several of these shapes are only used in metal—the *kalañ*, *foshi* and *bata* do not appear to be copied in clay.

Bamboo basketry is widely used for carrying containers, because of its lightness and strength Strong openwork light baskets are used particularly around Kathmandu in pairs, slung from a carrying pole (*kharpan*), for the transport of goods to and from market, fertiliser to the fields, fuel, and building materials. Larger single conical baskets are carried on the back for long journeys (*doko*, *thunse*). Both kinds are of varying quality, and are carefully selected according to weight of load and/or expected length of life. Baskets of closer weave, also circular but often supported on four small bamboo legs (*dalo*), are used in the house for storage of dry goods—chilis, grains, lentils, flour, presumably because of the aeration permitted by the weave.

Wood is preferred for tea bowls (usually lined and lidded with silver), and in the kitchen for oil jars and butter pots. It is specifically thought to impart a good flavour to these items, neither oil nor butter containers appear to be made at all in

either metal or clay. The shapes are varied, but share the universal features of lathe-turned wooden vessels—knobbed lids, turned rims and bases, cylindrical form, and decoration usually by means of sets of fine grooves encircling the pot and lid. Sometimes bands and medallions of brass are added externally. The contents in both instances preserve the wooden vessel, and many were said to be several generations old.

Clay is then seen in its turn to be used for its own positive qualities—its cheapness and availability, its cooling properties and refractory properties, its strength and versatility in forming and its flavour, while its corresponding disadvantages are equally recognised—its friability and lack of cleanliness for cooked foods. Its uses can be grouped into three classes. First, clay is used wherever suitable for making cheaper copies of brass, copper, aluminium and silver vessels for families which cannot afford the investment of 40 or 50 times as much for a non-breakable water or cooking pot—however much to their advantage it would ultimately be. Equally it is used for providing extra pots in those households which already have some brass vessels—the two forms are not exclusive. Such clay vessels as the *ghagro*, *ghaiñto*, *anti*, *swoña*, and *hāndi* therefore follow exactly the forms and decoration of their metal prototypes. So also do some of the small vessels used in household worship—small lamps for oil, bowls for incense, small offering pots and saucers. In both groups the additional properties of clay are an obvious advantage, although they do not overcome the basic problem of its fragility. Secondly, in a smaller group come vessels intended to be disposable, either in a domestic or religious context, and therefore again exploiting the cheapness of clay. They are distinguishable primarily by their careless workmanship and thin walls. These include pots made for payment to Brahmins for priestly duties, festival pots like the *saraswati dhara* as well as daily offering pots and saucers, and tea and *rakshi* dishes thrown away after use. Thirdly, come those vessels made of clay which exploit the more positive properties of clay—*chilan*, *chilim*, braziers and ovens, flower pots and fowl feeders on the one hand, and a group of rather less obvious vessels on the other in which clay is desired for its beneficial effect on the contents—the *gulloo*, or spouted pot used in the collection of the spirit in *rakshi* making· curd bowls, milk pots and *lassi* pots (*theki*) which are daily scrubbed out and re-used and the *acharkuli* or pickle pots and bowls for which again clay is specifically preferred Not surprisingly it is more difficult to trace any prototypes in other materials for this third class, if indeed they exist

NOTE

[1] Figures 1, 2 and 3; 1–11 are made of clay, Figure 3, 12–15 and 4 are in brass or copper. Pots are drawn to quarter-scale, metal vessels approximately only quarter-size.

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STRUCTURE AND EVENT: WITCHCRAFT CONFESSION AMONG THE KURANKO

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Among the Kuranko of north-east Sierra Leone witchcraft accusation is very rare yet witchcraft confession is not uncommon. This article presents a structural analysis of witchcraft beliefs, based upon Kuranko exegesis, and examines the nature of the relationship between knowledge and event—between the socially constructed stereotypes of witchcraft and individual cases of confession. It is suggested that a structural analysis of a cognitive system must include praxeological considerations and that the idea of the witch is an arbitrary sign subject to individual manipulation rather than a definitive concept referring to a phenomenon *sui generis*.

The surreptitious character of witchcraft makes its detection and description difficult. But when witchcraft does become manifest its adduction is usually based on either accusation (specific or non-specific) or confession. Although confessions often follow covert or overt coercion by others, the distinction between confession-oriented and accusation-oriented witchcraft requires investigation (Douglas 1970*a*: xxxiv; Ruel 1970: 333; Wyllie 1973: 74–5). It is now known that accusation and confession are emphasised differently in different societies and in different sectors or contexts within a single society. For the Azande, Evans-Pritchard recorded: 'I have never known a Zande admit his witchcraft' (1972: 125), and 'I have only received cases of confession from one Zande . . . perhaps the least reliable of my informants' (1972: 118). By contrast, other societies, notably in west Africa, are characterised by a rarity or absence of direct accusation and the presence of confession: Effutu (Wyllie 1973); Banyang (Ruel 1970); Ashanti (Ward 1956; Field 1960). Yet other African societies are notable for the rarity of both direct accusation and confession: Korongo (Nadel 1952); Dinka (Lienhardt 1951); Mbugwe (Gray 1969).[1]

My purpose in this article is to present an account of witchcraft among the Kuranko of north-east Sierra Leone, a society in which witchcraft confession occurs but in which witchcraft accusation is very rare.[2] On the basis of this account, and referring to comparative data from other west African societies, I shall then endeavour to elucidate some of the social and psychological variables that influence the pattern of Kuranko witchcraft.

I

Witchcraft and psychopathology

In seeking to understand the meaning of witchcraft beliefs it is useful to recall the distinction which de Saussure made between the signified and the signifier. The

Man (N.S.) 10, 387–403

arbitrary or unmotivated character of the sign means that the signifier—in this case 'witchcraft'—may refer to a wealth of phenomenological states or mental representations which, in essence, overflow the terminological category which seems to confine and define them.[3] In other words, we should resist any tendency to regard witchcraft as a phenomenon *sui generis*. We must also allow the possibility that the phenomena which are designated by the term 'witchcraft' in one society may also exist in other societies but go under other names.[4] At the same time it is necessary to recognise that the same phenomenon or form of behaviour may, in a single society, be regarded as legitimate in some contexts or among one category of persons and considered to be illegitimate in other contexts or among another category of persons (E. Goody 1970 on the Gonja dichotomy between male and female witches; Brain 1970 on the Bangwa distinction between child and adult witches). It has been frequently attested that witchcraft arises from or signifies ambiguities and stresses which occur in peripheral or interstitial areas of social structure (Marwick 1965; Douglas 1970*a*). It is this structure which must be analysed before we can understand its figurative representations. It is my view that what is called 'witchcraft' in certain societies is already established at the level of *structure* before it occurs or is expressed at the level of *event*.

The subject of witchcraft immediately confronts us with questions concerning the relationships between normal and abnormal behaviour. Many writers have been able to show a logical complementarity between the two concepts of behaviour and to demonstrate that witchcraft is a socially-constructed pattern of beliefs which, at the level of thought, *systematises* a dialectical negation of what is regarded as normal (Middleton 1971: 238–50; Beidelman 1971: 37–9). However, at the level of individual experience, this systematisation may be less complete. In Western societies we tend to distinguish psychological disorders as defined subjectively by the patient and as defined objectively by the physician (Szasz 1973: 194) These two ascriptions are not always congruent or mutually consistent. Moreover, by relegating the subjective view of the patient to the category of *delusion* and assigning to the objectively-defined (i.e. the collective) category the status of *real*, we often tend to forget that all definitions of order and disorder are relative and arbitrary. This brings us to an important distinction which must be made if we are to understand the nature of witchcraft, namely that the alleged disorder or abnormal state is *both* an existential reality for the individual concerned *and* an objective, collectively-defined symbol for others.[5] I shall therefore seek to understand Kuranko witchcraft both as a system of signs (in which the individual witch is simply a category) and as an aspect of individual existence (in which the witch is a categoriser).

II

The pattern of belief

The Kuranko term for a witch is *suwaye* (lit. 'night owner') and the cognate term *suwa'ye* means 'witchcraft'. Another word, *sugbingbe*, is sometimes used to refer to minor witches who are active no more than once a year; they avoid contact with real witches who would kill them on sight. Children are particularly vulnerable to witchcraft and to witches' ghosts (*pulan*). Belief in the evil eye (*ya yugo me*,

lit. 'eye evil/bad') is less prevalent; the evil eye is generally regarded as merely one manifestation of witchcraft, a symbol of envy.

Apart from the denotative meanings of the terms 'witch' and 'witchcraft', the Kuranko often use these words to connote extraordinary and potentially dangerous powers. Europeans (*tubabunu*) are often likened to witches or wizards because their remarkable mobility (in ships and aeroplanes) and their various inventions and weaponry are thought to be evidence of hidden, mystical powers—dangerous because they lie outside Kuranko comprehension and control. Certain legendary heroes (notably Sundiata) are sometimes said to have been witches because their extraordinary powers exceed the conventional boundaries of understanding and existence. Similarly, the masters or leaders of witch-detecting cults are compared with the witches whose powers they seek to nullify. The jealousies and rivalries which characterise relationships between non-uterine brothers (*fadenye*, lit. 'father's sons' relationship') are also said to be potential causes of witchcraft. Wealthy people who do not share their good fortune with others are regarded as likely victims of witchcraft. But in all these cases witchcraft is simply a metaphorical mode for expressing the latent dangers of envy and greed; as we shall see, *actual* cases of witchcraft, involve only male–female or female–female relationships.[6] Male–male antagonisms, on the other hand, tend to involve sorcery.

Although sorcery (the use of 'powerful/dangerous medicine'—*besekɔli*) may be distinguished from witchcraft in terms of a distinction between external-controlled and internal-uncontrolled powers (Douglas 1970*b*: 119; Leach 1971: 22–5), in practice the distinction is not always clear.[7] A medicine owner/master (*besetigi*), according to one informant (himself a *besetigi*), usually specialises in one of three branches of medicine: 1. curative and protective (deals with afflictions and diseases 'caused by God'—*altala kiraiye*—and with antidotes for afflictions and diseases 'caused by persons'—*mɔrgo kiraiye*), 2. destructive (which involves the use of medicines for harming people), 3. destructive (in which the curse—*gbɔrle*—is used instead of medicines). Many Kuranko refer to diseases 'caused by God' as 'natural', thus distinguishing them from 'witchcraft' and 'sorcery' which are the result of human agencies. But my informant disavowed any knowledge of witchcraft and stressed that sorcery and witchcraft were quite different. He could only protect people from witches; young boys often stayed in his house when their parents felt that they were vulnerable to witchcraft attack. The second and third categories of medicine-use might be termed 'sorcery' but the *besetigi* usually works on behalf of a client and is not culpable for what happens to the victim of his medicines. If a person has a legitimate grievance against another man and all other avenues of legal redress are closed, then he may enlist the services of a sorcerer. Checks against the abuse of the powers of medicines and curses do, however, exist. If a grievance is entirely a matter of personal malice then the medicines will be ineffective against the victim and they will return to harm the person on whose behalf they are being 'sent out'. Before a sorcerer will curse a man his client must take an oath, declaring 'If I am unjust/wrong in my accusation against X then may I suffer; if I am just/right in my accusation against X then may he suffer.' However, a person can sometimes circumvent these checks by purchasing powerful medicines from a foreign sorcerer or from a *mɔri*-man (Muslim medicine owner) and then using them independently.[8] The curse, on the other hand, can never be purchased; it must

always be uttered by a *besetigi*.[9] Moreover, the curse must always be made public. Not only is it an extension of secular legal controls, its efficacy depends upon the victim knowing that he has been cursed; as one informant put it, 'You have to know that you have been cursed.' The curse is lifted only when the offender confesses or, if he dies, when representatives of his sub-clan pay the *besetigi* to do so.

The use of harmful medicines (*besekɔli*), independent of a sorcerer (*besetigi*), to bring shame, misfortune or death to an enemy is, in Kuranko thought, a consequence of malice, envy, spite and groundless animosity; such actions are socially illegitimate. It is this aspect of male medicine use which Kuranko compare with witchcraft. We should also note that not all female 'witchcraft' is solely the consequence of involuntary internal powers. Poisons (*dabɛri*) like *munke* (lit. 'gunpowder') and *pɛnkan* are sometimes put in a person's food or water. *Pɛnkan* is pounded in a mortar at midnight by a person standing on his head and using his feet to grasp the pestle; the pestle makes the sound '*pɛnkan, pɛnkan*'. This poison is allegedly used by women, notably old women, against anyone they hate, especially small children. The symptoms of poisoning are said to be distension of the stomach and black vomit and faeces; the poisons also turn food and water black.

Although the surreptitious use of harmful medicines or poisons is often referred to metaphorically as witchcraft, the Kuranko normally restrict the term *suwa'ye* to denote nefarious and secretive female activities in which uncontrollable addictions rather than external substances and objects are involved. Witches are always female and this belief is supported by the Kuranko attitude that women have weaker wills and a lower threshold for enduring hardships than men.

This is why the male number is four and the female number is three; four stands for completeness and strength while three stands for the weaker and less complete female condition.[10] Nevertheless, during my fieldwork among the Kuranko I found that men were often loath to discuss witchcraft with me in public lest their conversancy with the subject was taken as evidence that they were 'witches'. In fact witchcraft is seldom if ever mentioned in ordinary conversation. And because witchcraft is primarily associated with women I found it almost impossible to extend my research to cover women's concepts of witchcraft. The data which are set out in the following pages are therefore based upon exegetical material given by male informants. We shall see later that the conceptual system which emerges from this material is not always consistent with the data which come from actual cases of confession, i.e. from women.

The Kuranko concept of the person involves a tripartite scheme. The body is *bɔnke* (lit. 'dust' or 'earth') or *fɛre bɔnke* (*fɛre* refers to non-social space, i.e. the bush or wilderness). The term *duge* is sometimes used as a synonym for *bɔnke*, *duge* here meaning 'ground'. At death the body is returned to the earth from whence it came. The term *nie* ('life force') is complementary to *bɔnke*. Thus, a living person may be described by the phrase '*a nie a ro*' ('he life is in') and death is often announced by the phrase '*a nie ara ta*' ('his life has gone out'). A person's shadow is called *nime* and it is said that a dead person has no shadow; his *nie* is transformed into a ghost or shade (*yiyei*) which can, in some circumstances, haunt the surviving kin. A female shade is frightened off by clapping, a male shade by knocking two sticks together.[11]

Witches are active only at night.[12] It is said that the witch's *nie* leaves the body

and 'goes out', often moving around in the body of an animal. Although the departure of the *nie* is associated with dying the witch does not die. Her body is shaken by convulsions and then breathing ceases. While the body is in this state of suspended animation it is vulnerable; if it is turned around then the *nie* of the witch will not be able to reenter it and she will die. However, if anyone disturbs the sleeping body of a suspected witch or is a witness to such an event they will not make this publically known lest the kinsmen of the alleged witch accuse them of murder. Kuranko point out that in such cases, there is no real evidence of witchcraft. Furthermore, to accuse a person of witchcraft is a serious offence. Of all cases heard in the Native Court in Sengbe chiefdom (1946–1967) only two cases of witchcraft accusation are recorded; the defendants were fined and ordered to 'beg' (apologise to) the plaintiffs. The only case of witchcraft accusation which came to my attention during fieldwork occurred in Sukurela in 1966. Two women, weeding a farm, quarrelled. One said, 'Now look, you are a witch.' The other replied, 'Yes, I used to sit on your house.' The case came before the chief's court and the accuser was ordered to withdraw her accusation and apologise; the women were not related. In another case (case 9, table 1) a woman confessed to witchcraft; she was the wife of a 'big man' and her confession was suppressed. No one made any formal accusation even though the witch had identified herself. Even the men's witch-detecting cult (*Gbangbe*) does not make direct or specific accusations;[13] the cult adepts, like ordinary diviners, simply discover whether or not witchcraft is being practised. When I asked one informant to give me evidence that witchcraft really did exist he said that his knowledge (*lɔne*) came from three sources: 1. 'It has a name and we have heard of it', 2. 'We have seen people die because of witches', 3. 'Witches confess' (*pɔrondo*).

Witches do not attack a victim's *nie* directly; they 'consume' some vital organ (usually the liver, heart or intestines) or drain away the victim's blood or break the victim's backbone by tapping him at the nape of the neck. It is believed that witches work in covens or associations and that the greatest threat of witchcraft attack lies within the localised sub-clan (*kebile*), i.e. 'from among those who share a common inheritance.'[14] As one man put it, 'Witchcraft is eating yourself' (meaning that a witch usually 'eats' her own child, her co-wife's child, her grandchild, or her brother's child) but 'Sorcery is destroying others'. Witchcraft operates as a form of blackmail and indebtedness. A witch will mystically 'open the door' of her house (i.e. she nullifies the protective medicines which the household head has placed on the door lintel). Then a witch from her coven enters the house and 'eats' one of its occupants, usually a child because children are less likely to be protected by 'personal' medicines. The aggressor is obliged to discharge her debt to her co-witch at some later time by making it possible for the co-witch to claim a victim from her house. One informant told me that 'only someone close to you could betray you to the witches by telling them where you sleep in the house and by opening the door to them.' The Kuranko often apply a proverb to illustrate this reasoning: *sundan wa dugu kɔro wɔrla bɔr duguranu de l sɔnti i ye* (if a stranger finds and takes a kola nut that has been hidden under the ground then persons who live in that place must have told him where to find it).

Witchcraft is not inherited; it is thought to be a 'natural' prochvity and this is also why, although the witch is treated as a criminal, the witch's kin are not

TABLE 1. Cases of witchcraft confession

Case	Relationship of victim to witch	Confessed reason for witchcraft
1	Husband	Husband gave unequal shares of meat to his wives; the junior wife, who received the smallest share, sought to 'harm' her husband. His anti-witchcraft medicine attacked her and she died.
2	Husband	The man's wife was pregnant; she tried to persuade him to have sexual intercourse with her but he refused. During his absence from the village she hired the 'armies of the night' to beat him up. He could not be found so the witches beat her up instead. She fell ill and had a miscarriage, then died.
3	Husband	The man was suffering from a severely ulcerated foot; a diviner said that his enemies were responsible. The man cursed whoever was attacking him; a day later his wife fell ill and confessed grievances against her husband. She died. Both the man and village opinion blame her for his affliction.
4(a)	Husband	The woman confessed on her deathbed to having been responsible for the death of her husband and her co-wife's child; no reasons were given.
(b)	Co-wife's child	
5	Co-wife's son's child	The woman quarrelled with her co-wife's son's wife over the sharing of some locust-seed cakes Shortly afterward her co-wife's son's child fell ill and died. The woman also fell ill and confessed to having been responsible for the child's death; she wanted to avenge herself for the quarrel she had had. She died as soon as her confession was over
6	Brother	Eight days after her brother's sudden death the woman fell ill and confessed that her brother had once refused to give her rice; she had, with other witches, beaten him up. Some months before she had tried unsuccessfully to kill him The woman was buried alive.
7	Brother's son	When the boy died suddenly a diviner was consulted who diagnosed witchcraft as the cause of death. The witch was cursed Within ten days the boy's father's sister fell ill and confessed that she had killed the child because her brother had refused to give her rice. The woman was buried alive.
8(a)	Husband	During terminal illness the woman confessed that she had once tried to kill her husband by witchcraft.
(b)	Brother's son's child	She was childless and she owed her witch-association a child; she had been responsible for the death of her brother's grandchild
9	Brother's daughter's son	Apart from the fact that the woman confessed to witchcraft in this case no other details are known; the confession was allegedly repressed by her husband's family
10	Not specified	Two witches were arrested by *Gbangbe*; they confessed during a court trial.
11	Not specified	Four co-witches were named during the confession of a dying woman (herself a witch); the four witches confessed to witchcraft during a court trial

Notes: 1 All child victims were male.

2. The witch's ghost (*pulan*) 'came out' in four cases (1, 3, 6, 9).

tainted by or held responsible for a witch's actions. Witches have 'bad *yugi*'; that is to say that they are victims of innate and ungovernable propensities to do evil. This internal force (*yugi*) cannot be resisted or willed away. But witches are said to use external aids to bolster their powers: witch suits of armour, witch gowns, witch aeroplanes, witch guns, witch animals. These are the things which the witch-detecting cult attempts to destroy. But the cult cannot destroy the witch directly; it can only disarm her. The animals most frequently associated with witches symbolise various attributes of witchcraft: predators (leopard), scavengers (hyena, vulture, palmbird), black animals (black cats), dangerous animals (snakes) night animals (bat, owl). The owl (*gbingbinyaga*) is often called the 'witch bird' because 'it is seldom seen and it flies by night'. The Kuranko refer to were-animals or shape-shifters as *yɛlamafentigınu* (lit. 'change-thing-masters') but the term usually refers to men who use their shape-shifting powers to harm others or destroy livestock. Witchcraft is associated with animality but sorcery involves, in these cases, the actual embodiment of a man in an animal.[15]

III

Witchcraft confessions

Table 1 presents details of eleven cases of witchcraft confession from two villages. The data are based on reports by six adult men each of whom could remember only one case of confession within his *kebile*. Other informants could recall no cases at all. Two of the six informants who supplied details of confessions also mentioned five other cases of confession in other *kebile* in their village. Space precludes the publication of all the details pertaining to these eleven cases and I will therefore give details of only three cases in order to illustrate three modes of confession.

1. (Table 1, Case 4) A woman confessed on her deathbed that she had 'killed' her husband and also 'eaten' her co-wife's child. She gave no reasons for this, nor did she give any details of her witchcraft. My informant was the eldest son of the 'husband' who had been killed by witchcraft. He said that the woman had been cheerful all her life and had never hated anyone. He was of the opinion that she had pretended to be a witch but he could not explain why.

2. (Table 1, Case 11) A woman confessed on her deathbed that she was a witch; before she died she named four associates. These four women were brought before the chief's court and ordered to demonstrate their powers and so prove that they were witches. The women requested some pawpaws and a lizard which they left in the courthouse while they were locked in a room. When they asked to be let out of the room they told the elders to cut open the pawpaws; they contained no seeds. They told the elders to examine the lizard; it was dead. Each woman was fined a cow. Two of the cows were killed and a curse was put on the livers. The women were then ordered to eat the liver together with *dɛge* (raw rice flour used in domestic sacrifices). Within a few weeks three of the women had died. The fourth woman, who is still alive, is not regarded as a proper witch; people say that 'she was not guilty of actually eating anyone; she was a witch but she did not practice witchcraft.'

3. (Table 1, Case 8) A child died suddenly. A few days later the child's father's

sister fell seriously ill and confessed that she was responsible for the child's death; she said that she had 'got her destiny through that child', i.e. that her sickness was a punishment for having killed the child. When she had gone to enter her husband's house (where the child was asleep) the house had been surrounded by fire; as she left she had been burned. She also confessed that when, many years ago, her husband had accompanied a European into the Loma mountains to hunt elephants she had transformed herself into an elephant and tried to kill her husband. She also told how, whenever the *Gbangbe* cult was active, she would hide 'between the earth and the sky' in order to evade detection. She said that she could change herself into a vulture, that she was a member of a coven, and that she had been responsible for her husband's failure to get 'the staff of chieftaincy'. When her confession had been heard she was taken from the house and left in the backyard to die.

The first mode of confession involves very few, if any, allusions to the stereotyped imagery of witchcraft (Cases 1, 3, 4, 5 and possibly 9). In the second mode of confession, which is a part of a public court hearing, the 'witch' demonstrates her powers of 'eating seeds' and killing at a distance (Cases 10 and 11). The third mode of confession involves elaborate and 'imaginative' allusions to the stereotyped imagery of witchcraft (Cases 2, 6, 7 and 8). Nonetheless, when reference to the stereotype is made it is often highly selective or minimal: the idea of witch associations is mentioned in only five cases, the idea of were-animals is mentioned in only one case, and in only two cases does the witch confess that she 'consumed' her victim (child-victims are usually 'eaten', adult-victims are 'beaten up'). We must conclude, therefore, that the ideas which emerge from actual confessions and the general pattern of beliefs which emerges from male exegesis are not coterminous and isomorphic. This discrepancy between the structure of belief and the events associated with the belief has been noted by other writers (Gray 1969: 171; LeVine 1969: 239; Ruel 1970: 338). Ruel's hypothesis that 'introspective' witchcraft and rigid or simple stereotypes tend to be mutually incompatible, is confirmed here; the relation between 'introspective' witchcraft and belief in were-animals is less pervasive in actual confession than at the level of generalised belief (Ruel 1970: 334–5).

The incidence of confession is also less than the incidence of accusation and suspicion. Many informants, speaking generally, claimed that most sudden deaths of children are caused by witchcraft. Yet diviners diagnose witchcraft in very few of these cases. Some diviners say that it is not in their power to diagnose witchcraft. Again, the *Gbangbe* cult was summoned to Sukurela five times during a six-month period but only one confession was reported during this time. Evans-Pritchard's remark that 'In speaking of witches and witchcraft it is necessary to explain that Azande normally think of witchcraft quite impersonally and apart from any particular witch or witches' (1972: 37) is also true for the Kuranko. Moreover, collective rites of exorcism and purification do not require specific accusations or individual witchcraft confessions. When Sukurela was plagued by a disease which has taken the lives of many small children, the town chief summoned the master of the *Gbangbe* cult—*Gbangbane*.[16] Witchcraft was diagnosed as the cause of the epidemic. Raw rice flour (*dege*) was sacrificed and *kebile* elders, representing every household in the village, ate the *dege*. Anyone practising witchcraft after participat-

ing in this sacrifice would die. The epidemic ended, but no confessions to witchcraft were made. On another occasion a man summoned *Gbangbane* when his son was seriously ill. Witchcraft was diagnosed but the man was told that his son would not die. He recovered. No confessions to witchcraft were made.

It is important to note that the *Gbangbe* cult, like divination, involves collective action and collective purification. When *Gbangbane* is 'out' (witch-detection is always a night activity) all women and children must remain indoors. This temporary but strict segregation of the sexes is comparable to initiation ritual, both serving to cleanse or 'whiten' the community by separating male and female domains. It is because witchcraft stems from male–female antagonisms that this rite of segregation is effective; the sacrifice of the white rice flour establishes a barrier beyond which witchcraft (which is associated with blackness) cannot pass. This is also why anti-witchcraft medicines (called 'sacrifices') which are fastened to the lintel of a door are usually white objects such as cotton wool or white cloth. And this idea also explains why a confessed witch is always taken out of the house to die and then buried in the bushland beyond the village perimeter.[17] In other words, witchcraft signifies the invasion of *domestic or village* space by *wild* and uncontrolled powers, generally associated with animals and the bush.

Before turning to more detailed analysis of the pattern of confessions I want to discuss the manner in which the Kuranko internalise beliefs in witchcraft during the process of socialisation.

IV

Witchcraft and socialisation

Several writers have shown how the concept of witchcraft develops as a significant and familiar part of childhood experience (Evans-Pritchard 1972: 31; Kluckholn 1967: 76–9; Brain 1970: 170–1; Nadel 1952: 18–29). Kluckholn and Nadel have stressed the importance of childhood experiences in forming ideas about the loci of witchcraft threat.

Among the Kuranko, children's nightmares are often attributed to the activity of a witch's ghost (*pulan*). The ghost enters a house in the form of a small lizard. It then proceeds to count the sleeping occupants of the house in pairs until it finds an 'unpaired' and therefore vulnerable person. The person wakes to feel an oppressive weight on his body; he may be unable to cry for help.[18] Informants told me that some children, after having been haunted by a *pulan* in this way, have to be forcibly 'straightened out' in the morning because they have been paralysed with terror. Children's fear of the dark is also related to witchcraft fears. Children also learn of the manner in which witches operate. The precautions which their parents take against witchcraft attack confirm in them the importance of protecting inner space (self or household) from the invisible powers which threaten it from without. Sudden deaths, often attributed to witchcraft, and the frequency of disease and infant mortality (approximately 50 per cent. in a sample from Sukurela) condition this early concern for protection (*kandan*).

For a girl the difficulties of resocialisation at marriage (which follows immediately after initiation at puberty) are considerable. Marriage involves total incorporation of the bride into her husband's *kebile*;[19] the young bride must learn

to regard her natal group as the 'out-group' and her husband's group, at least for the sake of her children, as the 'in-group'. This is doubly difficult when she is not only subordinate to her husband (whom she may not respect anyway), but also to her mother-in-law and her husband's senior wife (*baramuse*). Not only do women mediate affinal relationships between groups of men, they also mediate between their children and the children's paternal ancestors. A child's prosperity, luck or fortune (*baraka*) are relative to the quality of its mother's relationship with her husband; if she dishonours her husband then her children will suffer misfortune *fuguriye*, i.e. the opposite of *baraka*) because their paternal ancestors will withhold blessings (*duwe*, i.e. the source of *baraka*) from them. It is because women occupy these mediatory positions in the agnatic system that they are often held accountable for the tensions and disruptions that occur in both conjugal and affinal relationships. Discord among affines, a difficult confinement, a child suffering from a wasting disease, a hunter's lack of success, a dead man's spirit which haunts his living kin . . . in all these cases the onus and pressure are on the woman to make expiation possible through confession. Many Kuranko women say that their only escape from the pressures of male expectations is through domestic recalcitrance or adultery. Their only compensation and solace are found in relationships with mothers, age-mates and brothers. But these relationships must be clandestine lest they appear to betray a woman's primary loyalty which is to her husband and children.

Women are thus enjoined to explain misfortunes which occur within the subclan by searching their souls. Introspective explanation is not, however, a male characteristic.[20] Men divine, order, accuse, suspect, blame; women confess. Because witchcraft is associated in the first place with situations of *otherwise* inexplicable misfortune in the *kebile*[21] and in the second place with women who act beyond the socially-prescribed limits of their mediatory roles, it is understandable that women's introspective explanations should often find expression in terms of the stereotype of the witch. But women who confess to witchcraft are *both* under pressure from socialised attitudes and expectations *and* under pressure from *otherwise* inexplicable personal misfortunes. Except in two cases (Cases 10 and 11) the 'witch' confessed spontaneously during the final stages of terminal illness. Among the Kuranko direct social coercion by others as a variable influencing confession seems to be replaced by inner compulsions resulting from severe illness and accompanying depression. Among other tribes of Sierra Leone confessions seem to take place under duress: Limba (Finnegan 1965: 117–18), Mende (Harris & Sawyer 1968: 76, 80, 122); Temne (Littlejohn 1967: 338).[22]

Although witchcraft confessions occur in only certain circumstances (the sudden and inexplicable death of a person followed soon afterward by the sudden onset of serious illness in a female relative), we can elucidate other patterns from data on confessions which afford us further insight into the sociological fault-lines in Kuranko society.

V

Witches and their victims

That witchcraft attack occurs consistently between certain categories of kin is made clear from the data in table 1. The witch is either a sister or wife who either

attacks *directly* her brother or husband (or husband's senior wife) or attacks *indirectly* her brother's child or her husband's child (or grandchild). Victims are never neighbours or members of the senior generation. The following diagram summarises all cases.

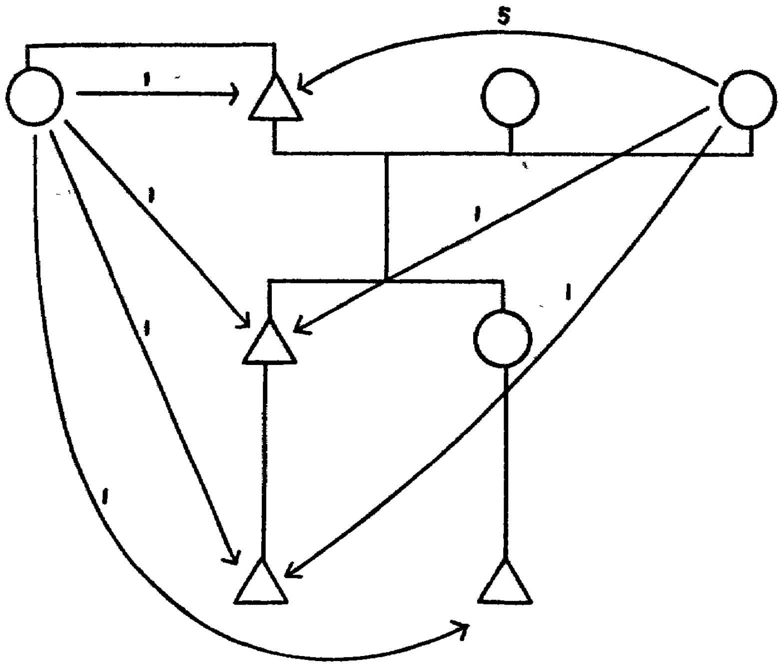


FIGURE. 1. Summary of 11 cases of witchcraft attacks (table 1) showing relationship between victim and witch and relative frequencies of attack.

Clearly, witches are always married women: sisters who have married out or wives who have married in. Although the Kuranko do not associate witchcraft threat with the field of affinal relationships, we are led, by these facts, to consider such an association. It is noteworthy that the Kuranko often regard problems in affinal relationships, notably between brothers-in-law, as springing from a conflict between sibling ties and the conjugal bond. A sister has the right to make claims on her 'bridewealth-linked' brother: she expects to receive domestic property from him, she expects him to mediate and sympathise in any dispute involving herself and her husband, and she expects him to offer support and assistance to her children in times of crisis (this includes assistance in her sons' marriages). The respect and deference which men show to their sisters are compelled by the fear of the sister's curse (*danka*). However, as a wife a woman is strictly subordinate to her husband and has no *direct* power of redress. Men tend to be caught in a 'double bind'; if they prevent their wives spending much time with kin, then they in turn will see little of their sisters. If a man gives much time and assistance to his sister

then he might be accused of working against the interests of his sister's husband; tensions between brothers-in-law are consciously related, by the Kuranko, to these mutually-opposed constraints of the brother/sister tie and the conjugal tie. A wife who retains sentimental attachments to her natal group or associates with outsiders to her husband's group (lovers, brothers, mother, or age-mates) is, in the Kuranko view, acting contrary to the jural expectations and obligations of marriage. A subtle and difficult diplomacy is involved here because a man must balance his obligations to his sister against his obligations to his sister's husband in order not to offend either. I suggest that the witch can be defined as a woman who puts her role as sister before her role as wife and mother; she subverts the conjugal relationship by acting toward her husband as if he were a brother, i.e. someone she has a direct claim upon. This idea is obviously connected with the more general Kuranko concern with the conflict between women-as-objects and women-as-subjects. It is also both consistent with the Kuranko theories about the causes of affinal discord and compatible with the Kuranko notion that a witch is an insider who is contractually allied with a group of outside associates.[23] When the witch is a paternal aunt or sister of the victim we can say that she overreaches her right to make claims on her brother, usually by making unjustified claims on her brother's sons or grandsons. It is worth remarking that, although a sister can curse her brother if he 'fails to give to her', she is not normally entitled to curse her brother's children; this means that the father's sister is feared, less for her power to curse than for her potential to be a witch (i.e. the curse is regarded as illegitimate). In this case too, therefore, the conjugal bond is subverted by overemphasis on the brother/sister link.

Witchcraft constitutes an attack on the conjugal relationship and, by implication, a nullification of affinal ties. More specifically, it indicates a conflict between uterine and agnatic affiliations. This point of view recalls Middleton and Winter's discussion of patrilineal societies with a 'house property complex' (1969: 14–15) in which uterine cells within the joint family unit are brought into competitive relationships. The co-wife who is favoured by her husband or whose children are materially and spiritually favoured by the paternal ancestors is, in a sense, the true insider; the under-privileged co-wife and her less-favoured children are peripheral to the group. Seven of the witches whose confessions are recorded in table 1 were junior co-wives. Like many other African peoples the Kuranko explain rivalries and tensions between non-uterine siblings in terms of rivalries and tensions between their mothers; this 'displacement of affect' is an aspect of the tendency to explain male–male dissensions by reference to female–female or female–male dissensions. In so far as witchcraft confessions always concern conjugal, brother/sister or co-wife tensions we can say that male–male rivalries or conflicts are displaced onto male–female or female–female dyads, thus serving to protect, by a form of masking, the ideal integrity of the male group.

The opposition between centre and periphery is socially manifest as an opposition between men and women, but Kuranko concepts of spatial boundaries also express this opposition. Significantly the animals which are commonly associated with witches are animals which are seen as capable of leaving their proper location or habitat and invading a location or habitat of primary value to man, i.e. the domestic group (Tambiah 1969). Examples are: lizard (associated with the witch's ghost—

pulan), leopard (which enters the village to kill domestic animals), toad, snake, palmbird, vulture, hyena.

VI

Conclusions

Witchcraft among the Kuranko is obviously only one aspect of a system of thought which encompasses male-female relationships, explanations of misfortune, socio-spatial categories and ritual. It is particularly important to relate witchcraft to other processes of social control and explanation, especially divination and sacrifice. Abrupt changes in a person's life, including both personal and social crises, generally lead a person to consult a diviner (*bolomafɛlne*); the master of the witch-detecting cult is simply a specialist diviner. In divination, interrogation of the client is minimal. Sacrifice always follows consultation with a diviner, either to avert disaster or to maintain harmony. Litigation, by contrast, involves a considerable amount of formal interrogation, both of the defendant and the plaintiff. Indemnification always involves the offering of a sacrifice. In the case of witchcraft, confessions are rarely made under duress or as a result of interrogation (the exceptions are cases 10 and 11). If a diviner or master of the witch-detecting cult diagnoses witchcraft then the family of the client (in the first case) or the community (in the second case) will offer a sacrifice, usually of raw rice flour (*dɛge*). If a witch confesses to witchcraft it is a result of self-interrogation and her death, being expiatory, is a kind of self-sacrifice. The relationships between law, divination and witchcraft confession can be shown as in table 2.

TABLE 2.

	Breach of norm	*Counteraction*	*Resolution*
Law	known crime	public interrogation in court	indemnification + sacrifice involving entire community
Divination	cause suspected	minimal interrogation in private	sacrifice involving domestic group
Witchcraft	cause unknown	self-interrogation	self-sacrifice

With regard to studies of witchcraft in Africa, the Kuranko seem to occupy an unusual position. Not only are accusations seldom made, but confessions are rarely made in courts, cult groups,[24] or under duress within the kinship group itself. Yet confessions are thought to be the only real evidence confirming witchcraft beliefs and fears. We find that there is considerable discrepancy between the confessional data and the general belief system. There is a further discrepancy between the number of occasions when witchcraft is suspected or diagnosed and the number of actual confessions. This may account for the fact that the Kuranko seek other explanations for misfortune and invoke witchcraft only as a last resort.[25]

If confessions do not invariably confirm general beliefs about witchcraft why is it that the beliefs persist without substantial revision? In the first place we should emphasise that the beliefs are simply one subset of a general system of signs referring to problematic aspects of Kuranko social structure. Witchcraft is not a 'thing' although its bizarre character inclines European observers to set it apart from

other, more readily comprehended, aspects of African thought. The metaphorical and arbitrary nature of witchcraft makes it largely independent of events. In the second place the pattern of witch-victim relationships, which is disclosed by studying a number of confessions, does conform to general beliefs about where witchcraft attack is most likely to occur. However, the link between witchcraft attacks and the conflict between conjugal and brother-sister relationships would not be recognised so directly by the Kuranko themselves. This may be because individual Kuranko have a very limited knowledge, and in many instances no knowledge, of actual cases of confession.

I have endeavoured to show that the psychological aspects of witchcraft beliefs and confessions are consequences or concomitants rather than causes of social conditions. The anxiety, guilt, depression and compulsions which accompany or instigate self-investigation and confession are 'signifiers' of problematic areas in social structure. These intellectually diffuse and emotionally intolerable experiences can only be interpreted in terms of objectivised and systematic beliefs present in the group's culture (Lévi-Strauss 1963: 171–2). In Kuranko culture witchcraft offers this template, but in other cultures other objective terms may be used to assign meaning to the same experiences. For this reason it is probably futile to attempt comparative analysis on the basis of the beliefs themselves; we can only compare structures, not the native representations of them.

One of the most arresting facts which has emerged from this study is the extent to which Kuranko society polarises the sexes in terms of oppositions between male/power/active/extrospective and female/powerless/passive/introspective. The witch—a victim of uncontrolled proclivities which she is powerless to resist—is, ironically a *product* of the male order which she threatens. Nevertheless, the self-confessed witch is more than just a category, she is a categoriser She alone decides that she is a witch, she alone seals her fate. Confession entails an acting out of repressed envies and guilt-laden hostilities. But it is also a liberation. She emphasises, retrospectively and belatedly, the group values which she has transgressed, but she also achieves a level of self-expression never before possible. This may explain the defiant and dramatic attitudes of some witches, particularly in cases where confession is not associated with severe illness (cases 10 and 11. See also Kluckhohn 1967: 83–4; Brain 1970: 170 on witchcraft confession as an 'attention-seeking device' among 'deprived' individuals). We may also note here that the image of the witch among the Kuranko often refers us back to the notion of unsanctioned secrecy which is in turn a counterpart of betraying secrets. It is because secrecy is so important in maintaining sexually exclusive domains (especially at the level of cult organisation) that the Kuranko regard the betrayal of secrets or unsanctioned secrecy as inimical to their social order which is based on male–female divisions and strict rules governing male–female relationships.

The compulsion to confess is a special instance of the impulse to express things felt but socially forbidden (Reik 1966: 199); confession 'harmonizes the submerged with the public identity' (Ruel 1970: 345).[26] If confession of witchcraft is, in Kuranko society, a form of suicide, it is also an affirmation of a woman's status as subject in a world which seems to have relegated her, socially and physically, to the position of an object. Through confession the 'witch' makes herself out of the conditions which have made her.

NOTES

My fieldwork among the Kuranko was carried out in 1969–1970 (eleven months) and in 1972 (three months); this was generously funded by the Nuffield Foundation.

1 The Korongo of the Central Sudan 'have no witchcraft beliefs at all' (Nadel 1952); other African societies are permeated with witchcraft (Mbugwe, Azande). Although the question of why witchcraft beliefs wax and wane over time or vary in significance from one society to another is outside the scope of this paper, it seems to be related to the presence or absence of alternative modes of explanation (Nadel 1952).

2 Accusation and confession under duress figure prominently in witchcraft among other tribes in Sierra Leone: Limba (Finnegan 1965), Mende (Harris & Sawyer 1968), Temne (Littlejohn 1967), Kono (Parsons 1964: 51–2).

3 In her study of witchcraft confessions among the Akan-speaking peoples of Ghana, Field notes that a variety of mental and physical illnesses are explained by reference to witchcraft beliefs (1960: 148).

4 See Field 1960.

5 cf. Evans-Pritchard's remarks about Azande reactions to accusation: 'Although they assert the moral guilt of others ... when accused of witchcraft themselves they plead innocence, if not of the act—for they cannot well do that in public—at least of intention. To an outsider it appears that there is a contradiction between denial of volition in one's own case and insistence upon volition in the case of others' (1972: 119).

6 Marwick writes that 'Informants may ... make very different statements about the same phenomenon when they are speaking generally and when they are referring to a series of specific instances' (1970: 284). Too many writers on the subject of witchcraft in African societies have failed to recognise the significance of this difference between the actual and the metaphorical.

7 Turner challenges the analytical usefulness of the Zande distinction between witchcraft and sorcery (1970: 118–9) but for Kuranko society the distinction is apposite and the native terminology supports it.

8 Muslim medicine-masters (*mɔri*-men and alphas) are often regarded as marginal and dangerous, not only do their medicines lie outside the inventory of known Kuranko medicines, the *mɔri*-men themselves are less subject to checks and constraints than Kuranko medicine-masters.

9 The Kuranko distinguish between the medicine-master's curse (*gbɔrle*) and the power to curse (*danka*) associated with certain kinship roles (sister, mother, father).

10 Freud's view of women echoes this view: 'We also say of women that their social interests are weaker than those of men, and that their capacity for the sublimation of their instincts is less' (Freud, in Szasz 1973: 197).

11 When the witch-detecting cult (*Gbangbe*) 'comes out' its activity is heralded by an adept who moves around the village knocking two sticks together. At this signal all women and non-initiates must retire indoors.

12 Informants say that 'if the dawn finds a witch out of her body then she will die'.

13 The *Gbaŋgbani* cult among the neighbouring Limba is often regarded, by the Kuranko, as a borrowing, however, there is no evidence that this Limba cult plays any part in witch-hunting (Finnegan 1965: 77–8).

14 cf. LoDagaba (J. Goody 1969: 71). The Kuranko admit that the father's sister could be a witch, but the mother's brother never; 'he belongs to another *kebile*'.

15 In Firawa some years ago a man was accused of killing his half-brother's sheep and goats. The men were notorious rivals. A marauding leopard was caught in a cage within the village but it escaped during the night. The man appeared the next day with scratches on his face and body. He was said to have been able to transform himself into a leopard.

16 There is no *Gbangbe* cult in Sukurela; the cult must be summoned from a village in a neighbouring chiefdom.

17 In two cases the self-accused witch was buried alive. As one man put it, 'A witch deserves no respect; if I had been able to choose she would have been thrown away without any burial. We buried her in the grassland beyond the village stream so that we could burn the grass when it was dry and so make her face into hell'. Although he refused to acknowledge any guilt for his participation in the burial of the woman, he was the first man in the village to be haunted by the witch's ghost. A *pulan brale* ('pulan-catcher') was summoned from a nearby village to catch the *pulan*.

18 *Pulan*-haunting was reported in cases 1, 3, 6 and 9.

19 The transfer of rights is considered to be absolute and in perpetuity. Thus, the girl's father declares: 'Now we have come with your wife. She is your thief, your witch, your daughter,

your everything. We have come to give her to you alive, but even when she is dead she remains your wife. You can flog her, abuse her, she is yours.'

[20] Many Kuranko men maintain that it is because of their 'openness' and honesty that they are so often recruited into the Police Force and the Security Police Force (in the Kono diamond districts). Their tendency to confess readily to any minor offence leads other tribesmen to 'look low' on them. But this introspective characteristic of Kuranko men is only in evidence when they are living away from Kuranko country, i.e. when they are marginal and 'like women'. A Kuranko proverb—*in sa ro a fɔr ma di, kɔnɩ la fere ha yi la* (to say no is difficult but liberation comes from it)—is used to explain that equivocation and deceit always lead to trouble. One informant compared this saying with a Mende aphorism, 'To say yes sir will not tear the collar off your shirt', in order to emphasise the difference between Kuranko honesty and pride, Mende obsequiousness and deceitfulness.

[21] Most cases of misfortune are explained by reference to God and the ancestors. It would be appropriate to say that 'Witchcraft attacks the virtuous, ancestors attack the wicked' (Gluckman 1970: 93).

[22] Among the Kuranko practically all cases of witchcraft confession occur when the 'witch' is suffering from a severe and terminal illness. In other words the ordeal is internal rather than externally administered.

[23] The idea of a witch-coven is an image of a woman's clandestine association with her mother or her age-mates who often scheme to arrange lover's trysts for her. It is also noteworthy that divorce only occurs when a woman has a lover who will marry her and repay bridewealth to her husband Divorce is usually called *sumburi* ('elopement').

[24] The majority of the confessions recorded by Field (1960) in Ghana were made at 'new profit-making shrines'. The cult associated with witchcraft confessions among the Effutu—*Tigare*—is also of recent origin (Wyllie 1973: 75) It is difficult to ascertain whether or not witchcraft confession in these Ghana societies is also of recent origin

[25] This may be why autopsies are never performed on dead 'witches' Among other societies in Sierra Leone autopsies (and the accompanying belief that a 'witch substance' can be localised in internal organs) produce confirmation of suspicions and may also allay people's fears that witchcraft confessions were simply responses to coercion

[26] Death is said to 'bring a darkness between God and man' Confession, like a mortuary sacrifice, is regarded as a means of transforming this blackness into whiteness (i.e a form of purification and clarification), so that animosities, grievances and evil intentions will not continue, after death, to disturb and haunt the living.

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ISLAMISATION AND MUSLIM ETHNICITY IN SOUTH INDIA

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The Muslim Tamils have lived in harmonious syncretism with their Hindu neighbours for centuries, but in recent years many have sought to differentiate themselves culturally. In the process they have undergone a process of Islamisation and have emerged as a distinctive ethnic community in Tamilnadu's northern cities. While commonly ethnicity has been analysed as an adaptation to political and economic competition, Muslim ethnicity in Tamilnadu develops in response to internal needs to acquire status and a sense of social position rather than in response to external relations. It is proposed that the type of ethnicity represented by the Muslim Tamils be called congeneric ethnicity emphasising the form's similarity to racial and national ethnicity while simultaneously suggesting its differences.

I

The purpose of this article is to examine the structure of Muslim ethnicity in South India and to explain why they are undergoing a process of Islamisation in Tamilnadu's northern cities despite their close identification with and integration into local society. I speak of the structure of ethnicity in order to emphasise that the components of Muslim identity form a framework which reflects the social order within which the Muslims live Identity is not simply a name or list of values and customs. A people's identity has a structure which sometimes involves a symbolic order and at times a corporate or social order. Thus, in India the identity of a caste group involves a symbolic structure associated with their caste including style of dress, caste speech dialect and caste specific ritualism as well as the social order of the caste structure.

It is also my purpose in this article to consider the generation of Muslim Tamil ethnicity, for the Muslim Tamils have emerged as converts from the Hindu Tamilian population with whom they share a common cultural heritage. While it is commonly felt that ethnicity is an adaptation to political and economic competition (e.g., Barth 1969; Bruner 1974), Muslim ethnicity in Tamilnadu is not. It develops in response to other factors, particularly internal needs to acquire status and a sense of social position rather than in response to external relations. Like Deshen (1974) I find that ethnicity may find its basis in cultural considerations while political and economic considerations appear inconsequential. The implications for a theoretical understanding of ethnicity is clear. Ethnicity is more complex than generally recognised. In particular we need to know more about the way ethnic distinctions arise. The Tamil-speaking Muslims illustrate ethnic development where ethnic identity provides little political or economic advantage.

It has long been recognised that Muslim ritual and behaviour often owe as much to local custom as they do to Islamic tradition. Accordingly it is not surprising to find Indian Muslims combining Indian and Islamic traditions. This is not to imply

Man (N.S.) 10, 404–419.

that Indian Muslims form a homogeneous population or that Islam is a monolithic religion: neither is true. Muslims in India are culturally, politically and linguistically highly diverse, and the character of Muslim identity and integration varies greatly. Thus, over much of northern India Muslims are characterised as reluctant acceptors of their minority status (see e.g., Spear 1967: 48). However in Tamilnadu State they are considered well integrated acceptors. While Muslim-Hindu antagonism is common in northern India, just the opposite characterises such relations in Tamilnadu. As one might expect, the Tamil Muslims identify closely with their Hindu Tamilian neighbours. It comes as a surprise, therefore, to discover that in Madras City many Muslims in recent years have tried to purify their ritual behaviour and through language and dress to establish more clearly their distinct identity as Muslims. In so doing they have differentiated themselves from the Hindu population by Islamisation.

Since they have lived in a state of harmonious syncretism with their Hindu neighbours for centuries, why should they seek to differentiate themselves? Why should they wish to Islamise? Has a growth in religiosity caused the Islamisation process and hence the differentiation of Muslim and Hindu? Or does Islamisation bring political or economic advantages? Or is it the outgrowth of other forces, perhaps an undercurrent of anti-Muslim sentiment? This is the explanation Aggarwal (1966) gives for the Islamisation process experienced by the Meos in Rajasthan and Hariyana. However, this interpretation seems unlikely in Tamilnadu where the friendly nature of Muslim-Hindu relations is common knowledge. What, then, is the explanation?

II

The Tamil Muslim are the offspring of Arab traders and local converts. They are an autochthonous population which, it has often been remarked, bears the stamp of Tamil culture and the political heritage not of conquest and rule, but of mercantilism and integration (see D'Souza 1955: 43–6). As merchants the Muslims are known for their astuteness and honesty. And while trade took them abroad to southeast Asia where, as in Malacca (see Ryan 1967 and Arasaratnam 1970), they were for centuries an important economic and political force, India has always remained their home to which they have returned As an integrated population the Muslims identify the bulk of their cultural traditions with those of the Tamil population. Thus, they claim their Tamilian origin, speak the Tamil language as their language, in the countryside they dress in much the same way as Hindu Tamilians and even refer to Muslim contributions to Tamil literature, the *sine qua non* of the locals' view of Tamil culture. As a result, the extent to which the Muslims are distinct from their Hindu neighbours is the extent to which they have emerged as Muslims from the local population. An examination of urban Muslims in Pallavaram towns reveals the extent to which this differentiation has taken place in the environs of Madras City, which is in many ways Tamilnadu's cultural and political centre.

III

The emergence of the Tamil Muslims as a distinct population separate from Tamil Hindus is particularly evident in Tamilnadu's northern cities. In this article

I refer to these Muslims as Muslim Tamils in order to emphasise their orthodoxy in contrast to the Tamil Muslims who have not Islamised their customs. In the Madras metropolitan area Muslim Tamils are readily distinguished from Hindus by appearance and attitudes. In contrast to the Tamil Muslims the majority wear clothes which distinguish them from the Hindu population. Men wear *lungis* (a sarong-like garment) and *topis* (distinctively Muslim hats), sometimes a shawl, and women wear brightly coloured saris of a kind Hindus do not wear. Frequently women observe some degree of *purdah*, which is a custom that has not reached south Indian Hindus as it has in the north, and when in public wear a *burka* or a white cloth to preserve their modesty. Muslim Tamils also distinguish themselves by language, because many do not share with Hindus a loyalty to Tamil. Hindu Tamilians are zealous defenders of Tamil and its heritage, which they consider to be one of the world's great literary languages. In contrast many Muslim Tamils, especially those in the state's northern cities within the last three generations, have tried to replace Tamil with Urdu as their household language. They have done so under the misconception perpetrated at one time by the Muslim League, that Urdu is *the* language of Indian Muslims. And they have wanted to be proper Muslims. It follows that the Muslim Tamils' loyalties lie not so much with their Tamil heritage, as popular knowledge would have us believe is true for all Tamil-speaking Muslims, as with what they think of as being Muslim. They see themselves as sharing and partaking in Tamilian culture, but also as separated from it by the Muslim traditions and world view which they wish to emulate (Mines 1972b, 1973b).

It is their vision of what a Muslim ought to be that lies at the base of their own conceptual separation of themselves from Hindus. Muslim Tamil world view and values emphasise equalitarianism, independence and a strong work ethic in contrast to the Hindu view which emphasises social hierarchy, interdependence and status dependent on the ability to command others to work (Mines 1972*b*; 1973b). While it is important to stress here that the contrast involves different perceptions of ideals, nevertheless Muslims are conscious of the distinctions, and their ideology does affect their behaviour even though it does not describe their behaviour. They are equally conscious that Muslims elsewhere do not fulfil their ideals, so they see themselves as standing between Tamil culture and their Muslim values which they strive to fulfil.

Because they identify themselves as Muslims, they want to be good Muslims. Accordingly, in Pallavaram, a suburb of Madras, the leading Muslim Tamils attempt in their behaviour to set an example of austere orthodoxy which is generally accepted as a standard for behaviour. As Sunnites of the Hanafi and Shafi schools, they attempt to follow closely the laws of Islam and their school's interpretation of the *Hadiths*. They believe that minimally a Muslim should observe the *Juma Namaz* (Friday prayer, the most important prayer of the week), although ideally the five daily prayers should be observed. They believe there is no mediator between the individual and God. And they believe that *Id* (festival) days should be observed with prayer, alms-giving and sacrifice, when requisite, and the appropriate feasting or fasting. It must be noted that what is considered orthodox by Pallavaram's Muslims is influenced by local beliefs. Among the community rituals they observe are *Moharam*, *Akhir-e-Chahār Shamba* (last Wednesday of *Safar*), *Mīlād-e-Nabi*

(the Prophet's Birthday), *Laylatu-I-Mi'rāj* (the day the Prophet was invited to heaven), *Shab-e-Barat* (a kind of All Souls Day), *Bakar-Id* (festival commemorating Ibraham's faith), and *Ramazan* (the month of fasting), including *Idu-I-Fitr*. Despite orthodoxy, Sufi-inspired saint worship forms an important popular undercurrent to the orthodox Islamic standards of the Muslim community's leadership, and certain saint's days (*urs*) are observed at various times during the year. The most important of these is the *Abdul Kadar Jilani Urs* commemorating the death of Mohaiyadeen Abdul Kadar Andakai Jilani (alternately: Pīr-i-Dastgīr, Ghaus-ul-A'zam Muhiy-ud-dīn), a Bagdad saint born in *Ano. Hijra* 471. Throughout Tamilnadu he is the most important saint, and his shrines (*dargahs*) are everywhere. Pallavaram has four *dargahs* dedicated to him under various names.

Pallavaram's Muslim Tamils disagree about the orthodoxy of all these ceremonials. For example, some think the Last Wednesday of Safar celebration improper, since it is not in the *Hadiths*, and accordingly argue it should be ignored. Similarly, the orthodox disapprove of certain activities associated with the Prophet's birthday (*Mīlād-e-Nabi*). Many Muslims in the vicinity of Madras City make the birthday an occasion for a pilgrimage to the top of Pallavaram Mount where they view the Prophet's tunic (*jibbah*) enshrined, pray for the Prophet's assistance in their affairs and offer special prayers (*fatihahs*). Pallavaram's orthodox Muslim Tamils consider the pilgrimage reprehensible idol worship and make a point of saying that most of the pilgrims are not Muslim Tamils. The Muslim Tamils observe the birthday by attending speeches by *moulvis* (Koranic college degree holders), by listening to *moulut* (book commemorating the Prophet) readings in the mosque and to Tamil and Urdu speeches about the Prophet's life. A few consider even the *moulut* reading unorthodox. And so it goes; everyone is concerned with being a proper Muslim and follows practice which according to his knowledge is orthodox. Each is perfectly aware that his behaviour is being judged by others and accepts the standards set by religious leaders in the community.

Sufi-inspired *urs* (festival commemorating a saint's death anniversary) celebrations stand in contrast to the austerity of most Muslim Tamil festivals and represent a different, more folksy focus. *Urs* festivals are pervaded by a holiday spirit and are much livelier, more joyous affairs than are the comparatively sombre, standard ceremonials such as *Bakar-id*. Perhaps because of this they have a very wide appeal. The *urs* festivals are in many ways reminiscent of Hindu festivals, since, in addition to the wondrous feats of faqirs, they include processions and the sanctification of the Muslims' residential area, much as Hindu cart festivals do. Nevertheless, in Pallavaram some effort is made to circumscribe *urs* festivals with orthodoxy, and saint worship represents a more popular undercurrent to the tenor of Islamic ritual than the dominant theme which orthodoxy represents. Thus the orthodox stress that saints are great men worthy of respect but that prayers should never be offered to them, except, some argue, with the understanding that the saints will pass them on to Allah. Only Allah should be the recipient of prayers. Nevertheless, saints often become objects of worship and their worshippers guilty of *shirk* (idolatry and establishing associates to God). As a result, most of Pallavaram's religious leaders stay away from *urs* celebrations, and argue that people should participate more for the fun than for the worship. Nevertheless almost everyone does worship and all seem to enjoy *urs*.

Standards of orthodoxy set by religious leaders affect non-ritual behaviour too, and further differentiate the Muslims from Hindu Tamils. For example, at feasts Muslim men sit close together so that they touch and may pass delicacies from plate to plate, a practice which is radically contrary to Hindu behaviour. Hindus generally consider such acts ritually polluting, but Muslims encourage such practices as expressive of their equality. So too, many Muslims have adopted Urdu as their household language, because they associate it with being Muslim. The orthodox also avoid a number of activities which many Hindus enjoy, including attending the theatre, playing cards, drinking alcohol, and using music for ceremonial purposes. Again, avoidance of these activities separates Muslims and Hindus. Quite clearly the Muslim Tamils have a view of the ideal Muslim which is distinct from the Hindu ideal. This ideal creates a tension among Muslims which reflects their wish to be seen as orthodox by their fellows, that they be considered good Muslims even when it is not always clear what is orthodox. Viewed from the perspective of their social origins, Muslims are stressing their distinctiveness, not their similarities, with the majority population. Their social status as Muslims comes not from their relationships with Hindus but from how closely they fit the Muslim ideal: that they dress, speak and behave like Muslims.

Emerging from a common cultural matrix with the Hindu Tamilians, the emphasis on orthodoxy differentiates the Muslims and represents the formation of a separate Muslim Tamil ethnic identity. An ethnic community is a population which:

1 is largely biologically self-perpetuating
2 shares fundamental cultural values
3. makes up a field of communication and interaction
4. has a membership which identifies itself, and is identified by others, as constituting a category distinguishable from other categories of the same order (Barth 1969: 10–11, following Narroll 1964).

An important aspect of the first criterion is the community's sense of being biologically distinct, that their origins if not their race are unique. The Muslim Tamils are an ethnic community[1] which has emerged from a homogeneous cultural setting after the introduction of Islam. The definition obscures the obviously dynamic character of group boundary formation and maintenance. Further it suggests nothing about how ethnicity becomes an important component of social identity.

It is easier to explain how Muslims maintain their ethnic identity than it is to explain their group boundary formation. Disregarding modern conversions to Islam, which are few, the Muslim Tamils are biologically self-perpetuating and consider themselves as different from other Muslims and from Tamil-speaking Hindus. They marry among themselves and only rarely marry Muslims from different groups. This is largely a result of the Tamilians' marriage preference for kinsmen rather than any restriction on establishing marriage ties with other Muslims. But in any case, the pattern helps maintain the Tamilian's separate identity. Similarly, their sense of self-awareness is heightened by the central role of the mosque in their social and religious lives. Usually children learn about Islam in schools associated with mosques and the management of mosque affairs provides a context for community leadership in much the same way that temple manage-

ment does for Hindus. Through Urdu, the mosque and Tamil newspapers (especially *Nuurul Islaam*, *Mani Malarsi*, and *Urimai Kural*) oriented towards Muslims, a field of communication and interaction is created. Further, since Muslims have their own standards for determining prestige and status, they distinguish themselves from others on the basis of orthodoxy as well as distinctive dress and sometimes speech. In other words, newspapers, marriage ties, particularly Muslim standards for behaviour, koranic schools and mosques contribute to the maintenance of Muslim identity. The *Tablique* Movement (a reformist religious movement) and the Muslim League political party have also contributed to this consciousness even though neither organisation now draws much support from the Muslim Tamils. But why Muslims have consciously differentiated themselves from Hindus by language, dress and orthodoxy is less clear, especially given their comfortable integration into Hindu Tamil society, for in many ways Muslims are integrated and think of themselves as an integral part of Tamil society. Is the answer to be found in political or economic considerations? Does orthodoxy lead to political or economic advantages?

In many spheres of life in the cities, Muslims and Hindus (as well as other minority communities) interact closely and share the same opportunities. Most significantly, a relatively large number of Muslims are prosperous, reflecting their success as businessmen throughout Tamilnadu. Their relative success adds to their sense of involvement in Tamil society, which is reflected in many of their activities. Commonly Muslims and Hindus of similar economic interests interact cooperatively when they feel it is in their interest, and in Pallavaram they have jointly organised and now head a merchants' association for their mutual benefit. They do so because it is in their best economic and class interests to do so. Religious identity is not nearly as significant as these practical interests for determining interaction and co-operation among merchants. Mayer's (1970: 19) study of Muslim political attitudes in Trichchirapalli City located near to Madras City's south arrives at a similar conclusion: class considerations are politically more significant than religious affiliation. A simple case in point is the co-operation merchants show in lending money to one another interest free when they are in a pinch (Mines 1973*b*), or in their live-and-let-live attitude about black-market activities, a potential sore point among competing businessmen. Co-operation arising from coincidentally shared economic interests results in a high level of Muslim-Hindu integration in the bazaar.

Similarly, Muslims and Hindus interact freely in politics, joining with others of similar persuasion regardless of religion in their support of the various parties. The lack of political unity among Muslims reflects the absence of a perceived external threat which might otherwise unify them. Instead, Muslim political behaviour to some extent parallels that of the Hindus. Very few of the Muslims are first and foremost supporters of the Muslim League, which we might take as a sign of their pursuit of religious separatism into the political sphere; and those that do are often regarded as fanatical even by Muslims. In contrast, they do support in large numbers Tamilnadu's ruling party, the D.M.K. (*Dravida Munnertra Kazagham*), just as Hindus do, and they have participated in its membership for years. Incidentally, the D.M.K. espouses a strong separatist idealogy for Tamilnadu, and Muslim support consequently is in line with their local identification as Tamilians. Muslim Tamils of different political persuasion support Indira Gandhi's Indian National

Congress. In Pallavaram members of both religious communities hold local leadership roles in these parties. A Muslim is the head of the local D.M.K. organisation and has a large Hindu and Muslim clientele. Similarly, Muslims and Hindus participate jointly in the adminstration of Pallavaram (see Mines 1972a: 20). Muslims feel they have equal opportunities to achieve class status and prestige as well as leadership positions.

The Pallavaram Merchants' Association similarly demonstrates intercommunity cooperation. It has members belonging to all the major religious communities, and of course without the help of its members, the Association could not exist. This association does little more than regulate shop hours and bazaar holidays during normal times, which is an important function in itself. But in times of crises and when problems of mutual concern arise it provides the organisation for taking quick and effective action concerning a wide range of activities. During the Indo-China altercation in 1962, for example, the Association donated Rs. 2,000 to the Indian effort. And in 1969, when Annadurai, the popular Chief Minister of Tamilnadu State, died, it organised a feast for the poor in his name. Muslim and Hindu merchants from both the main parties participated. Merchants co-operated because it was in their personal interest to do so. Annadurai was such a popular man, that merchants of different political persuasions had little choice but to agree to the sponsorship. The Merchants' Association, therefore, cuts across ethnic and religious lines and in conjunction with the foregoing supports the contention that ethnicity provides little economic or political advantage to the Muslim community as a whole. I say little rather than no advantage, because economics and politics are far-reaching phenomena and it would be unrealistic to say that no such advantages occur. It is clear, however, that in Tamilnadu to consider these advantages primary would give the analyst little heuristic insight into the phenomenon of Muslim Tamil ethnicity.

Perhaps as good an indicator as any of the Muslims' relative position in Tamilnadu is the intensity of their expression of felt integration. Granting their minority status and the existence of inequality, Muslim Tamils in Madras City and Pallavaram consistently point to Hindu-Muslim sociability and are adamant about Hindu-Muslim good-neighbourliness. They get along and they are fond of pointing out how well they do, well aware that elsewhere Hindu-Muslim relations are not so convivial. They attend one another's weddings; they have friendships with one another; and they even contribute to one another's shrines.

Muslim Tamil ethnicity provides no overt political or economic advantages. Except for the Mosque administrative organisation which restricts itself almost entirely to mosque and inner-Muslim affairs, there are no exclusively Muslim voluntary associations in Pallavaram. I have already noted that the Muslim League has only a handful of supporters, so there are no corporate groups organised on the basis of ethnicity designed to advance or safeguard Muslim Tamil interests even though other Muslim groups do have such organisations. For example the Da'udi Bohras have the Anjuman-e-Burhani, a voluntary association designed to promote Bohra social and economic welfare. Consequently the Muslim Tamils' concern with their ethnicity does not stem from economic or political considerations, but from some other basis. Economically and politically they are an integrated population.

Clearly, Muslim Tamil integration in the cities involves the dichotomisation of religious and secular activities. The Muslims seek to separate themselves as Muslims from Hindus, yet participate as an integrated population in other spheres of life. Hindu tolerance allows them the freedom of a strong religious identity separate from their relations with Hindus, while the lack of discrimination allows Muslims to pursue opportunities on a par with Hindus in areas of life not dominated by Islam. Yet the question of why they have sought to differentiate themselves ethnically through the process of Islamisation remains unanswered. Clearly Islamisation is not a reaction to Hindu hostilities. Nor does it provide political or economic advantages. To what is it a reaction?

IV

Is the nature of Muslim integration and identity uniform throughout Tamilnadu? Is there this same separation of religious identity and behaviour from secular behaviour, this same push to Islamise? My observations in the village of Edaikottai, near Dindigal City in Madurai District demonstrate that the answer to both questions is no. In Edaikottai village Muslim Tamils do not maintain a separate ethnic identity. First of all there is not the uniformity of dress which distinguishes Muslims from Hindus in the city. Many Muslim men wear the *dhoti* in the same manner as non-Brahmin Hindus. Women, however, do observe purdah. Second, Tamil, the local language, is spoken both within the mosque and in the village, and Urdu is not considered the language of Muslims. Third, and most significantly, there is not as clear a separation between Muslims and Hindus during Muslim ritual activities as there is in the cities. Furthermore, an individual's Muslim identity and public status are not based on orthopraxy as they are in Pallavaram. On the contrary, class and caste-like interactions, not only among Muslims but also among village Hindus, are particularly important for determining status and prestige, while identity is determined by descent. A Muslim is born a Muslim just as a Hindu is born into his caste group. What makes the differences noted here all the more important is that the Muslims of Edaikottai do not live in the village year around. They are city-dwelling merchants who return to their native village annually in order to attend the festival (*urs*, known in Edaikottai as *kurs*) of the Baghdad saint, Abdul Kadar Jilani. During the year most live in Madras City and its environs, including Pallavaram. In other words, they are the same Muslims just described in section III above as striving to Islamise. Accordingly, village behaviour provides a striking contrast to urban behaviour. In Pallavaram and Madras, saint worship occurs more as an undercurrent to the orthodox rituals of Sunni Islam, while in Edaikottai saint worship provides the focal ritual of the year. And, furthermore, the village's saint worship is much more opulent and dramatic than is seen in either of the two northern cities. Clearly, social contexts determine the particulars of self-image and Muslim integration in the two different settings, one urban, the other rural. Since it is the same Muslims behaving differently, it is possible to isolate the factors which account for the differences between the two social contexts.

A short description of Edaikottai's *Kadar Jilani Kurs* serves to illustrate how different it is from the Muslim festivals observed in Pallavaram and Madras, and how different the basis of Muslim identity. The festival itself is influenced by Hindu

custom and in fact is reminiscent of Hindu annual cart festivals when village gods circumnavigate their villages on temple carts, sanctifying the villages and their inhabitants for the coming year. The *Kadar Jilani Kurs* lasts eight days, the last three involving nightly processions circumambulating the village. These processions are the most important part of the festival, and the village's Muslims travel from all over the state to attend. The processions sanctify the participants and the village. Sandalwood paste, flowers and sugar which are carried in procession are distributed at the end of the eighth night and are believed to be a powerful prophylaxis against disease.

The processions are influenced by Hindu tradition. The image taken out on the sixth night is a floral and paper replica of the saint's grave (the image is called *vasaga malai*, garland of recitation, referring to the recitation of the saint's genealogy). The image carried on the seventh and eighth nights is a *kurs*, a towering cone of brightly coloured paper and lights, crowned by peacock feathers which are thought to have strong curative powers. The processions begin at midnight and last until dawn, beginning and ending with a *fatihah* (prayer) At each crossroad the procession stops and the men of the street make floral offerings of jasmine. The processions stop every few yards.

The processions are preceded by neon lights and the roar of a generator. Immediately in front of the image walks a group of faqirs, with tambourines, emitting a cacophony of sounds and calling the praise of the saint; then comes a *nagaswaram* (a double reed musical instrument which Hindus associate with joyous occasions) troup, a three-piece western band, a group of Hindu drummer-dancers and a group of men demonstrating their skills at *lathi* (bamboo pole) fighting.

While the processions proceed at night, during the day the Muslims rest, talk, arrange marriages, feast the poor, renew old acquaintances and compete with one another in their efforts to demonstrate their urban business successes. In other words, the days are crowded with status competition, the formation of new alliances and the continuation of disputes. Among the Muslims who during the year live scattered through the state, the nightly processions represent the symbolic unity of the village Muslims. In Victor Turner's terms the ceremonial creates a feeling of *communitas* (Turner 1969: 96) when the unity of the village Muslims is felt and social differentiation is minimised. The daily activities represent the opposite. diversity and status differentiation. Anti-structure is sought at night and structure dominates the days.

There are several features about the *kurs* which are strikingly different from the behaviour of festival occasions in Pallavaram and Madras. First, there is the use of music as an important aspect of the ritual occasion. Muslims do not use music in this manner in the cities. One has only to recall that many riots between Muslims and Hindus have been sparked by the music of Hindu processions to realise the significance of the Edaikottai Muslims' behaviour. Not only is the procession accompanied by music, but also the Muslims are the same ones that in Pallavaram, Madras and other cities would be offended by such behaviour.

Second, the processions involve non-Muslims as well as Muslims interacting in relationships of interdependence. The *vasaga malai* and the *kurs* are carried on the shoulders of large numbers of Harijans, members of untouchable castes. The drummer-dancers are also Harijans. The relationship between the Harijans and

Muslims is traditional, as is the grain and money payment the Harijans receive. The musicians and faqirs are paid in a similar manner. This interdependence is contrary to Muslim ritual activities in Pallavaram and Madras where Hindus of any caste, let alone Harijans, are not participants. In the cities Muslim festivities are exclusive affairs. In Edaikottai, Muslim religious activities are not as clearly segregated. Muslim and Harijan interdependence in Edaikottai is hierarchical and the Muslims are in the superordinate position.

Third, the use of the *vasaga malai* and *kurs* images runs contrary to urban orthodoxy, where images are not carried even during *urs* ceremonies. The village practice smacks of idolatry and saint worship (*shirk*), which in the urban setting is rejected by Pallavaram's Muslims but which in their village they accept as appropriate behaviour. In Edaikottai the stress is not on Islamic orthodoxy but on pomp and wealth, and Sufi-inspired saint worship is the focus of religious activities. Identity is a product of birth, while prestige and status are achieved not by orthodoxy, but first and foremost by the Muslims' demonstrations of wealth and social superordinance.

V

How are the differences between the behaviour of these same Muslims in Pallavaram City and Edaikottai Village to be explained? How is it that in Pallavaram religious behaviour stresses orthodoxy while the same Muslims in Edaikottai blatantly ignore their austere urban standards of orthodoxy in favour of music, saint worship, pomp and display? And what does the explanation tell us about Muslim identity and Islamisation?

The answers are to be found in what the Muslims stress in their evaluations of themselves and others, and in the structural basis of their identity. Identity, status and prestige are all closely intertwined. In the northern cities of Madras and Pallavaram, a Muslim's identity is based on his group identity as a Muslim and on his personal prestige and status. But in no case is his identity a purely ascribed appellation; it is something which he establishes in his relationships with others and in the way that they perceive and evaluate him. Identity is something which is actively pursued and maintained. It is a plastic thing, so if he changes his behaviour in a significant way he also changes his identity. Thus in terms of his personal prestige and status a Muslim is judged on the basis of factors like class, political affiliations, wealth, power, education, intelligence, likeableness, as well as on the basis of what kind of a Muslim he is. Among urban Muslims this latter factor is particularly important because it determines how they relate with one another. Since all the Muslim Tamils are Sunnites, in Pallavaram they evaluate one another in terms of their orthodoxy and perceived behaviour, the standards for which are set by the leaders of the Muslim community.

The two aspects of Muslim identity, religious identity and personal prestige and status, are not exclusive of each other. A wealthy man, for example, may be considered a low status Muslim. Muslim merchants who sell liquor often display wealth but are given a low status by their fellow Muslims. They find it difficult to arrange marriages for their children with families of similar wealth. Obversely, a person may have considerable status as a Muslim and yet be poor, as the case of

Kabir illustrates. Kabir is a poor man, but he is one of the religious leaders of Pallavaram. His political influence among his followers is considerably greater than that of many wealthier men. His followers look to him for advice on all aspects of their lives, and I found that members of his community often took his word as final. Among Muslims, a wealthy, capable man, who is also considered orthodox, ranks high as an individual and as a Muslim. Few men hold high status in both identity spheres. Typically, the wealthier a man becomes, the harder he strives to establish an identity as an orthodox Muslim.

The appearance in Pallavaram of Muslim-Hindu integration stems from social contexts where Muslim identity is not socially relevant, but where Muslims and Hindus coincidentally share political, social, or economic interests. In these instances, Muslims and Hindus as individuals interact according to their interests. They join together to achieve common goals, to enforce bazaar rules which are beneficial to them, to support the political parties of their choice, to further their business interests or to maintain friendly relations with business associates.

To speak of the dominance of the majority group when discussing these relationships is misleading, because it suggests an actively subordinate position for the Muslim minority and an actively superordinate role for Hindus. Instead, Muslim individuals and Hindu individuals interact as independents, coming together in joint efforts when they see the value of the other's support, both accepting the same standards of success. In this sense, Muslims are compliant with the interests of the majority population and are integrated (see Schermerhorn 1970: 6–7), as is also indicated by their contributions to the Indian war effort against China in 1962 and by their sponsorship of a feast with Hindu merchants in 1969 to commemorate the death of Annadurai. In their pursuit of wealth and political power Muslims are able to achieve success sharing the standards of the majority society. But their religious standards are different from the Hindus'; they cannot achieve status as a Muslim among Muslims except by pursuing their own standards. In order to establish their Muslim identity they have 'Islamised' and segregated themselves from Hindus. Thus urban Muslim Tamil ethnicity stems from an internal need among themselves to achieve status. It currently owes little to Hindu-Muslim relations. Yet it is possible that the rise of ethnic distinctions may open the door to subsequent discrimination in so far as it emphasises the Muslim Tamils' visible distinctiveness. May not Hindus resent this 'clannishness'?

How do the Hindus view the Muslims? It is apparent that Muslims offer no social threat to them. Tamil Hindus do not fear Muslims, and, like the Tamil Muslims, they point to cooperation and to friendship between the communities. Thus a Muslim acquaintance of mine was invited by his Hindu friend to the latter's first viewing of his bride. Hindus recognise Muslim distinctiveness but are unconcerned about Muslim ethnicity. This nonchalance is possible because Muslim ethnicity is not an attempt to regulate external relations with Hindus and Hindus are not affected by it. Consequently boundary maintenance, which Barth (1969) finds so important in contexts where ethnicity does define external relations, is not a source of political or economic tension among Muslim and Hindu Tamils. It is clear, therefore, that analytic approaches which consider ethnicity as a population's adaptation to political and/or economic competition are taking an unnecessarily limited view. Muslim Tamil ethnicity is for the internal consumption of Muslims and arises in

response to the dictates of what is considered orthodox. This orthodoxy is non-Tamilian in nature having a decidedly northern character involving as it does the acquiring of Urdu. This northern influence is also apparent in the hiring of *Hafizes* from the Daru-l-Ulum Deoband to recite the Koran during Ramazan in Pallavaram's mosques.

Status and identity in Edaikottai village appear in striking contrast to the urban situation. The dual aspects of identity found in the urban setting are still present, but, significantly, Muslims do not have to segregate themselves from Hindus in their religious activities. This is because their identity as Muslims is not something they must achieve; on the contrary it is ascribed by birth. In the village, therefore, little concern is paid to establishing an identity as a Muslim. Most are concerned about establishing their personal status and prestige. Within their own community this is done through personal displays of wealth and power. Hence, status-seeking explains several of the ceremony's characteristics. A *Kurs* as elaborate and as opulent as that performed in Edaikottai costs a great deal of money for decorations, feasts and fireworks, as well as for the performers, so it provides opportunities for individuals to display their wealth and successes through contributions to the festival as well as through public feasting. In addition, the *Kurs* is a demonstration of the Muslim community's wealth, religiosity, and relationships to all others in the village. The *Kurs*, therefore, combines ritual and secular features with status seeking. Muslim status is expressed through the medium of their ritual relationships and the grandness of their ceremony.

Edaikottai's *Khadar Au'lia Kurs* begins to make sense. On the emotional level, it is a time of excitement and fun, a chance to get away from daily routines. It is also a ceremonial which acts as a prophylactic against disease and misfortune. At the more abstract level of social relations, its significance is threefold. First, the *Kurs* is a ritual expression of the social unity of the Muslim village community. It is a joint endeavour and the circumambulation of the Muslim residential area sanctifies it and the residents. Second, the festival provides an arena of status competition among the Muslims while demonstrating the symbolic cohesiveness of the group. And third, it is an expression of ritual status which indicates the relative hierarchical position of Muslims compared with other castes which form their audience in the village. At the highest level of abstraction the *Kurs* expresses the multiplex relationships that exist among the Muslims, their village, and a supernatural, their village saint. It is the second level of meanings which is most pertinent to understanding Muslim identity and integration.

In the performance of the *Kurs*, individual identity is of secondary importance to the demonstration of group unity and wealth expressed in a ritual milieu. Being a Muslim means something different from what it does in the city. It is an identity determined not by religiosity but by birth. To be a Muslim is to be part of a group which holds a status *vis-à-vis* the other communities within the village. It is the Muslim community as a whole that sponsors the festival, and status relative to others is determined by their combined demonstration of power and wealth. A corporate Muslim identity of this sort is absent in the city because the community is dispersed and is not a unit of social interaction as households and voluntary associations are. The *Kurs*, therefore, provides a symbolic expression of Muslim status as a corporate group within the village, and in its use of music and in its

stress on saint worship it is comparable to customary veneration of a village deity. It takes this form understandably because in effect the Muslims are playing to a village audience where such behaviour is traditional. Urban orthodoxy in contrast would be meaningful only to themselves and would not establish their position in village society as it does in the city. In other words, the village Muslims are completely integrated into the village society. They share not only the Hindus' standards of success, but also their standards of religiosity founded in group identity.

The Muslims say that at some time in the past they were converted from Hinduism. Conversion began a process of religious differentiation which still goes on. Partap Aggarwal has documented such a process among the Meos of Rajasthan and Hariyana (1966). The Meos were originally nominal converts to Islam. They retained their Hindu names and continued after conversion to interact with the Hindus of their region as a dominant caste. This included maintaining ritual ties with caste Hindus. During the period leading up to and following the partition of India and Pakistan the Meos underwent a process of Islamisation. They changed their names to Muslim ones, they purified their religious practices and ended their ritual ties with caste Hindus. In other words, they moved from a state of complete integration to a state where at least in the religious sphere they were no longer integrated.

A similar process of Islamisation is observed in Tamilnadu. The Edaikottai Muslims have emerged from their Hindu environment, have left the village for the city and there have Islamised. But the process is not unidirectional; it is contextual and reversible. Why? Because urban Islamisation is a response to differences in rural and urban social structure rather than to deep-lying social change such as the Meos have experienced which prevents reversibility. The Meos have lost their pre-Partition social position as a result of radical political and economic change emanating from the division of India and Pakistan. But the Muslim Tamils have not experienced similar change and are able to respond more flexibly to different social environments. There appears to be no pressure from the dominant Hindus forcing Tamilnadu's Muslims to Islamise. On the contrary, urban Muslims have consciously chosen to Islamise their customs. In the city, Muslims lack a corporate identity and religious identity is achieved, as are status and prestige. In order to establish and maintain their religious identity they have found it necessary to create a new group sense through Islamisation. Muslim identity in the village, however, is corporate so that religious identity is ascribed, but social status and prestige are achieved.

VI

The argument which grows out of the particular case of the Muslim Tamils is that social structure is the determinant of the structure of identity. Village social structure is characterised by hierarchy defined by interaction and closed corporate status defined by birth (see Marriott 1968*b*: 105–7). Muslim identity structure is ascribed and is likewise that of closed corporate groups which are ranked. Urban structure in contrast is relatively open. Status is defined by readily-apparent attributes and behaviour (1968*b*: 103–5). Class is more significant than corporate rank (consider Mayer 1970: 19), and group identity is that of vaguely-ranked ethnic categories. The cultural content of identity is not determined by social structure,

but it is influenced by it. In the village an identity based on orthodoxy would make only limited sense to fellow Muslims but contributes little to the Muslims' identity and status in the community. Descent, interaction and displays of wealth establish religious identity and status in the village. However, in the open urban context, group identity is vague. Identity in cities could not be based on corporation, because fellow villagers are dispersed. Identity as a Sunni Muslim is all that remains and orthodoxy helps to establish and maintain this identity.

In sum, when the structure of society is open, then Muslim identity will be based on attributes and the appropriate Islamic behaviour will be stressed. However, when the structure of society is closed and when identity is corporate, the Islamic content of behaviour need not be stressed, while local traditions for determining status will be, and they may contradict Islamic customs as they do in Edaikottai.

The implications of the rise of Muslim Tamil ethnicity are multiple. Barth (1969), Cohen (1974) and Bruner (1974) all see ethnicity heuristically as adaptations to political and economic niches where ethnic boundary maintenance and the regulation of external relations are important. Bruner, for example, shows how urban ethnicity operates in Indonesia to affect job hiring and to determine which ethnic groups will dictate standards of etiquette in different urban settings. In other words Bruner finds ethnicity important in the regulation of intergroup behaviour and economic competition. Similarly he finds ethnic lines clearly defined and boundary maintenance important. This is very different from what we find among the Muslim Tamils. Their ethnicity is not adaptive and has nothing to do with the regulation of intergroup behaviour and economic competition. The Islamisation process the Muslim Tamils are experiencing is confined to the regulation of inner-relationships among Muslims. It develops as a response to an internal need among Muslims to achieve status where the standards of behaviour are not Tamilian but orthodox. The adaptation- and ecology-oriented approach, therefore, fails to provide the perspective necessary to understand Muslim Tamil ethnicity. Shortcomings arise out of the etic nature of the approach. This etic approach carries an analytic bias which, in some cases leads to insight, while in others, such as the case in point, it obscures the causes of ethnic development. In fact this shortcoming raises the whole question of whether ethnicity can be adequately analysed without emphasis on the emic view Without it Muslim Tamil ethnicity is incomprehensible. This is not to deny that political and economic approaches to the study of ethnicity have proved insightful. They have. But an emic approach which views ethnicity as a cultural phenomenon should prove equally fruitful. This is the approach which Deshen (1974: 282) calls for and which proves most fruitful in the analysis of the Muslim Tamils ethnicity.

The main forms of ethnicity that have been recognised by scholars are tribal ethnicity, racial ethnicity and national ethnicity (see Enloe 1973: 23–4). It is clear that the Muslim Tamils do not fit within this tripartite classification. I suggest that a fourth type, 'congeneric ethnicity'[2] which is common to India be added. Congeneric ethnicity is ethnicity which arises when corporate castes are replaced by non-corporate identities which retain some of the symbolic identity of the original group including in particular the general caste name and a sense of separate origin. The Muslim Tamils who moved from their villages have lost their corporate

identity but they have retained their sense of a distinct origin as Muslims and with few exceptions clearly distinguish themselves biologically in terms of origins from local Hindu castes. They retain instead an identity as Muslim which is much broader than their identity in their native villages. The Islamisation process reflects their wider identity base as part of the greater Indian Muslim community. They are seeking identity as Indian Muslims rather than as Tamil Muslims.

Hindus in their turn are undergoing a similar process. Caste fusion has been associated with urbanisation in recent years in India. As a result groups of related castes are joining together on the basis of what they consider common biological origins and forming new caste communities which advocate intermarriage within the new group. These new caste identities are also examples of congeneric ethnicity. They retain a common cultural distinctiveness based on caste plus a sense of separate biological origin which to the outside observer appears fabricated. It is nonetheless real to the community members.

In general terms congeneric ethnicity occurs in India because of the particular nature of its social structure. Caste and religious community form an important aspect of individual identity. A person is always the member of a particular group. In the village this is a corporate *jati* or caste-like group. In the modern Indian city this corporateness is lost, but the caste appellation or general community identity is retained as in the case of the Muslim Tamils These groups now have a much broader base than the corporate caste groups of the village. Consequently the boundaries of the new groups are less restrictive in terms of criteria of membership, since the aim is at fusion of groups of people who previously claimed distinctive identities. In addition to the community name or caste appellation these groups retain a sense of separate biological origins usually expressed in terms of their shared descent from a common ancestor who is viewed as different from the ancestors of any other community. These communities are ethnic groups of the congeneric type, and many modern expressions of caste in urban India are examples Although he does not use the term ethnicity, this is what Hardgrave (1970) describes as the direction of change of the Nadar caste in Tamilnadu. It is also what the Barnetts (1974) describe among the South Indian castes. And it is the pattern which the Muslim Tamils are following. Islamisation is an expression of this new identity among the Muslim Tamils. Congeneric ethnicity is its outcome.

NOTES

Data on the Muslims of Tamilnadu State are based on research conducted between September 1967 and February 1969. I wish to express my appreciation to the Foreign Area Fellowship Program which supported this research. In this article I restrict my comments to discussion of Muslim Tamils all of whom belong to the *Sunni* sect of Islam (see Mines 1972*a*, 1972*b*). The Sunniites form the vast majority of Tamilnadu's Muslim population.

[1] I purposely choose to use the term community instead of group in this context in order to avoid implying any group organisation or corporateness.

[2] My selection of the term congeneric ethnicity is to emphasise the form's similarity to racial and national ethnicity while simultaneously suggesting its differences.

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HUNTING-SCAVENGING PROTOHOMINIDS: A MODEL FOR HOMINID ORIGINS

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The interpretation of the baboon-protogelada evolutionary transition as a response to the exploitation of new food resources, the availability of grasses and their seeds in open savanna country, is reasonable on dental and ecological evidence. But the extension of this model by Jolly (1970) to explain hominid origins is not justified, as detailed functional comparison of the known hominid dentitions indicates. It appears that the morphotype of the Plio-Pleistocene hominid dentitions evolved in response to strong positive selection for increased incisivation and increased molar ability to withstand compressive forces. It is argued that these features, given the phylogenetic heritage of the first hominids from their pongid ancestry, are particularly appropriate to meat tearing and bone crushing, and that the protohominid dentition became adapted to a dietary regime consisting primarily, but not exclusively, of scavenging.

I

Introduction

Few models of hominid origins so far proposed, that have concerned the details of the ancestral hominid morphology, have attempted to explain trends in morphology in terms of the mechanical function of the emerging characters. Historical interpretation in the biological sciences presents difficulties for it is almost certain that the times of evolution of numerous characters of a lineage did not necessarily coincide, with one system often evolving more rapidly than another, due to the differing kinds of selection pressures to which they are exposed. Thus mosaic evolution is more the rule than the exception. It is my contention therefore that synthesis should not attempt to accommodate all morphological data in a group of considerable antiquity which contains several known and probably other unknown taxa. Instead, analysis should be restricted to one functional system at a time across all the known relevant species. Only when the one complex of related characters is understood as well as possible can conclusions, based on both cladistic and functional analyses, satisfactorily delineate its phylogenesis (i.e., genealogical and adaptational history). My inquiry is directed towards an understanding of the ancestral hominid dentition because a) most of the fossil evidence is dental, b) eating habits should be most faithfully reflected in the morphology of the dentition, and c) feeding adaptations are of utmost importance in the evolution of locomotor behaviours and the nature of social interactions.

II

Although the Plio-Pleistocene hominid fossil evidence has been accumulating at a phenomenal rate during the past two decades, the crucial Miocene evidence is still largely lacking. Increasing sophistication in methods of phylogenetic analysis,

Man (N.S.) 10, 420–429.

however, coupled with a better understanding of the morphology of the two distinct kinds of Plio-Pleistocene hominids, can, to some degree, remedy the absence of an adequate Miocene record. One can reconstruct a morphotype (a list of character states for the last common ancestor) and thus visualise some aspects of the inferred ancestor of known hominids.

Jolly (1970) has recently put forward an explanation of hominid origins relying on the model of baboon-protogelada transition. He properly focused on a basic zoological problem, namely the exploitation of new food resources, as opposed to the past anthropological emphases on social behaviour, as the primary channelling force in the exploitation of new resources by a newly emerged species. He clearly emphasised that habitat and food resources were the major causal factors in the evolution of the masticatory, and subsequently the locomotor apparatus, and, to a large degree also, ultimately gave rise to the development of social interaction.

It is my intention here to show that the model of baboon-protogelada transition that Jolly proposed fails to explain the pongid-protohominid transition; the morphological, inferred functional and inferred etho-ecological changes in the protohominids do not parallel the origins of the gelada baboons from a more primitive stage represented today and in the fossil record by the various forms of *Papio* and *Parapapio*. This task is facilitated by the fact that the feeding mechanism is the best known aspect of the fossil record, so that both phylogenetic reconstruction and the mechanical-functional appraisal of morphological entities involved in feeding are feasible.

III

The hominid morphotype

The problem of what we call pongids and a protohominid is an important one and so therefore is the choice of characters which will define the earliest hominids from their pongid forebears.

In drawing up characters of the 'hominid adaptive complex' Jolly (1970) did not attempt to distinguish primitive or advanced features of either the dentition or cranium in Plio-Pleistocene hominids and he clearly did not reconstruct in detail an inferred hominid ancestral condition from all the available hominid evidence. He preferred to refer to a 'hominid complex', or to a list of characters found when viewing all known hominids. He has, however, implicitly built into his 'hominid character complex' those traits largely characterising the robust form, thus, in effect, showing the belief that such characters are more primitive. This list, I believe, is clearly biased towards a picture of protohominids as reflected by the robust hominid feeding mechanism.

It is evident that an appreciation of incisor transformation, beyond the panacea-like use of the concept 'reduction', has not been seriously considered by Jolly (1970), as both the catarrhine *Oreopithecus* and the hominid *Ramapithecus* are treated categorically only as fossils with front teeth smaller than those of fossil or living pongids. Nevertheless, as stressed by Gregory (1916) and recently by Every (1970), the most blatant *morphological* and *mechanical-functional* contrast between the hominid morphotype and Miocene pongids lies in the nature of the anterior dentition. Was the canine-incisor line, compared with the cheek teeth, proportioned in the hominid morphotype as in the gracile or in the robust hominids? That this is

important is shown by the analogy of the variation of the anterior dentition in known hominids and *Gigantopithecus*. The most significant phylogenetic and, at its inception, adaptational contrast of these taxa lies in the fact that in *Gigantopithecus* the incisors are reduced independently from the canines, which are still relatively robust, whereas in both hominid lineages the size variations of the incisors and canines are apparently closely related to one another genetically. But in *Gigantopithecus*, the robust canines functioned as part of the cheek tooth row (as indicated by their wear). Moreover, as far as it is possible to tell from the Chinese Pleistocene mandibles, a canine sexual dimorphism was present. The lack of reduction in the canines in contrast to that in the incisors, the cheek-tooth function of the canines and their retention of pongid sexual dimorphism is the combination of conditions found in *Gigantopithecus* that contrasts this genus to hominids. These unique trends, incidentally, bar this pongid from any significant close cladistic relationship with hominids.

In the hominid anterior dentition, significant canine sexual dimorphism is all but absent in either gracile or robust hominids. This lack is in startling contrast to the cranial dimorphism of robust hominids (Leakey 1971) and a host of other dimorphic features in the extant gracile form. But the latter conditions, on the other hand, are usually accompanied in most catarrhine species by sexual dimorphism in the canines. This unique occurrence of no sexual dimorphism in canines but its retention in cranial and somatic characters and in general size requires some special explanation. We are otherwise left with having to postulate that reduction of the canines and sexual dimorphism in that area is due to lack of selection, while at the same time we must postulate causes *not affecting* the canines which maintained and added dimorphism to the rest of the body in hominids! To postulate positive selection, for the use of the hominid anterior dentition with a specific masticatory role, however, seems more reasonable. Positive selection for increasing the area of incisivation and accentuating the cutting ability of this region would result precisely in the conditions observed in hominids. It would also result in the ontogenetic and genetic interdependence of the canine-incisor row. Thus, when future evolution of the hominid morphotype condition would require an accentuation of the cheek dentition and relative neglect of the anterior one, we would expect the canines, along with the incisors, to diminish in size rather than expand with the premolars. This is exactly how we may explain the reduced canine-incisor dentition of the robust Plio-Pleistocene hominids. The canines of the robust forms become reduced when the teeth distal to them are phyletically expanding only because their ancestral (protohominid) role has been intimately tied to incisivation, and genetic influences affecting the incisors are probably also influencing the canines. It therefore follows that the reduced anterior dentition of the robust forms should not be regarded as representative of the postulated hominid morphotype.

IV

The lack of parallel between the baboon–protogelada and the pongid–protohominid transitions

Jolly, like many others, assesses adaptations associated with 'open-country' habitat. But 'open-country' can have quite different meanings in terms of avail-

ability of food resources. Morphological characters have *no direct* correlation with the openness or denseness of the habitat and vastly divergent morphological characters can have equally strong indirect correlations with this habitat. The correlation is not a causal one. Thus, gelada teeth have converged to some degree with those of colobine monkeys *because* both process greater bulks of food with cellulose content and use their incisors less than the macaque-baboon group. Similarly, the correlation between the heavily buttressed mandible of spotted hyenas and extant elephants is not due to the open habitat but to heavy stressing tied to the very divergent diets of these two species. Wolves and spotted hyenas are both open country animals, but it is the greater proportion of bone in the hyena's diet that is causal, phylogenetically, in a relatively much more robust mandible.

A hypothesis of morphological variation in relation to dietary preferences must primarily rely on those characters upon which operate the selective forces emanating from the relative quantities, and the average consistency of various foods eaten. In establishing dietary preferences of past species the dentition is clearly paramount.

As Jolly (1972: 111–15) points out, the *Theropithecus* group of species is clearly differentiated in cheek tooth construction in the following mechanically significant ways. In addition to an increase in the relative size of the cheek teeth, the crowns develop higher cusps and the individual crowns themselves become mesiodistally relatively longer than those of more primitive papioninines. These transformations are linked to a capability to withstand wear and to increase in cutting ability to deal with masses of grass, the bulk of the gelada diet. In fact, the major advantage of the high cusp-crests or, more properly, the ridges of gelada cheek teeth is that, after initial wear, double rows of enamel cutting edges become exposed which immensely improve the mincing of grass. The pattern displayed by gelada cheek teeth is one that is stringently paralleled in numerous lineages of mammals which subsist on grass. Perhaps the most striking of these convergences is that seen in macropodid kangaroos which, like several other groups of mammals which deal with sheets of fibrous cellulose, evolved a nearly identical cheek tooth system. Canine reduction in height is unlikely to be of any positive significance inasmuch as a specific graminivorous adaptation is concerned; even in the very large *T.* (*Simopithecus*) *oswaldi*, although the canines were reduced (unlike those of the living gelada), the tips of these teeth still conspicuously projected past the occlusal surfaces (Jolly 1972: 114). Canine reduction and habitual independent wear of the tip of the slightly projecting tooth, then, certainly not the kind seen in hominids, has not directly accompanied the cheek tooth transformation of the protogelada or later geladas.

Incisor reduction is a pervasive pattern in *Theropithecus* and it most likely had already occurred in the *Theropithecus* morphotype. Such a reduction, whereby only the relative area and not the qualitative aspects of the morphology change, can perhaps be explained by a 'somatic budget effect', similar to the explanation of loss of eyes in cave fishes or the reduction of wings in flightless birds. A parallel event occurred in *Gigantopithecus* (Simons & Chopra 1969; Simons & Ettel 1970); in this Asian genus cheek teeth were emphasised, phyletically hypertrophied, and the canines became premolarised and part of the straight, extended cheek tooth row.

It is important to emphasise that the incisors alone were reduced; sexual dimorphism, possibly a primitive pongid character, remained manifest in the canines of *Gigantopithecus blacki*, perhaps only because it was advantageous that canines be used as an additional triturating surface.

A distinction is usually drawn between the bilophodont teeth of monkeys and the Y-5 pattern of apes. But both *Papio* and the fossil, somewhat more primitive, *Parapapio* have low-crowned 'bunodont' teeth, in spite of the fact that they possess cross-connexions between the paracone and protocone, and the metacone and hypocone of the upper molars; the protoconid and metaconid, and the hypoconid and entoconid of lower molars. Specimens of the generally labelled 'bunodont' teeth of Miocene *Dryopithecus* apes show that cross lophs exist between the metacone and protocone, the crista obliqua, and between the protoconid and metaconid, and hypoconulid and entoconid; other ridges are also often prominent. Despite these similarities of the cheek teeth of the ancestor groups the descendants (*Theropithecus* and the proto-hominid) followed very divergent paths of evolution. This has been overlooked, or explained away by other quite irrelevant evidence presented in the seed-eating model. Postulated feeding adaptations must be examined against the dentition, both the details of the individual teeth and as a whole, and not to other irrelevant anatomical details such as the convergent presence of buttock cushioning in both geladas and *extant* hominids. Kangaroos hop, have long tails, and crop grass with their front teeth, yet the morphological, functional, and dietary convergence with geladas, mirrored by the cheek teeth, is both obvious and simply explained by similar diet.

Unlike the individual cheek teeth of geladas, which increased both the height and the number of ridges and subsequently the number of functional cutting edges during the buccal and lingual phases of chewing, those of protohominids blunted the ridges and flattened the crown in general. Dental wear maintained cutting edge relief in gelada teeth for primarily buccal and lingual phase cutting, while on protohominid cheek teeth the thick layer of apical enamel assured the maintenance of flat crushing surfaces even after considerable wear. Unlike the astonishing increase in the relative size of molars from the first to the third in the geladas, in protohominids there was a decrease, M_3 becoming clearly much shorter in relation to M_2 or M_1 than in most other hominoids.

In geladas then the relative increase in the amount of cutting edges and the incidental but resulting increase in the cheek tooth surfaces can be related to the bulk of the diet which, low in nutrients and abrasive at the same time, must be consumed in bulk. Jolly (1972: 112) has presented strong and convincing arguments in favour of this explanation. But the mechanical explanation acceptable for the gelada morphology is quite unacceptable for hominid cheek teeth, in particular for their increase in the relative thickness of the enamel and their reduction of M_3 relative to the other molars. Both the thickened enamel and the robustly constructed, thick jaws of early hominids indicate heavy stressing. Whether the thick enamel was a character that enabled the protohominids to withstand great compressive forces on the tooth crowns or facilitated extensive and very frequent food preparation by the cheek teeth is a question difficult to determine from this aspect of the teeth alone. Consumption of grass or other leafy material is not a satisfactory explanation for it, as these teeth lack the characteristic extensive ridging of the cheek

teeth of most grazers and browsers. Reduction of the third molar also argues against large bulks of grasses or even pure seeds, as an increase rather than decrease in M_3 surface area would be at a premium.

The baboon-protogelada model does not satisfactorily account for the transition sequences of the pongid-protohominids. Jolly attempts to ameliorate this by strongly emphasising perhaps the only character that is at all vaguely convergent in the two transitions—the robustly stressed nature of the bones involved in feeding. Aside from the heavy use of the masticatory muscles, comparison of the two transitions does not reveal any other significant similarities. Indeed, divergent morphologies and functions characterise the two morphotype dentitions. The conclusion is inescapable, therefore, that the biological roles performed by the divergent morphologies and mechanical functions were equally dissimilar.

V

Mechanical function and biological role in the protohominids

The seed eating model of Jolly, like others before it, accepted as a basic mechanism for anterior tooth reduction a lack of positive selection rather than transformation for changed function.

With the exception of Every (1970), and to some degree Gregory (1916), most students of hominid dentition, perhaps impressed by the large, robust, piercing canines of other hominoids, considered the *loss* of size in the canines the significant evolutionary event. The loss of the primitive size, morphology, and function of these teeth did not indicate to many the *gain* of an alternative character state and function. The hominid canine is not vestigial but has become transformed, with accentuated mesiodistal cutting edges, into an incisiform tooth, and has become incorporated, in an apparently highly exacting manner, into the continuous apical edges of the canine-incisor row. Every (1970) wrote of the hominid anterior dentition that 'Incision ... was profoundly improved by the significant addition of a unique feature—anteroposterior (retrusive) shearing; it was no longer a simple vertical (orthal) action in the canine-incisor region such as that seen in the non-hominid anthropids today'.

Every's views have not been widely accepted probably because he consistently attempted to relate the novel dental complex of hominids to their use as weapons, concentrating his arguments against the age-worn hypothesis that man's canines were reduced when teeth as weapons were replaced by tools for defence or attack. His lack of dietary concern prevented him from explaining why this hominid morphotype evolved its dentition in a specific manner—aside from any agonistic or social significance.

Jolly, on the other hand, has argued, largely from his own convictions of a parallel between protogelada and protohominid origins rather than from any tangible morphological facts, that meat-eating in carnivorous chimpanzee-like apes would not account for the character complex present in the hominid morphotype. Curiously, throughout his paper he advocated the need for, but did not undertake, a functional analysis; he suggested the necessity '... to set aside the current obsession with hunting and carnivorousness, and to look for an alternative activity

which is associated with "open-country" life but which is functionally consistent with the anatomy of basal hominids' (1970: 8–9). Jolly (1970: 18) stressed the provenance of seeds from 'grasses and annual herbs, which still provide the bulk of the calories of most hominids . . . the diet of basal hominids was centered upon cereal grains as that of the chimpanzee is upon fruit and that of the gorilla on herbage'.

It is a fundamental inconsistency to recognise the divergence of the cheek teeth (the very area which processes the food postulated) between the two phylogenies, and at the same time accept the fact that geladas eat the very plants from which the protohominids were supposed to have obtained the bulk of their diet. Geladas chew up and do not spit out the seeds of grasses they eat, yet their cheek tooth adaptation is unlike that of hominids. To postulate seed eating for basal hominids, however, using grasses as a source of grain, one also must postulate that they undertook painstaking search for the small seeds within clumps of grasses, and avoided the consumption of blades or leaves of grasses and other plants. Jolly recognises (1970: 21) 'The ability to exploit *grass seeds* as a staple is not seen in other mammals of comparable size, though it is seen in birds and rodents, presumably because the agile hand and hand-eye coordination of a higher primate is a necessary preadaptation to picking up such small objects fast enough to support a large animal' (my emphasis). This implies, of course, that the superior eye and hand coordination of a hominoid was not used in a vastly more economical and efficient way to obtain food in an environment with other resources. The hominid locomotor mechanism is drastically transformed in the common ancestor of both gracile and robust lineages to cover large distances, yet a shuffling, haunch-sitting ape, bent on picking tiny seeds from the grasses all around, requires relatively little territory to cover. In fact the locomotor requirements would certainly favour staying close to the ground where the food grows, rather than undergoing the drastic change involved in bipedalism.

In spite of his insistence on the relevance of protogelada origins to that of hominids, Jolly is fully aware of the fact that early hominids could not really have had a niche very similar to geladas. In assessing the divergences between *Theropithecus* and hominid adaptations, he states that they '. . . indicate features adaptive to different aspects of the "open-country" habitat, and analogous adaptations to the same aspects' (1970: 18). In passing he recognises the differences between the two adaptive complexes, but none the less clings to his hypothesis, squeezing the apparent adaptations of probably bipedal, long hind-legged protohominids with a different feeding mechanism into the model of quadrupedal, long-armed, grass-feeding geladas. Even for those characters which blatantly contradict the analogy, he says that, 1) the different molar morphology, obviously not meant for graminivory, is adapted for seed eating (which is likely to be a type of graminivory in grasslands); 2) differences in jaw construction can be related to the rolling action of the tongue to deal with the alleged object of specialisation, seeds, and that this was a preadaptation for articulate speech.

A measure of an incisor's effectiveness is perhaps best appraised by its ability to a) cut well, and b) last long. The former criterion could be related to its apical linear dimension as this determines the usable cutting edge, whereas the latter is usually correlated with the height of the tooth, as in rodent incisors, for example.

Do we have any evidence whether the protohominid has reduced the linear dimension of its incisors? That we do not. However, from a reconstruction of the hominid morphotype, it is evident that the more significant derived features of the protohominid canines are their more incisiform morphology, apical wear, and their ontogenetically rapid edge to edge alignment with the incisors, rather than their reduction in height. The question that remains is whether canine reduction and alignment with the incisors is the positive response to a new mechanical demand posed by a particular diet, or whether canine reduction is the result of cessation of the ancestral biological role and subsequent lack of selection for its former size and morphology. The answer lies in the new morphology of the hominid canine, a fact postulated here to be functionally more important than its reduction in size.

As argued under the discussion of the hominid morphotype, hominid canines were transformed, rather than reduced. We might postulate that alleles controlling the development of incisor morphology extended their field of influence over the canine in response to the need for additional incisivation, and perhaps to the need to reduce projection of canines which interfered with the new functions of either the incisor row or the postcanine dentition.

If one agrees that the canines became truly incisiform, inasmuch as they mesially closed rank with the incisors and reduced crown height to incisor and premolar levels (an indisputable fact for the Plio-Pleistocene hominids), then the area of incisivation became enlarged phyletically. This is in contrast to consistently caniniform canines of all geladas, irrespective of the slightly reduced condition found in the giants of that group.

VI

Habitat and behaviour

Having examined in some detail the morphological and mechanical aspects of the pongid-hominid transition, I shall now turn to the habitat of this transition and suggest a behavioural model that can account for the known characters of the primitive hominid feeding mechanism.

In Africa Miocene habitat transformation occurred on a massive scale; lowland tropical forests were reduced to increasingly open woodland and savanna, and these ultimately to grasslands (Andrews & Van Couvering 1975). As Jolly (1970; 1972) postulated, the evolution of the *Papio* group may be best explained in a woodland-savanna environment, of which there are extensive belts bordering forests in Africa. Explanation of the baboon-protogelada morphological transition centres on the change in feeding habits due to changes from a rich woodland-savanna to a poorer grassland where feeding became specialised as the various grasses became the primary plant food resource.

To postulate grass feeding specialisations for the protohominids would suggest a similar ecological background for the pongid-hominid transition. But the functionally significant morphological changes (argued above) from pongids to protohominids as they relate to feeding adaptations do not parallel the protogelada baboon transformation. Perhaps the protohominids remained in woodland-savanna, an environment usually much closer to forest habitats than the relatively poor

grasslands with which the living gelada is associated. The protohominid dentition may have evolved in such surroundings again in response to dietary behaviour. The classical study by Gregory (1916: 321) of the Heidelberg hominid shows his characteristic insight into hominid adaptations: 'The sharply worn tips of the vertically placed incisors indicate an "edge-to-edge bite" of the upper and lower incisors which would be well fitted for pulling and tearing meat from bones. The stout premolars and molars and enormous jaws would be well able to chew the meat and crack the bones. If it be objected that a carnivorous animal should have large canines it may be replied that the human manner of killing the food and of eating it is wholly different from that of quadrupedal carnivores'. In spite of Gregory's subsequent notions of canine reduction being the result of tool use, adaptation to the niche of a scavenging species, often cited as characteristic of Plio-Pleistocene hominids, can account for the dentition of the protohominids. Strong, vertically implanted incisors plus incisiform canines become the tools which grasp and tear meat, tendon, and fascia. Selection for this role would have been intense as, without good tools, much meat could not be ripped off the carcasses left over by larger predators. Large, pongid-like canines would interfere with this role. Accentuation of other features of the face could produce a signalling apparatus as effective in threat as the canines, particularly when the food-getting needs of both males and females would tend to override canine display.

The cheek teeth of scavenging protohominids would not require the usual posteriorly increasing tooth row of herbivorous species—and therefore the reduction in the relative length of the molars. Crushing function, however, commonly performed by either hyenas or other mammalian scavengers or carnivores would require a powerful mandible, but only with localised need for crushing Thickened enamel would be at a great selective premium for either withstanding the abrasive effects of cracking ribs, metapodials, joints, or for increased longevity, or for both. The relatively puny ancestral pongid teeth were meant for fruit eating rather than for this sort of hard use. In fact, the ridged dental relief of the pongid ancestors would have been at a considerable disadvantage, for the great compressive forces required to crush gristle and bone would induce great bending moments on a delicate cusp pattern with thin enamel, and these could result in consistent breakage and subsequent selection against the relief and cingulae.

In conclusion, it is my belief that, given the pongid dentition, its transformation by the protohominid species can be accounted for by the hunter-scavenger model. Admittedly this cannot be 'proven'. The mechanical results of the morphological transformation, however, do call for an explanation of the positive selection for both a) an area of incisivation which captured the canine, and b) a cheek tooth row which curiously 'shrank' in its relative dimensions when the molars are compared with one another, but hypertrophied and had undergone selection for withstanding a specific kind of wear and great compressive forces.

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THE COGNATIC DESCENT GROUPS OF CONTEMPORARY SAMOA

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The structure of cognatic descent groups constitutes an area of empirical and theoretical interest to social anthropologists. Cognatic descent groups are a prominent feature of present-day Samoan society, and for this reason Samoa is of especial importance to students of the Pacific as well as to students of comparative social structure. The purpose of this article is two-fold: (1) to indicate recurrent issues in the comparative study of cognatic descent groups, and (2) to clarify certain features of contemporary Samoan social organisation. Samoan descent groups and their attached offices are presented within a corporate framework, and the article concludes by noting the utility of corporate analysis for the cross-cultural study of social units similar to those described for Samoa.

Considerable interest has been generated in the nature and variety of cognatic, or nonunilineal, descent groups[1] since the appearance of Goodenough's (1955) and Firth's (1957) seminal articles on the subject almost twenty years ago. Much of the attention since then has focused on several issues involving the comparative structure and 'on the ground' operation of cognatic and unilineal descent groups (see, for example, Davenport 1959; Firth 1963; Goodenough 1961; 1970: 39–67; Murdock 1960; Sahlins 1963; Scheffler 1964; 1966). Researchers working in the Pacific have recognised the need for reassessing the utility of applying to Pacific descent systems models and constructs based on the structure of 'classic' African unilineal descent systems, such as those described for the Nuer and Tallensi. This need is apparent, since it is clear that the structure and organisation of those Pacific descent systems that have been intensively studied, such as the Maori (Firth 1963), the Tikopia (Firth 1936), the Kwaio (Keesing 1967; 1968; 1970; 1971), and the Choiseulese (Scheffler 1965), require approaches and constructs which the African material cannot fully provide. Furthermore, meaningful comparisons of social units incorporated on the principle of cognatic descent cannot proceed until concepts and models are constructed that can accommodate the complexities of the ethnographic facts described for the Pacific and elsewhere.[2]

Discussions of the issues concerning unilineal and cognatic descent generally involve two recurrent questions: (1) are cognatic descent groups corporate, and (2) what kinds of relations obtain between the localised 'core' of a cognatic descent group and its geographically dispersed membership? In this article I examine these two questions in the context of contemporary Samoan social organisation[3] and indicate certain conceptual problems of cognatic descent that require some re-thinking.

I

My description of Samoan cognatic descent groups follows from the corporate analysis and framework developed by M. G. Smith (1966; 1968; 1974) which has

Man (N.S.) 10, 430–447.

been reviewed and modified by Brown (1974) and W. Tiffany (in press) and applied to the political organisation of American Samoa (W. Tiffany 1971).[4] A brief outline of the properties of corporations is followed by discussion of the corporate units of present-day Samoa. I conclude by noting the appropriateness of a corporate framework to an analysis of Samoan cognatic descent groups and their attached offices, and I suggest that this approach can be useful for understanding the structural diversity of cognatic descent groups in general.

In Smith's framework, patterned social regularities are structured by corporations:

> . . recent work suggests that the quality and modes of order in any social system reflect its corporate constitution—that is, the variety of corporate types which constitute it, their distinctive bases and properties, and in the way in which they are related to one another (Smith 1966. 121).

Corporations are characterised by eight properties: identity, presumptive perpetuity, closure, membership, autonomy, exclusive common affairs, procedures, and organisation (Brown 1974: 32–7; Smith 1966: 116–17; 1968: 194–7; 1974: 176–7). A corporation has a unique identity in the sense that corporation members conceive of themselves as constituting a unity. Presumptive perpetuity refers to the presumed persistence of the corporate unit, regardless of changes in its personnel that occur through time. Closure, on the other hand, 'refers to the rules that effect this perpetuity, the rules for excluding and recruiting personnel' (Brown 1974: 32). A corporation has a definite membership, which in turn has some degree of autonomy to conduct affairs that are exclusive to it; corporations also have procedures and organisation for regulating their corporate affairs (Brown 1974: 37; Smith 1968: 194–5). Social units will differ in their combinations of these properties (see Brown 1974: 32). 'Perfect', or fully corporate forms such as offices (i.e., corporations sole) and corporate groups possess all eight properties, whereas, 'imperfect' corporate forms such as corporate categories and commissions do not (Smith 1966: 122–3; 1974: 176–80). As we shall see, Samoan social relations are structured by the presence of several corporate forms having varying properties.

Contemporary Samoan social organisation is characterised by the presence of corporate groups organised on the principle of cognatic, or nonunilineal, descent. The cognatic principle of descent reckoning enables an individual to claim membership in a particular common descent group by tracing descent from that group's reputed founder through either male or female links. In other words, there is no determined order in which males or females are used to trace one's genealogical connexion to a descent group's founding ancestor (cf. Davenport 1959: 558; Firth 1957: 6; 1963: 23, 26–7; Murdock 1960: 2).

The 'āiga. The largest and most genealogically inclusive corporate group in Samoa is the non-exogamous cognatic descent group which is referred to as an *'āiga*. An *'āiga* descent group is identified by the title of its founding ancestor and by the fact that *'āiga* members identify themselves as consanguineal descendants of that founder. Ideally, the membership of an *'āiga* descent group consists of all the consanguineal descendants of the founding ancestor, who is usually a male. In actuality, however, not all consanguineal descendants are recognised as members of the *'āiga* descent group in question. The implications of this statement require some comment.

One important factor influencing the genealogical recognition of a particular consanguineal descendant of an ʻāiga's founding ancestor is that member's active participation in his or her descent group's affairs. It is true that some consanguineal members are disinclined to maintain all of their ʻāiga memberships, and over time such inactivity may lead to the eventual omission of that consanguineal link to the ʻāiga's founder. In effect, then, the genealogically ascribed criterion of consanguinity delimits the boundaries of an ʻāiga's membership. However, ʻāiga boundaries are also influenced by social processes involving the extent of individual participation in ʻāiga affairs (see Firth 1963: 28; Scheffler 1965: 99–100). ʻAiga participation assumes various forms, and participation as an ʻāiga member may include all or a combination of the following: (1) economic support of ʻāiga exchanges and ceremonial redistributions, (2) residence on the estate of the ʻāiga, (3) cultivation of land vested in the membership of the ʻāiga, and (4) political support.

Retention in an ʻāiga's genealogy thus involves participation in that ʻāiga's affairs in addition to the principle of consanguinity. This means that persons who have remained inactive for several generations may be permanently 'lost' from the genealogical record of the ʻāiga descent group in question. But what about 'temporarily' inactive members whose cognatic links are still remembered and retained? Informants are adamant that a mutually recognised consanguineal link is sufficient for an inactive member to gain re-entry, so to speak, into the affairs of an ʻāiga.[5] For example, a consanguineal member of an ʻāiga descent group cannot be refused permission to reside on and to cultivate a designated parcel of that ʻāiga's estate. On the other hand, this permission must be obtained prior to that person's residence and cultivation. Ideally, permission cannot be refused, but a member's rights may not be exercised unless the constituents of his ʻāiga agree to accept his rights.[6] A reputed troublemaker, for instance, could have difficulty in acquiring permission for obtaining residence and cultivation sites on an ʻāiga's estate.[7]

The principle of optation adds still another dimension to the issue of delineating ʻāiga descent group memberships. The nature and extent of an individual's participation in his ʻāiga's affairs is influenced by his simultaneous memberships in other ʻāiga as well (see Keesing 1968). In other words, individuals differentially participate in the affairs of their ʻāiga descent groups. Furthermore, the number of one's potential ʻāiga memberships is, for the most part, greater than the number of actual descent group memberships that are maintained at any one point in time. The limiting factors of time and geographic distance in addition to the economic, political and psychological resources entailed in maintaining active memberships preclude participation to all descent groups to which an individual can trace a consanguineal link. This means that the number of multiple co-memberships that is actually maintained will be far fewer than the number of potential descent group memberships. In addition, the range of active ʻāiga memberships varies among individuals. The number of active ʻāiga memberships tends to be highest among holders of chiefly titles and other upwardly mobile persons who recognise the political and economic importance of maintaining far-flung ties of mutual rights and obligations among various ʻāiga memberships as bases of support for their current and future endeavours.

The residential core and dispersed memberships. One important structural implication of maintaining active ties with different ʻāiga concerns residence patterns.

Those consanguineally related members who currently reside on that 'āiga's estate constitute the localised core,[8] or nucleus, of 'āiga membership. This core also includes other co-residents who are most frequently 'āiga members' affines and cognates of these affines. The residential core of an 'āiga descent group is comprised of households which I have defined elsewhere (S. Tiffany 1974*b*) as residential and commensal units whose personnel conduct domestic activities together. The number of households constituting the residential core of an 'āiga descent group varies considerably. In one Western Samoan village that I studied, the number of households representing an āiga's residential core ranged from one to seventeen; in another (sub)-village of Western Samoa, the residential cores of the two 'āiga studied were represented by seven and fifteen households respectively.

The residential core of an 'āiga descent group is traditionally associated with one particular named estate where the descent group is believed to have been originally established by its founder.[9] The localised residential core of an 'āiga occupies the estate associated with that descent group, whereas its geographically dispersed consanguineal members reside on estates attached to different 'āiga in other villages located throughout the archipelago. At present, many Samoans also reside abroad; I regard consanguineal members who do not live in the islands as constituting part of the geographically dispersed membership of their 'āiga descent groups.

To summarise the discussion so far: The presence of the first four properties of identity, presumptive perpetuity, closure, and membership enable the researcher to determine if the social unit under consideration is a corporation (Brown 1974: 39; Smith 1966: 119). An 'āiga, as a corporate descent group organised on the principles of cognatic descent, possess these four properties. An 'āiga has a distinct identity; that is, an 'āiga descent group is recognised by the title of its founding ancestor. Furthermore, members of that 'āiga view themselves as a distinct unit vis-à-vis other 'āiga. An 'āiga descent group can be easily identified by locating its residential core of consanguineally related members who live on the named estate of that 'āiga. It is presumed by its members as well as non-members that a particular descent group has existed since time immemorial (even though this may be an historical fiction) and will continue to endure indefinitely. An 'āiga has closure in the sense that the criterion of consanguineal descent from a common founding ancestor is a prerequisite for membership. The criterion of descent is modified by the fact that some consanguineal members are eventually sloughed off and forgotten if they do not actively exercise the rights and obligations of 'āiga membership.

The four properties of identity, perpetuity, closure, and membership indicate what an 'āiga *is*. I now turn to the question of what an 'āiga descent group *does*; that is, what are the affairs of a descent group, and how does it regulate its affairs? To answer this, I examine in turn the remaining four properties of corporations enumerated in Smith's framework: organisation, procedures, autonomy, and exclusive common affairs.

II

The presence of specified common affairs that are exclusive to an 'āiga descent group presupposes an internal organisation for regulating those affairs (Brown 1974: 32; Smith 1968: 194; 1974: 175–6). As Beattie (1964: 247) notes, 'Organization is structure in action, its actual working out in practice, in space and time.'

Organisation consists of the recurrent relations that obtain among discrete, enduring parts or units; these relations are expressed by reference to a corpus of specified rights and obligations (see Brown 1974: 39). The internal organisation of an *'āiga* descent group consists of three regulatory components: (1) incumbents of chiefly titles, (2) the *'āiga potopoto*, which is an *ad hoc* commission in Smith's framework, and (3) constituent *faletama*, or 'segments'. The following pages describe the nature of the rights and obligations that obtain among these component units, the kinds of affairs regulated by them, and the procedures employed for conducting the business of an *'āiga* descent group.

Titles. Every *'āiga* descent group in Samoa has certain titles exclusively associated with it. There are many different types of titles in Samoa;[10] I am restricting discussion here to non-paramount *'āiga* titles which are generically referred to as *matai* titles. *'Aiga* descent groups own several *matai* titles; the highest ranking or senior title of an *'āiga* refers to the title of its reputed founder. All other *matai* titles belonging to an *'āiga* occupy various positions within that *'āiga*'s internal hierarchy of ranked titles. All *matai* titles associated with a particular *'āiga* may not be filled at any one point in time; indeed, *matai* titles can remain vacant for years, and even generations.

Traditionally, *matai* titles were not transmitted by hereditary succession in Samoa, and present-day title successions continue to be determined on the basis of three major considerations: (1) consanguinity, (2) service, and (3) personal qualities. These three principles are general guidelines which are subject to differential interpretation, depending upon the personnel involved and the particular circumstances surrounding a title succession.[11] Incumbents of senior ranking *'āiga* titles are elected to hold office by consensus of that *'āiga*'s membership which is represented by its *'āiga potopoto*. An *'āiga potopoto* is a 'formally authorised' *ad hoc* commission (Brown 1974: 36; Smith 1974: 178–9) that consists of an assemblage of *'āiga* members for the specific purpose of discussing matters pertaining to *matai* title successions or removals.[12] According to Brown (1974: 36), 'Such commissions are established on a discontinuous basis, usually to discharge some irregular, special, or otherwise discontinuous duty.' An *'āiga potopoto* commission, unlike a 'perfect' corporation, lacks presumptive perpetuity; however, an *'āiga potopoto* possesses a regulatory apparatus for administering the affairs entrusted to it by its descent group constituents. Whenever a title succession comes up for consideration, an *'āiga potopoto* is assembled for the purpose of discussing the relative qualifications of the various candidates seeking the vacant office. A meeting of an *'āiga potopoto* is scheduled by incumbents of that *'āiga*'s junior titles and is announced in the local newspapers and over the radio several days or a few weeks in advance. The ability to trace a consanguineal link to the descent group concerned is the only necessary condition for exercising one's right to attend an *'āiga potopoto*. That is, an individual may have lived in New Zealand or Hawaii for the past ten years and contributed little or nothing to the economic and/or political support of his *'āiga* during that time; yet his right to attend his descent group's *'āiga potopoto* may not be denied.[13]

Attendance at a scheduled *'āiga potopoto* is not compulsory. Consanguineal members who are actively concerned with the matter of a title succession must indicate this interest either (1) by attendance at the *'āiga potopoto*, or (2) by having another

member (usually a junior ranking title holder) represent his opinion and interests at the meeting. If a member does not indicate his active interest, then it is assumed by the other constituents present or represented at the *'āiga potopoto* that this person has forfeited his right to dissent from the *'āiga potopoto*'s eventual selection of a candidate or candidates.[14]

The decision of an *'āiga potopoto* is not binding unless all interested parties consent. This means that several formal sessions (in addition to informal meetings) may be held over a period of several months, or even years, if those constituents supporting one of the contending candidates for the vacant title persist in support of their nominee. The manoeuvrings of gathering constituent support and convincing other candidates to drop their claims are subjects for another paper. Suffice it to say that unless consensus is achieved by the *'āiga potopoto*, the title either remains vacant or the dispute is taken to court for adjudication. Traditionally, a disputed title either remained vacant until a consensus was reached, or the title succession was resolved through warfare.

Titles are of considerable interest to Samoans, and it is clear why this should be so. The affairs regulated by an *'āiga*'s senior ranking title holder have important implications for *'āiga* members who share varying interests in their descent group's corporate estate. The incumbent of a senior title is regarded as the administrator of his descent group's estate who has been entrusted by his constituents to promote and protect the welfare and dignity of *'āiga* members. Many traditional *'āiga* affairs continue to be regulated by chiefs, and include the following: (1) the allocation of *'āiga* lands for cultivation purposes, (2) the designation of house sites on the *'āiga*'s estate, (3) the arbitration and mediation of intra- and inter-*'āiga* disputes, (4) assessing goods and labour for *'āiga* participation in ceremonial redistributions and village-sponsored projects, (5) representing *'āiga* constituents on the village council of chiefs (*fono*), (6) maintaining other corporate property such as the *'āiga*'s official house site (and possibly a savings bank account), (7) maintaining the *'āiga*'s genealogy, and (8) protecting the integrity of other *'āiga* titles such as junior ranking *matai* titles, and perhaps a *tāupou*, or 'ceremonial maiden', title.

Faletama. One other component unit of *'āiga* descent group organisation must be mentioned. This unit is referred to as a *faletama*, whose membership is less inclusive genealogically than that of its *'āiga* of which it is a part. In other words, a *faletama* may be viewed as a 'segment' or subdivision of its *'āiga*.[15] Ember (1959: 575) defined a *faletama* as including 'all those people who acknowledge common descent from a brother, son, sister, or daughter of the founder of the sept [i.e., *'āiga* descent group].' I propose to modify this definition in order to take note of three points.

1. A *faletama* segment may refer to the genealogically recognised consanguineal descendants of an *'āiga*'s founder's siblings or children. That is, a founder's siblings and children may each serve as an apical point of reference of a *faletama* segment. This does not necessarily hold true for every *'āiga* descent group, however. *'Aiga* constituencies have been influenced over time by demographic and political variables. Thus, some siblings or children of an *'āiga*'s founder may not be genealogically recognised because they or their descendants had few or no issue. Or perhaps some children of an *'āiga*'s founder moved into other regions, established their own descent groups, and were eventually dropped from the genealogy of their 'parent'

'āiga. In short, *'āiga* descent groups may differ in the number of primary or 'higher-order' *faletama* segments that are genealogically recognised as apical points of reference from their founders' respective siblings and children.

2. A descendant of a sibling or child of a descent group's founder may also serve as the apical point of reference of a *faletama* segment, particularly if that apical individual held a high ranking title. These *faletama* units may be analytically distinguished as secondary or 'lower-order' segments, as contrasted to the primary *faletama* segments whose apical points of reference are the children and siblings of the descent group's founder.

3. Females tend to serve as apical points of reference of *faletama* segments if they are sisters of males who held high ranking titles.

Traditionally, the membership of an *'āiga* was formally distinguished between the *tama tāne*, or those who were related to the descent group through a male link, and the *tama fafine*, or those who were related to the descent group through a female link. The *tama fafine* were usually geographically dispersed, whereas, the *tama tāne* tended to comprise the residential core of the *'āiga*. Senior title successions were passed through the *tama tāne*, although the *tama fafine* exercised consultative and veto powers over proposed candidates and were influential in other *'āiga* affairs as well (see Davidson 1967: 23–4; Gilson 1970: 34–9; Mead 1969: 18; Schultz 1911: 51–3). Senior titles, however, did occasionally pass to members connected through a female link if *tama fafine* descendants were able to press their claim to the title through political manoeuvring or coercion (see also Gilson 1970: 62, fn. 45). Contemporary *'āiga* memberships differ in the extent to which they honour the traditional distinction between the *tama tāne* and *tama fafine*. Indeed, it is not unusual today to find *tama fafine* descendants pressing their claim to a vacant senior title by supporting their own candidate in a title dispute (see also S. Tiffany 1974*a*: 42–3).

Figure 1 illustrates a partial genealogy from the point of view of a male ego who has held a senior ranking *matai* office for the last twenty years. This figure indicates the *faletama* segments that ego recognises in his *'āiga* descent group.[16] All males in this figure who are indicated as an apical reference point of a *faletama* segment also held their descent group's senior ranking title, and all *faletama* segments headed by females in ego's genealogy were sisters of males who held their *'āiga*'s senior ranking title.

Faletama segments are commonly identified in two ways. Certain titles may be exclusively associated with the consanguineal descendants of that *faletama*'s apical ancestor. The number of *matai* titles associated with a *faletama* vary; in addition, these *faletama* titles are ranked relative to each other and may be discontinuously occupied. Constituents of a *faletama* may also have exclusive rights to their own *tāupou* title as well. *Faletama* members jealously guard their titles, and any attempt to usurp or infringe upon the rights and dignities associated with certain titles by another *faletama* segment of the same *'āiga* may precipitate a serious dispute and possibly result in a court case if the matter cannot be resolved among the parties concerned.[17] A *faletama* segment may also be identified by locating the portion of designated *'āiga* land that the segment's descendants occupy. In other words, co-resident consanguineal descendants of a *faletama*'s apical ancestor tend to occupy a certain portion of their *'āiga*'s estate. These co-residential 'clusterings' of *faletama*

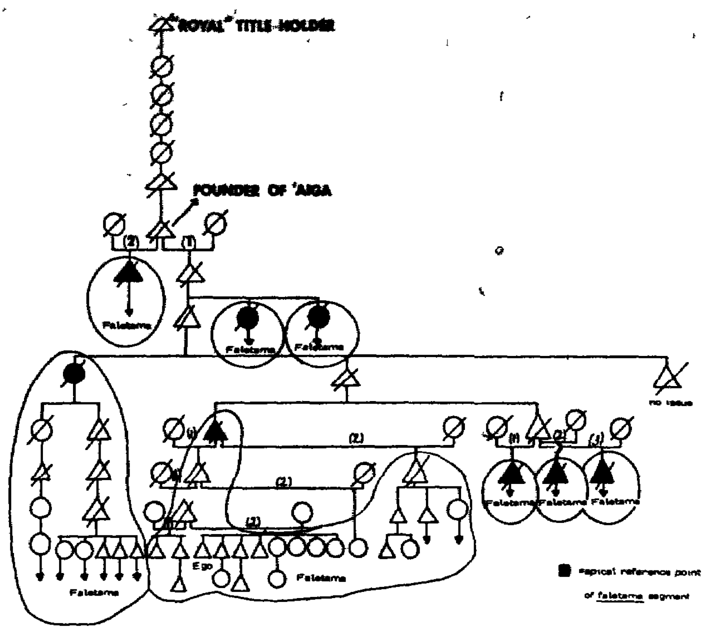


FIGURE 1. Partial genealogy indicating *faletama* segments of an *'āiga* descent group.

segments often occur if a *faletama*'s apical ancestor held his *'āiga*'s senior ranking title.

Faletama memberships are not mutually exclusive. An individual may belong to a plurality of *faletama* segments associated with a particular *'āiga* descent group, as higher-order *faletama* tend to be internally sub-divided into lower-order *faletama* segments. *Faletama* memberships are not clearly differentiated in daily life, except that members of a *faletama* segment tend to be spatially localised on their *'āiga*'s estate, as mentioned earlier. *Faletama* memberships are most clearly delineated during the course of some important internal descent group issue, such as a disputed title succession. Political exigencies arise which require individual commitments and alignments; thus, descent group members come to be associated with one particular higher-order *faletama* (and its constituent lower-order segments) as opposed to the other higher-order *faletama* segments recognised within the same *'āiga* descent group.

Traditional *faletama* segments were labile genealogical and political constructs. All *'āiga* descent groups were divided into a plurality of higher- and lower-order *faletama* segments, and the number and size of these segments varied among different *'āiga*. Traditionally, the political fortunes of *faletama* segments waxed and waned, depending in large part on a *faletama*'s ability to withstand pressures from other *faletama* efforts to reduce its autonomy by usurping its important titles or other associated dignities and privileges. Some *faletama* segments were able to consolidate their political resources, fissioned from their 'parent' *'āiga* descent

group, and in the process elevated their political status to that of 'āiga'. Genealogies would then be accordingly 'changed' to legitimise this new political condition. Samoan genealogies, then, are political statements, not demographic statements, and there is no way of documenting the actual extent to which *faletama* segments 'hived off' from *'āiga* descent groups in the past and successfully established themselves as *'āiga*.

The generation of new *'āiga* descent groups by the fissioning of *faletama* segments has considerably decreased in Samoa since contact. Warfare is no longer an acceptable means of affecting a political and genealogical break from an *'āiga* descent group by members of a dissident *faletama*. Moreover, the traditional method of manipulating genealogies as political statements has been checked in large part by the presence of court institutions and their files of recorded genealogies (see Crocombe 1971). In other words, genealogical statements have been made permanent through writing and their preservation in court records, where today they may be inspected by disputants and used as 'precedents' in land and title cases.

Contemporary *faletama* are relatively stable segments which are regarded as politically subordinate sub-units of their larger and more inclusive *'āiga* descent groups. For one thing, *matai* titles associated with a *faletama* are considered subordinate in rank to their *'āiga*'s senior ranking *matai* title. Furthermore, many of the responsibilities of *faletama* chiefs are delegated to them by their *'āiga*'s senior ranking *matai*. These responsibilities most often include administering the daily political and economic affairs of households located on the *'āiga*'s estate and channelling goods and labour for the purpose of contributing to ceremonial redistributions.

Thus, on the one hand, a *faletama* possesses limited autonomy relative to its larger and more inclusive *'āiga* descent group. On the other hand, each *faletama* segment may differ in the extent of its autonomy relative to other *faletama*. That is, the *faletama* segments of an *'āiga* descent group do not necessarily occupy co-equal status. This is due in part to rank differentials in their respective *faletama* titles. *Faletama* segments also differ in their relative bases of political and economic support. Competitive relations exist either latently or openly among different segments (Smith 1956: 40–1); indeed, the relations that obtain among different *faletama* segments may range from relative harmony to bitter acrimony. The competitive aspects of inter-*faletama* relations are often manifest in disputes over electing a successor to hold their *'āiga*'s senior title (S. Tiffany 1974*a*; W. Tiffany 1975).

I conclude this section by briefly describing the nature of relations that currently exist among one *'āiga*'s *faletama* segments and its chiefly offices. Figure 2 illustrates the genealogical composition (excluding unmarried dependent children) of two *faletama* segments that comprise the localised core of their *'āiga* descent group. These *faletama* co-occupy their descent group's estate located in a Western Samoan village. Persons A and B (fig. 2) co-hold the senior title 'X' of their *'āiga*; each co-holder also represents his respective *faletama* constituents (numbers one and two respectively). All the households comprising the localised core of this *'āiga* are attached to one of the two *faletama*. Chief A administers the daily affairs of his localised constituents who occupy five households, whereas Chief B administers the affairs of his constituents who occupy ten households.[18]

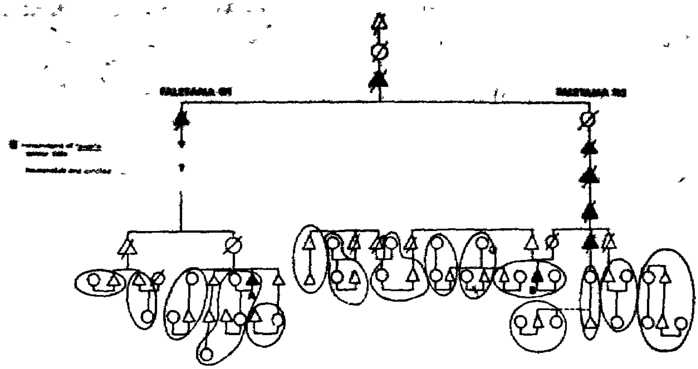


FIGURE 2. Partial genealogy showing localised core of an *'āiga* descent group.

Relations between the two *faletama* segments were very hostile prior to the splitting of their *'āiga*'s title over twenty years ago by court order. The former incumbent of title 'X' was a member of *faletama* two, and *faletama* one constituents refused to back down from their demand that a successor be chosen from their *faletama*. The title dispute was eventually taken to the Land and Titles Court where the title was divided between two incumbents representing each *faletama* in the hopes of restoring *'āiga* harmony.[19] In fact, however, relations between the two *faletama* segments have deteriorated since the court's decision. The two co-holders maintain considerable social distance between each other through minimal involvement in the affairs of the other's *faletama*. Thus, activities requiring co-ordinated efforts of both *faletama*, such as accumulating goods or assessing labour for contributions to ceremonial redistributions, are avoided whenever possible. Instead, each co-holder contributes primarily as the representative of his *faletama* constituents and secondarily as the co-representative of his *'āiga* descent group. The polarisation of relations between the two segments is such that each chief built his own official guest house rather than share the same building.[20] *Faletama* antagonisms also contributed to the withdrawal of the adult women of Chief B's *faletama* from the village's women's health committee.[21] In short, each *faletama* operates as a relatively autonomous unit and only serious matters concerning the *'āiga*'s estate or protection of *'āiga* dignity are sufficient to bring the co-holders face to face for consultations and discussions. Such *'āiga* matters would most frequently include resolving inter-*faletama* personal conflicts and disputes over descent group land or mediating inter-*'āiga* conflicts.

In summary, an *'āiga* is an autonomous corporate group whose elected title holders administer a wide variety of descent group affairs. By virtue of his office, a chiefly title holder has the power and authority to implement decisions and protect the interests of his *'āiga* constituents. A titled office is delimited by its identifying title, specified procedures for acquiring the office, its presumed perpetuity, and its regulatory capacity for administering the affairs of its constituent *faletama* segments and those of its *'āiga* descent group. A titled office can therefore be regarded as a corporation sole which possesses all eight properties of formal corporations

(Smith 1966: 117–19; 1968: 196; 1974: 178–9). A *faletama*, as an administrative subunit of its *'āiga* descent group, constitutes a third distinct corporate unit in Samoan society. Many of the affairs regulated by *faletama* and *'āiga* units overlap, particularly those involving the daily affairs of household management and the maintenance of peaceable relations among co-residents. Competitive relations often exist among an *'āiga*'s *faletama* segments. Competition among *faletama* sub-units influences their respective regulatory capacities, the nature and scope of their affairs, and the degree of autonomy that they exercise vis-à-vis other *faletama* within the same *'āiga*.

III

The concept of corporation has several implications for the structure and organisation of cognatic descent groups as contrasted to those of the unilineal variety. Perhaps the single most important point at issue is the question of whether cognatic descent groups can be properly called corporate groups. Most writers would probably agree that the corporation concept has analytic value, even though there is no consensus on what a 'corporate group' is (see discussions by Brown 1974; Cochrane 1971; Dow 1973; Fortes 1969: 291–308; Goodenough 1971). Furthermore, the difficult question of examining the structure of cognatic descent groups within long-established conceptual frameworks of corporate unilineal descent groups still remains. This means that efforts to confront the problem of cognatic descent groups as corporate units must acknowledge the significance of studies of African lineages in influencing the manner in which cognatic descent groups have been described and conceptualised.

The association of unilineal descent groups with corporations is stated by Fortes (1959; 1969), Leach (1962), and Radcliffe-Brown (1950), whose interpretations of corporation would deny, for example, the Maori *hapu* and the Samoan *'āiga* from consideration as corporate descent groups. Assertions that cognatic descent groups are characterised by 'ambiguous' memberships (Leach 1962; Radcliffe-Brown 1950: 82) and the lack of genealogical closure (Fortes 1959, 1969: 287) imply that such groups cannot act as corporate units. However, others (e.g., Caplan 1969, Firth 1963; Keesing 1968; Scheffler 1964) have noted that the structural features of genealogical 'openness' is not a valid reason for excluding cognatic descent groups from consideration as corporate units. It now appears that social units ordered by the principle of cognatic descent can, in fact, be corporate in nature, although the problem continues to be confused by differences in definitions of what constitutes 'corporateness'.

Excluding the *hapu* and the *'āiga* from consideration as corporate groups calls for reassessment of the corporation concept and its usage in the study of unilineal descent groups. Indeed, the utility of 'African models' based on corporate unilineal descent groups is now seriously questioned by several students, especially by those who have worked with cognatic descent systems. There are at present three major emphases in this reassessment process.

1. Recent approaches to the study of cognatic descent systems that do not 'fit' comfortably within the framework of African unilineal descent have taken off in several directions. Some writers, for instance, have assessed the utility of 'African

models' in understanding and accommodating the diversity of social units found in the Pacific, particularly those of the New Guinea Highlands (see Barnes 1962a; 1971: 261–4; DeLepervanche 1968; Scheffler 1973: 774–80; Strathern 1969: 1972).

2. Other studies (e.g., Gough 1971; Keesing 1970) have examined the appropriateness of using 'African models' for understanding unilineal descent systems which possess various 'cognatic' features. Research on unilineal descent groups now tends to emphasise affiliative choices available to individuals (e.g., Forde 1963; Fortes 1963; Mayer 1965), thus reflecting, for example, the growing awareness that 'optation' is not exclusive to cognatic descent systems.

3. Meanwhile, some writers question the usefulness of distinguishing between unilineal and cognatic 'types' of descent (see Goodenough 1970: 46–7, 67; Keesing 1970; 1971; Leach 1961a: 10–11; 1961b: 3–4; Southall 1971). These considerations involve in part a reassessment of the heuristic value of 'unilineal-nonunilineal' typologies and the pitfalls of 'butterfly collecting' (see Leach 1961b: 2–4).

Appropriately enough, our models change to accommodate the ethnographic complexities that we seek to describe and understand. The corporate framework employed in this article represents an important conceptual approach to the description and analysis of particular cognatic descent groups that avoids butterfly collecting because it does not attempt to classify descent groups on the basis of varying combinations of 'unilineal' and 'nonunilineal', or cognatic, features. Corporate analysis is in addition well suited for comparative purposes. Barnes (1962b: 405) rightly states that 'Comparison cannot be equated with butterfly-collecting, despite Leach's assertion. Typologies have to be constructed before comparisons or extrapolatory generalizations can be attempted.' A corporate approach enables the researcher to determine the corporate properties of social units that comprise a descent system. These properties can then be systematically compared with those of other descent systems in order: (1) to determine the similarities and differences in organisational features that obtain among different descent systems, and (2) to determine any patterned variations in the combinations of organisational features described for different descent systems.

Corporate categories. A few comments are also in order concerning the concept of category. A corporate category, in contrast to a corporate group, has no autonomy, no common affairs and no internal organisation to regulate any affairs (Brown 1974: 35; Smith 1966: 123). Like 'corporation', the term 'category' has been employed in a number of ways whose various meanings may be briefly summarised as 'an unorganised aggregate of persons who do not share an estate'. The use of the category concept assumes great importance in the study of cognatic descent groups. This is because the geographically dispersed membership of a cognatic descent group is sometimes referred to as a descent *category*, whereas, its localised core of members is viewed as a descent *group*.[22] This distinction suggests that those geographically dispersed persons who simply share consanguineal descent from a common apical ancestor do not constitute a corporate group. In other words, additional factors are necessary in order to consider a dispersed aggregate of consanguineally related persons a corporate group (see Schneider 1965: 47). This is a valid point, and I have no argument with it except to point out that (1) the geographically dispersed members of a Samoan *'āiga* do not, according to the corporate framework used here, constitute a category, and (2) a dispersed membership,

as contrasted to a descent group's localised core, is not unique to cognatic descent groups but is characteristic of certain unilineal descent groups as well.

In Samoa, certain *'āiga* affairs (e.g., maintaining the official house site and representing *'āiga* interests at village council meetings) are conducted on a continuous and regular basis by those co-resident members who are localised on their *'āiga*'s estate. Other affairs (e.g., economic contributions and selecting a candidate for a *matai* title) are sporadically conducted but are none the less important. These latter affairs are conducted by localised as well as by geographically dispersed *'āiga* members. It is not necessary, however, that every non-resident member express continuous interest in all descent group affairs. In other words, non-residence does not preclude a member's entitlement to participation in his descent group's affairs, although it is clear that non-residence will influence the nature and frequency of his participation.[23] Incumbents of titled offices attached to *'āiga* descent groups may be geographically dispersed as well; that is, title holders do not necessarily live on a continuous basis with the residential core of the *'āiga* to which their title is attached. Routine administrative affairs are delegated to a junior title holder if the senior ranking chief is temporarily absent from his *'āiga*'s estate or if he resides elsewhere (often for purposes of holding a wage job). Absentee chiefs are expected to return to their domicile[24] for periodic visits to check that *'āiga* affairs are being conducted properly. The primary right to regulate certain *'āiga* affairs rests with the office of the senior ranking chief, and these affairs may not be transferred to or conducted by junior *matai* without the concurrence and participation of the senior chief, regardless of his physical whereabouts. A dispersed membership and absentee administrators do not alter the corporate nature of *'āiga* descent groups. The internal organisational structure for regulation of *'āiga* affairs persists, irrespective of the geographic location of some of its members or administrative office holders.

The localised and dispersed memberships of *'āiga* descent groups are similar to certain exogamous unilineal descent groups characterised by a localised core of co-resident members (which also includes affines of members and other co-resident non-lineage members) in addition to out-marrying geographically dispersed lineage members who may continue to exercise varying jural rights of entitlement in their natal lineage. Murdock (1960: 1–2) refers to these exogamous unilineal descent groups associated with a unilocal pattern of marital residence as '"clans"'. In his discussion of personal kindreds and corporate descent groups, Firth (1963: 23) points out that writers tend to view a lineage as 'a tidy systematic set of people'.

> It is often assumed for analytical purposes that in a lineage activity everyone present is a member of the lineage in question, and all members are assembled. Yet rarely are all the members of a corporate descent group actually gathered together in one place. They are often simply represented by some of their number; moreover, these are commonly diluted by the presence of wives and other affinal kin coming to help with the jobs at hand. In anthropological analysis the corporate descent group is an ideational concept; it is an abstract unit in relation to units of the same type. For the people themselves it has a unitary abstract character as a component in calculating marriage-exchanges, etc. Empirically it is a smaller unit, representative and operationally selective. But while jurally the whole descent group may act as a unit, in economic and political affairs it may not do so (Firth 1963: 23–4).

We can draw a simple conclusion from these observations. Localised and dispersed descent group memberships are characteristic of certain unilineal and all corporate cognatic descent groups; and this organisational feature does not affect the

regulatory capacity of these groups for conducting their corporate affairs and activities.

Two related points emerge from discussion of these issues and problems. For one thing, cognatic descent groups are ethnographically rich in variety, and our concepts and models require some modification and rethinking in order to accommodate the full range of structurally diverse cognatic descent groups that have been reported. Secondly, cognatic descent groups do differ from the unilineal variety, although there is no consensus on what the precise nature of these differences are.

This article has been concerned with the cognatic descent groups of present-day Samoa. By describing these social units and their attached offices, I have attempted to clarify certain issues in the study of cognatic descent groups. I suggest that a corporate framework constitutes a valuable approach to the study of cognatic descent groups, and that this approach is analytically useful for comparing other kinds of corporate forms with those discussed here for Samoa.

NOTES

The field material presented in this article was gathered during seventeen months (June 1969–October 1970 and March 1971) of pre-doctoral research in American and Western Samoa with the support of the Wenner-Gren Foundation for Anthropological Research and the UCLA Department of Anthropology. Further research in Samoa was financed during the summer of 1973 (June–August) by the American Philosophical Society. The author wishes to thank these institutions for their assistance. My thinking has been influenced by the published and unpublished works of M. G. Smith and R. G. Crocombe, as well as J. D. Freeman's views (personal communications) on Samoan social organisation; however, none of these people is responsible for the interpretations presented here. I also wish to express my appreciation to the late Mata'afa, Prime Minister of Western Samoa, Fa'asuamale'aui Pātū, the late Momoiseā Imoa, and the many other Samoans who contributed to my research efforts. I am also grateful to William A. Lessa for his encouragement and assistance.

[1] Many terms may be found in the literature to refer to such groups, although 'cognatic' and 'nonunilineal' appear to be the most frequently used at present. Davenport (1959) and Goodenough (1970: 54), for example, prefer the term 'nonunilineal', which Murdock (1960: 2) has criticised as being 'negative rather than positive in its connotations'.

[2] Other Pacific cognatic descent groups include those of Rapa (Hanson 1971), the Huli of New Guinea (Glasse 1968), and the Northern Gilberts (Lambert 1966). Cognatic descent groups located elsewhere have been described by Caplan (1969), Lloyd (1966), Solien (1959), and Southall (1971).

[3] Unless noted otherwise, my remarks concerning Samoan social organisation include both the Territory of American Samoa and Western Samoa.

[4] The reader is referred to the sources cited, as a detailed discussion of Smith's corporate framework is not attempted here.

[5] An *'āiga*'s genealogy is maintained either by the incumbent of that *'āiga*'s senior ranking title or by his talking chief (*tulāfale*). Today, most genealogies have been recorded, and many title holders have come to depend upon these written accounts.

[6] The rights and obligations pertaining to land in Samoa will be considered in a future publication. Recent discussions include Holmes (1971) and Weston (1972).

[7] It could be argued that all consanguineal members' rights are contingent on 'proper behaviour'; repeated offences can and do result in an active member's expulsion from his *'āiga*'s estate. Traditionally, expulsion from an *'āiga*'s estate (or banishment from the village) was a potent means of social control. Today, however, chiefly attempts to expel a troublesome *'āiga* member may not always be recognised by the Land and Titles Court.

[8] This term is borrowed from Crocombe (1964: 30).

[9] *'Aiga* descent groups associated with very high ranking or 'paramount' titles, for example, are exceptions to the unilocalised pattern characteristic of the majority of other (lower ranking) *'āiga* (see Davidson 1967: 15–30; Freeman 1964: 556–7; Gilson 1970: 51–64).

[10] Such titles include 'ceremonial maiden' (*tāupou*) titles (Keesing 1937), certain honorific titles (*ao*) (Pitt 1970: 84–7), and paramount ('royal') titles associated with sub-district and district levels of political organisation (Davidson 1967: 24–9; Gilson 1970: 29–64).

[11] For example, the principle of consanguinity can be overridden in favour of an outstanding individual who is not a consanguineal member of the *'āiga* concerned, particularly if no suitable consanguineal member is available (see S. Tiffany 1974*a*: 42–4).

[12] A candidate may also be appointed to hold titled office by court order if a disputed title succession is taken to the Land and Titles Court (Western Samoa) or to the Land and Titles Division of the High Court (American Samoa) for adjudication. See S. Tiffany (1974*a*) for discussion of a title removal case in Western Samoa that involved both an *'āiga potopoto* and the court.

[13] It may very well be that the influence of such a person's opinion at an *'āiga potopoto* would be slight; however, I have no ethnographic evidence to support this one way or the other

[14] A *matai* title may be split between two or more co-holders by consensus of the *'āiga potopoto* or by court order. The extent to which title splitting occurred in traditional Samoa is not clearly known. Inspection of genealogical records at the Land and Titles Court in Western and American Samoa confirms my general impression that important titles were infrequently split in the past. There is indication, however, that the practice of title splitting has greatly increased in recent times and is used as an expedient by an *'āiga potopoto* commission or by the court for resolving especially bitter disputes over title successions (see S. Tiffany 1974*a*: 46–50). Discussion of the way in which a candidate for a parliamentary seat in Western Samoa attempted to gain more votes through title splitting is given elsewhere (S Tiffany 1974*c*).

[15] A 'segment' is meant to imply here that an *'āiga* descent group can be internally differentiated into a number of distinct components called *faletama*. I borrow the term from M. G Smith (1956: 40) who, in reference to segmentary lineages, describes segments as 'stable sub-units' which 'are characterised by dual differentiation in terms of interests and descent. These segments, within limits, operate as internally autonomous groups.'

[16] The unmarried dependent children of persons in ego's generation are not shown in this figure. It should be noted that it is a violation of both customary and statute law in Western Samoa to publicise the genealogy of an *'āiga* descent group.

[17] An inter-*faletama* dispute over a *tāupou* title was taken to the Land and Titles Court of Western Samoa about ten years ago. According to court records, representatives of one *faletama* claimed that members of the other *faletama* were interfering with their constituents' exclusive right to appoint an incumbent to fill the titled office. This case was eventually settled out of court when the 'intervening' *faletama* acknowledged that it had no right to participate in the decision of the *tāupou* title succession as it had been agreed several years prior to the dispute that the *tāupou* title would be divided between the two *faletama*, and each *faletama* would thus select its own incumbent (Government of Western Samoa, Department of Justice, Land and Titles Court, files).

[18] These figures are based upon my fieldwork census conducted in 1970. The personnel of one household administered by B moved away in 1972. Since his retirement from government service, Chief B spends much of his time with a married daughter who lives in a village located on a different island.

[19] I lived with Chief B for several months, and even though he grudgingly admitted that a genealogical connexion did exist between himself and A, he did not wish to acknowledge it publicly. Chief A, on the other hand, associated me with his opponent and politely avoided answering my inquiries in this regard, which accounts for the question mark in fig. 2.

[20] This dramatised the state of tension between the co-holders very well, as the two guest houses are less than fifteen feet apart from each other.

[21] The women's committee is a formal group whose membership consists of all adult women residents of a particular village. The activities of the committee are usually co-ordinated by the wives of the highest ranking chiefs of that village. There were other factors which precipitated withdrawal too, but they are not relevant to the present discussion.

[22] Scheffler, for example, makes a distinction between Choiseul Island 'cognatic descent categories' (1965: 53–62) and 'descent groups', the latter referring to the 'primary residential-proprietary segments' of Choiseulese society (1965: 92). This distinction has relevance to the Choiseulese themselves who distinguish between those who 'stayed put' as opposed to those who have 'gone out' (1965: 57).

[23] In her discussion of the cognatic descent groups of Mafia Island, Tanzania, Caplan (1969: 422) states that non-residence and infrequency of participation in descent group affairs do not preclude a consanguineal member's right to be acknowledged as a member of that particular

cognatic descent group, even if membership rights have not been activated for 'two or three generations'. Descent group membership can be re-established by invoking a remembered genealogical connexion to the descent group in question or by consulting an elder who is the repository of the descent group's genealogy. Both Samoa and Mafia Island are similar in this respect.

[24] I follow Leach's (1958: 125–6) distinction between 'residence' as one's dwelling or where one is situated at the time, and 'domicile' as one's 'official' or customary residence.

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ANIMAL SOULS AND HUMAN DESTINY IN CHAMULA

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The Chamula (Maya) Indians of southern Mexico explain much of individual fate and fortune, as well as personality differences, in terms of a system of classification of animal soul companions, ranging from jaguars for the rich and powerful, to skunks for the poor and humble. This precolumbian concept involves the association of an individual—not of a group, as signified by the concept of totemism—with a soul companion, given at birth, who shares for a lifetime every stroke of fate of its human counterpart This system and related soul concepts are articulated with the rest of Chamula society in such a way that a single conceptual vocabulary deals with such diverse aspects of life as personality and economics, destiny and sex, beauty and cosmology, illness and physical distance. Indian drawings and a native text are also included in an attempt to translate Chamula ideas about self, body, soul and destiny in relation to society and cosmos

I

This article describes and interprets some principles of selfhood, body, soul and destiny among the Chamula Indians of southern Mexico. These people make their living as subsistence maize agriculturalists and craftsmen, and as day-labourers in Chamula and in the Mexican-Mestizo community. They speak Tzotzil, which is one of the Maya family of languages. Although there is some bilingualism in Spanish and Tzotzil, perhaps as much as ten per cent. among men, the community is predominantly monolingual and is culturally conservative in its sense of separateness from Mexico national culture. The *municipio* of 40,000 persons consists of over 100 scattered patrifocally organised hamlets which are politically and ritually dependent upon a single ceremonial centre, which is in turn the community's point of articulation with Mexican government agencies and the Catholic Church. Chamula is thus the syncretic product of over four centuries of contact with Spanish and Mexican customs and institutions.

Descendants of the Ancient Maya, the Chamulas share a pan-Meso-American indigenous belief in what is generally known as *nagualismo* or *tonalismo* in the anthropological literature of the area (Adams & Rubel 1967; Colby 1967; Foster 1944; Madsen 1967; Villa Rojas 1947; 1963; Vogt 1969: 369–74). Although this ancient belief has many local variant forms, it always involves the association of an individual—not groups as generally signified in the concept of totemism—with an animal soul companion which roams in a quasi-natural habitat and shares for a lifetime the sickness and health, good and bad fortune, of its human counterpart. In many Meso-American Indian societies, the concept is specifically associated with the behaviour of witches. In others, such as Chamula, animal soul companions affect the destiny of all people in the community. The nuances and dynamics of this concept in particular communities are complex and elusive but worth pursuing

Man (N.S.) 10, 448–461.

nevertheless, for in this concept lies, in a shadowy, covert form, what might be called a philosophy of individual being. In other words, this article attempts to translate some Chamula ideas about selfhood.[1]

I shall first define some Chamula categories that relate to body, self, souls and destiny. I shall then describe the dynamics and points of structural resonance of these ideas with other parts of Chamula society and cosmology, for it is striking to note the extent to which a single conceptual vocabulary deals with aspects of Chamula life as diverse as body and beauty, illness and economics, destiny and sex, cosmology and 'theory of personality'. I will conclude with an illustration and interpretation of the concept as it appears in a narrative text.

II

People and animal souls

Chamulas believe that they live in the Fourth Creation, the most successful and most recent of four successive attempts on the part of the primary Sun/Christ deity to establish an ordered universe with proper beings. They believe that they are the only truly successful beings (speaking *bac'i k'op*, 'the true language' or Tzotzil) and that they inhabit the very centre of the square earth-island. This gives them an advantageous position in dealing with the Sun/Christ deity, for they believe that they occupy the only place on earth which benefits from the full direct rays of the sun when it is at its zenith. The sun is ultimately responsible for all life and land forms on earth, as well as for the primary categories of time and space. In fact, the boundaries of the universe are delimited by the sun's vertical path over (day) and under (night) the earth. (See Gossen 1972 and 1974, for more data on cosmology.)

Thus, man and the universe were created by the Sun/Christ and are constantly at his mercy. He is the first principle and grand overseer of order. The Sun/Christ and his kinsmen, the saints, constantly demand remembrance and supplication in the form of people's prayers, songs and sacred substances, many of which are in fact actual or metaphorical representations of heat—such as incense, tobacco, rum and candles. In analogous fashion, the whole realm of the normative and the desirable—models of and for proper human behaviour, to borrow from Clifford Geertz—is typically discussed by Chamulas in relation to the hot (desirable)/cold (not desirable) dichotomy. For example, the human life cycle begins as a cold foetus (called a 'monkey', *maš*, for monkeys preceded the creation of the Sun/Christ's order in the First Creation). It continues as an ever-increasing cycle of heat, the hottest point being reached shortly before death. However, human energy, as the cycles of sacred heat of the Sun/Christ, must have control and predictable form in order to become socially useful. From these two factors, then, accumulation of spiritual heat and control of it, derives *part* of the explanation for human difference or personality. Some Chamulas become politically, economically or spiritually dominant over others, they have hotter hearts (*mas k'išin yo?nton yu?un*). Others are less successful; they have colder hearts (*mas sikil yo?nton yu?un*). Still other states involve great heat (energy) *without* proper form or control. Examples are drunkenness, anger and witchcraft. The same key metaphors describe active, fertile sexuality (heat) as opposed to impotence, sterility or barrenness (coldness). Likewise, controlled heat serves as metaphor for describing mastery of the spoken word and evaluating individual performances, as for example, in oratory or

prayer. To speak well, by this artistic canon, is to 'speak with a heated heart.' To speak 'coldly', in contrast, is not desirable (Gossen, 1975).

In spite of their professed interest in attaining a many-faceted state of balanced spiritual heat as adults, Chamulas are well aware that people approach this ideal with varying degrees of success. Some die at the age of twenty; some live to be respected, 'hot' elders. Some achieve economic success, with regular annual surpluses of maize; others die in debt. Some are powerful and influential; others must live their lives in subservience and deference to others. Some have good health, while others are constantly ill. One of the perplexing mysteries of Chamula life, then, is the nature of individual difference and individual fortune in the world of the sun. While differing degrees and qualities of spiritual heat *describe* differences in people, these metaphors do not really *account for* or *explain* the variation. Consideration of this problem, if I may call it that, leads to the main subject of this article—just what constitutes and determines individual nature, fate and fortune in Chamula.[2]

Central to the Sun/Christ's plan for managing and maintaining order in the universe was a scheme of delegation of powers to his mother (the Moon/Virgin Mary) and several saints and other supernaturals. He gave to St Jerome (known in Tzotzil as *totik bolom*, 'Our Father Jaguar'), the responsibility for man's individual destiny.[3] The senior aspect of St Jerome sits with the Sun/Christ on the third (highest) level of the sky. His junior aspect lives in a sacred mountain called Tzontevitz which lies in Chamula territory. In both of his aspects, St Jerome carries out his mandate for overseeing human destiny through the medium of three types of spirits or souls which are associated with each and every human being.

The first of these is called *ʔora*, meaning 'time,' 'fate,' or 'destiny.' A kind of predestination which is irrevocable from birth onward, the *ʔora* for each person has the form of a multicoloured candle which is placed by St Jerome in the third (uppermost) level of the heavens at the time of an individual's birth. Different lengths represent different lifespans. The longer ones of course have potential for generating greater heat in the associated individual's life span than do the shorter candles. As long as his candle burns in the sky, the associated person and his animal soul companion live. When it goes out—either naturally, by burning up its fuel, or prematurely, through the intervention of a witch (*hʔakʼ čamel* or 'thrower of sickness')—the person and his soul companion die. Because of its crucial importance to the fate of the individual, the *ʔora* candle is nearly always symbolically present in shamanistic curing ceremonies. It is represented by multicoloured candles which are readily available in all places where Chamulas trade.

Related to *ʔora* is a second type of individual spirit. *ʔora* is represented in the living human body by the *čʼulel*, which is an invisible essence located at the tip of the tongue. It is the first spirit to become associated with the body as a foetus—symbolising the lighting of the *ʔora* candle in the sky—and is the last to depart from the body several days after physical death—symbolising the total extinction of the *ʔora* candle. It is this essence of the individual that goes to live eternally in the underworld and that returns to visit relatives each year at the Feast of the Dead (October 31 and November 1) as long as they put out food for it. During life the *čʼulel* may be afflicted by an illness called *čʼulelal*, which is generally classified as a grave illness which threatens death. Since the *čʼulel* has thirteen parts, any or all

parts may be afflicted. This causes various degrees and kinds of sickness. In the sky, *č'ulelal* illness is manifested by the flickering, toppling over or nibbled wax (all usually done by witches) of one's *ʔora* candle. A final significant fact about the *č'ulel* spirit is that it is also present, with all of the same attributes, on the tip of the tongue of one's animal soul companion.

This leads to the third type of individual spirit, the animal soul companion itself, called *čanul*, deriving from *čon*, which means 'animal.' The *čanul* is assigned to the individual at birth by St Jerome and the Sun/Christ. This animal shares every stroke of fortune that its human counter-part experiences. It is also of the same sex as its human associate. The *čanul* has two 'aspects,' a junior (*ʔie'inal*) and a senior (*bankilal*), each of which has thirteen parts.[4] The junior aspect of the *čanul* lives in the sacred mountain named Tzontevitz located in Chamula municipal territory, where it is tended by the junior aspect of St Jerome. The senior aspect of the soul animal lives on the third level of the sky where it is tended by the senior aspect of St Jerome. During the day, these associated animals roam the woods and fields of their territories much as an ordinary animal would. However, at night, St Jerome, in both his junior and senior aspects, herds them into corrals—the junior one on the sacred mountain, the senior in the third level of the sky. Here they spend the night in relative safety from the perils of darkness—a time when most misfortunes, particularly those caused by witchcraft, occur. It is in order to guarantee this night-time care that one must make occasional prayers of supplication to St Jerome. (See figures 1 and 2 for Chamula Indian drawings of these animal soul corrals and St Jerome, the attendant.)

By far the most common cause of illness in Chamula is believed to be the loss of or injury inflicted upon one or several of the twenty-six parts of the *čanul* (thirteen for each aspect, junior and senior). Loss can occur during sexual intercourse, fright, excitement, pleasure, anger or accident. Thus human illnesses are typically caused by numerous afflictions of the *čanul*, and, somewhat less frequently, from the afflictions of the *č'ulel*, associated with the candle of destiny in the sky. This makes



FIGURE 1. Animal soul companions (senior aspect) in their corral in the third level of the sky. Drawing by Marián López Calixto. Note dead animal (a coyote) in the foreground; the artist explained that this coyote and his human counterpart died after an attack from the animal soul companion of a witch. St Jerome, patron of animal souls, is helping a small animal (a deer) into the corral.

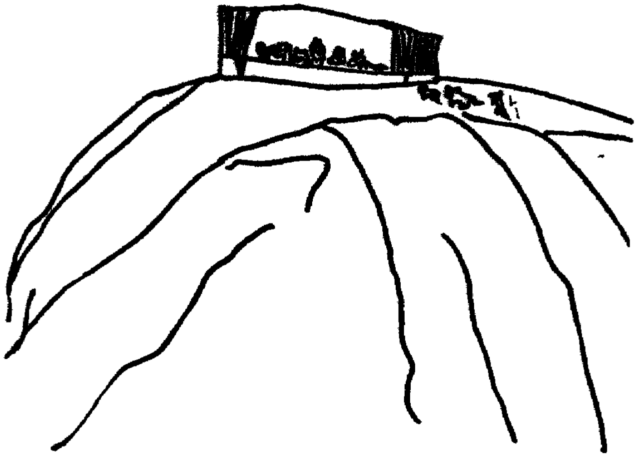


FIGURE 2. Animal soul companions (junior aspect) in their corral near the top of Tzontevitz mountain, in Chamula *municipio*. Drawing by Marián López Calixto. The artist explained that the three animals waiting to the right of the corral are the soul companion animals of witches (here shown as cow, a dog, and a horse); they are waiting for 'good' soul companion animals to venture out of the corral.

a total of thirty-nine parts of the human spirit (twenty-six for the double *čanul* and thirteen for the *č'ulel*) which can be afflicted singly, plus a very large number of combinations of parts which can be jointly afflicted or lost. It is the shaman's role to ascertain by pulsing and by observation of symptoms what parts of the soul are afflicted. He must then prescribe the necessary candles, herbs, flowers and foods and say the proper prayers at a formal curing ceremony to restore health and integrity to the soul configuration. (See Vogt 1969: 416–76 for detail on curing ceremonies in neighbouring Tzotzil-speaking Zinacantan.)

All animal soul companions are wild animals with five digits. It is this sympathetic trait—five digits—which establishes the special tie between mankind and these animals. It is significant to note here that the Tzotzil word for 'man' (*vinik*) is the same as the word for 'twenty,' indicating that the total of twenty digits is considered to be a striking aspect of human physiology and an important anatomical trait of soul companion animals as well.[5] All are mammals. Furthermore, they are ranked according to three levels. The third level is the most *bankilal* ('senior') and thus includes those animals associated with rich and powerful individuals; the second level with moderately successful people; and the first with the humble and the poor. The only animal which all informants agreed to be a third level *čanul* was the jaguar. The most powerful shamans and political and religious leaders typically have the jaguar soul companion. In the second level are generally ranked the coyote, weasel and ocelot. The first level (most *ʔiʔtsʼinal* or 'junior') typically includes the rabbit, opossum and skunk. There is general agreement among informants about the jaguar's first rank position and about the low rank of opossum, skunk and rabbit. However, the second level is not consistently reported from individual to individual. (See table 1, below.)

TABLE 1. *The science of the concrete: Chamula animal souls*

Wild soul companion animals (5 toes, like humans)		*Domestic and other anomalous animals* (Cloven hooves, claws, etc., unlike humans)
Carnivores	Herbivores and Omnivores	Hervibores and Omnivores
Stronger people, rich people and cargo-holders.	Weaker people, ordinary people.	Witches have one of these plus a strong wild animal soul companion.
jaguar ocelot coyote fox weasel	rabbit skunk raccoon opossum squirrel	cattle sheep pigs chickens turkeys wild birds insects
Early creation. Distant habitat (lowlands, hot). Infrequently seen, nocturnal.	Later creation. More frequently seen in highlands Nocturnal, diurnal and crepuscular.	Recent creation. Seen every day near house compounds. Diurnal.

Note the paradox: that some of the same animals which are used for ritual sacrifice in curing ceremonies and other rituals also are the familiars of witches. Ambiguous power that makes them effective symbols makes cattle, chickens and turkeys useful for both sacred and asocial ends. Sheep are never eaten, ritually or otherwise; pork is eaten only occasionally.

I am of course indebted to the work of Claude Lévi-Strauss (1966) in my attempt, shown in this table, to translate the logic of animal soul classification.

These beliefs are related to the reluctance of Chamulas to kill any of these animals, for they might thereby be killing themselves or a relative. This is not to say that they do not do so, only that they kill them with some fear and trepidation. *Most* individuals of these species are not animal soul companions of anyone; yet there is always a dangerous uncertainty. Narrative folklore of Chamula abounds with reports of such misfortunes. The following is an abstract of a 'true recent narrative' about a Chamula woman's mistaken killing of her own soul companion and her consequent death.[6]

> There was a woman who scolded a weasel, not knowing it was actually her own soul animal. The weasel was angry and decided to eat the woman's baby chicks and eggs that had not yet hatched. The woman saw the weasel go into the chicken's nest and ran it off, chasing it to its cave. She dug it out and clubbed it to death and then burned it up with fuel oil. She died three days later, for it had been her own soul animal.

Although people should have only one *čanul*, witches have a strong animal soul companion *plus* an aberrant, anomalous creature that is outside the class of animal soul companion creatures. It is significant that the number and type of digits, e.g., claws and cloven hooves, are the traits which Chamulas note as making them 'different' from soul companion animals. Such second animal souls for witches are typically domestic animals and fowl and wild birds. It appears that the anomalous classificatory position of domestic animals—neither 'of the woods' nor human—gives them important intermediary power both as sacrificial animals (particularly so in the case of chickens and turkeys) and as witches' soul companions. Witches require the power of a strong animal soul companion so as to be dominant over

all other *čanuletik*, in order to do them ill if their clients so desire; they also require the anomalous animal in order to give them access to places such as house compounds where they will not be noticed. This enables them to do evil things surreptitiously, as in the form of a turkey or chicken, for potential victims would normally not expect a domestic animal or bird to hurt them. Furthermore, domestic animals are constantly present in and around the living compounds of all families, thus giving them intimate contact with the members of the household. This enables them to harm individuals in the household more easily. (See table 1, above.)

All people, including witches, may learn the identity of their animal soul companion through dreams (see text below). Typically, people have the same dream three times. Although an individual's animal soul companion figures prominently in this dream, its meaning is not always clear. In these cases, a shaman must be consulted. While shamans and witches can usually make sure of the meaning of their own and their clients' animal soul dreams, many people are not sure of the identity of their soul animals even in adulthood. Even if they do know, they are reluctant to discuss the matter publicly.

In sum, then, the factors which determine individual fate and fortune are almost completely beyond the individual's control. They derive primarily from the Sun/Christ and St Jerome, the candle of fate, and from the junior and senior aspects of the animal soul companion. The healthy body carries this constellation of influences in equilibrium; it is the passive bearer of forces over which it really has no control. The afflicted body suffers from a disequilibrium situation in the whole; a part is missing, injured or destroyed. As preventative measures, about the only ways an individual can affect his own destiny are 1) to pay homage to the Sun/Christ, St Jerome and the other saints so as to encourage them to continue their vigilance and compassion for mankind, and 2) to be a conservative, conforming, generous individual so as not to attract anger or jealousy, for these could be turned into attacks of witchcraft. As a curative measure, the only traditional way to remedy one's afflicted condition is to hire a shaman to intervene on one's behalf by performing rituals (called *k'elel* or *ʔilel*, 'watching over' or 'seeing' respectively) whose primary goal is to restore an equilibrium situation to the several individual spirits which affect the organism.

III

Dynamics

Four Chamula axioms about selfhood may be gleaned from the above. First, one's health and fortune are governed by a complex set of extra-somatic forces which one may influence only modestly, and then only with the help of a specialist Second, living in health and happiness amounts simply to coping with the inevitable. Third, all people are not equal. This principle follows from the simple fact that some people are born with a jaguar soul companion and others with a rabbit. Never shall the twain meet, except in a dominance-submission relationship. To fulfil one's destiny, then, is essentially a matter of discovering, accepting, and coping with oneself; that is, to discover what one's animal soul probably is and its

relative position in the ranked order of beings. This information reveals one's place in the social universe and in its image, the spiritual universe. A fourth axiom relates to the individual's discovery process of his situation: although preordained, one's long and short range health and fortune are discovered after the fact, by *a posteriori* reasoning.

The discovery process is complex. It requires an understanding of the cosmos and of the condition of one's *ʔora* candle in the sky in relation to those of others. It also requires knowledge of the identity of one's animal soul companion and its 'role' in the universe of soul companion animals.

Of these requirements in the discovery process, an understanding of the cosmos is the most accessible to all Chamulas. The primacy of the Sun/Christ as the first principle of time and space is stated countless times in narratives and in related ritual sequences. The first notion of being was the Sun/Christ's; every adult knows about his original delimitation of the universe and his first vertical orbit—under and then over the earth island—in the First Creation, and about his four efforts in the successive Four Creations to create satisfactory beings. Even now, in the Fourth Creation, the Sun/Christ remains the key symbol of order as evidenced in the seasons and in the daily cycles of heat and light, which are viewed as essential to human survival. Human destiny is therefore ultimately tied to the continuing benevolence of the sun deity and his kinsmen, the saints. This principle is so fundamental as to constitute common knowledge (see Gossen 1972; 1974). Yet nearly always the state of the deities' satisfaction with one's behaviour is known only after good or bad fortune occurs.

Less accessible to ordinary people is knowledge about one's *ʔora* candle of destiny in the sky. Only the best shamans and most dangerous witches claim to know this with certainty: to find out about it requires a strenuous journey of the specialist's animal soul companion to the site of these candles on the third level of the sky. I have been told that it is not usually considered worth the trouble, for 1) it is hard to get there, and 2) it is very hard to change the condition of the *ʔora* candle except by special petition to St Jerome or the Sun/Christ. However, these soul journeys and petitions are made in the case of grave illnesses which are believed to involve a toppled or damaged *ʔora* candle. Good shamans are able to ascertain the general condition of a patient's *č'ulel* by pulsing his wrist. Thus, although absolute knowledge about the candle itself is extremely hard to come by, shamans have as their stock and trade the ability to read the 'reflex' presence of it (all thirteen parts) in the form of the *č'ulel*.

Discovery of the identity and relative power of one's *čanul* or animal soul companion is also hard to accomplish with absolute certainty. Humble people are said to have only the general understanding that they have one of the humble animals—for example, a rabbit or an opossum. However, most shamans and major religious figures and political figures have this information at a much 'firmer' level of certainty. By far the most common medium of discovery is the dream. In this way—usually through a dream which occurs three times—the person discovers the identity and relative social position of his animal soul in the hierarchy of power and influence which prevails among all of the soul companions. The person may also discover the favoured (or doubtful) position of his soul companion in relation to the overseer, 'Our Father Jaguar' or St Jerome.

IV

A text

In order to base the concluding remarks of this article upon substantive data rather than on my own modest efforts to paraphrase and translate Chamula abstractions, I would like to quote in full a Chamula Tzotzil narrative. Originally recited by a shaman as evidence for his own strength and effectiveness as a curer, this testimony chronicles a dream in which the shaman discovered the extraordinary strength of his jaguar soul companion.[7]

Concerning Animal Soul Companions

Well, my godfather told me this story when he had a dream one time when he was sleeping.

A jaguar walked towards a road. When he arrived at the road, he climbed a tree When he saw a man coming, he climbed down and bit him quickly; then he suddenly grabbed him He cut out his heart and ate it along with the tongue; but he didn't eat all of them. He just ate a little bit He carried them to his cave When he arrived at the cave, he gave little bites to his friends because they were hungry. That is why he gave a little bite to each of his relatives They were afraid to go out to look for their own food, so every night he went out to look for food. He did nothing but eat people Every night he went to the place where the big tree was.

Finally the people realised that there was a jaguar by the road. They went to kill him, they went at night to kill him. They took their guns. When they arrived at the place where the jaguar was, they found him sitting in the tree When the hunters arrived, they turned on their lights and saw him trapped in the tree They saw that he was sitting there. They took a shot [at him]. Then my godfather saw that they had shot at him He knew right away when the gun fired. He stopped the bullet He grabbed it with his hand The gun roared again, this time he didn't stop the bullet. It hit him in the ribs. When the bullet hit him in the ribs, he quickly lifted his hair and cleaned the place where the bullet had entered That is why there wasn't a lot of blood. The gun roared again. The bullet missed him. He grabbed it in his hand. Finally he realised that he was not going to be able to escape the bullets He scrambled down the tree, fleeing He went to hide in the hills. He didn't die When my godfather saw the jaguar flee to the hills, he woke up. He was really trembling! He was very scared, but he was still lying there in bed. His animal soul had been scared His body was still asleep in his bed.

But when my godfather woke up, he had a terrible headache. He had a fever, he couldn't get out of bed. He almost died. He was in bed for two weeks. He slowly recovered, but he didn't eat for four days He only drank a little corn gruel. Since his animal soul had defended himself with his hands, they looked as if they had been burned. Just imagine, all the skin peeled off his hands! He didn't work for four weeks. My, but he was very frightened. The fact is that he almost died. Then, when he got better, he talked. He told me. 'I was very sick. I almost died I didn't eat for four weeks, and all the skin simply peeled off my hands. They really hurt,' said my godfather. 'The fact is that I [i.e., my animal soul] went along the road eating people, and then they were going to kill me It's that they realised that the road was dangerous. They realised that there was a jaguar on the road. Then they went to kill the jaguar, but he was my soul animal. That is why I almost died They shot at me many times, but they didn't hit me. The bullets went to one side. I grabbed the bullets with my hand Since my hand protected me, all the skin came off,' said my godfather. 'But this happened a long time ago,' he said. That is how he found out that his soul animal was a jaguar, for he saw him while he was sleeping 'I am on the third level. I am the most powerful I know that I am on the third level. That is how I defend my friends,' my godfather said to me. It is because the jaguar is the strongest [animal soul]. He is on the third level. The opossum, the weasel and the fox are on the first level. They are the weakest. The first level is for the souls of people who don't know how to cure. The second level is for the animal souls of those who can cure a little bit. The third level is for the most powerful curers That is where the animal souls of the witches are, and also those of good curers. I was told this because the man was my godfather.

(Translation by Eleanor A. Gossen.)

V

A reading

The most striking revelation in the narrative is the strict identity relationship between the shaman and his soul companion. The jaguar is not merely a metaphor for the narrator's being, but is an integral, causal component of his state of being. All that happened to the jaguar also happened to his human counterpart. It is not entirely clear whether the informant's godfather was a witch (for his jaguar soul companion actually killed many people) or whether he was a virtuous shaman interested in ridding the community of witches or perhaps of other non-Chamulas. In the latter case, the murders would not be asocial acts but rather a demonstration of the man's (jaguar's) moral strength and interest in the community's welfare. The informant who told me the narrative suggested the latter interpretation by noting that the victims were probably witches themselves, non-Chamulas full of evil designs on the fortunes of Chamulas. In this case, their deaths were a good thing. Thus, a theme of altruism runs through the text. This attitude justifies the jaguar's fierceness. He does his killing for the sake of his relatives and his community. His actions are purely selfless. To use his words, 'That is how I defend my friends.'

Another important theme which runs throughout the text is predatory biting and eating behaviour. This trait first occurred among the people of the First Creation and is now identified with current (Fourth Creation) asocial human behaviour in distant, non-Chamula places. For example, Chamulas constantly asked my wife and me if people bit and ate one another in our country, since they knew that we came from very far away. The logic of this favourite question became clear when we learned that this indeed was the habit of the people of the First Creation. That was one of the reasons why the Sun/Christ destroyed these people, for they made stewing meat of their own children. Thus, it is significant that great social distance, deep antiquity *and* great supernatural strength (such as that exhibited by the jaguar) share a *possibly* predatory orientation to the rest of the universe. To be exact, strong animal souls are associated with the totality of human experience through the four creations (large animals, particularly jaguars, have been around continuously since the First Creation, according to narrative accounts). Furthermore, they are associated with the most distant reaches of space on earth, in that their habitat is the tropical lowlands, an area populated by Spanish-speaking Mexicans, whom Chamulas do not like or trust. The lowlands are believed to reach to the very edges of the earth, far away from their cool highland home in the centre of the earth. In contrast, smaller and less powerful soul companion animals were first created in the Second and Third Creations and thus do not have the cumulative temporal experience which the larger ones do. Also, most of these humbler creatures are found closer to the Chamulas' home highlands, making them more numerous, more visible and more vulnerable. All of this makes sense when one reflects on the fact that, among humans, there are certainly more of the weak than there are of the strong.

Thus, basic principles of time and space seem to be underlying factors which give order not only to the strength of jaguars vis-à-vis other soul companion animals, but also to the general concept of animal souls as a component of individual being. This information is summarised in table 1. In general, strong animals,

particularly jaguars, have distant time-space associations, giving them a kind of overview of universal experience. Furthermore, the strongest soul animals are nocturnal carnivores that are infrequently, if ever, seen in the highlands. In contrast, the weakest soul animals (e.g., skunks and rabbits) are diurnal and crepuscular herbivores and omnivores who are associated with the very close reaches of time and space known to Chamulas, i.e., the areas around their home compounds. They are seen frequently, and are often killed, thus explaining in explicit fashion the vulnerability of the weak and the poor.

Returning to the text, the shaman appears to have ambivalent feelings about control of his power, i.e., how to explain his jaguar soul animal's attacks on others. Although he states that this behaviour amounted to an assertion of his strength for defence of his weaker fellows, the chronicle of events does not make the jaguar's motives entirely clear. It appears that a predatory attitude of an individual's own soul animal with regard to others' animal souls may be used for the defence of the community as well as for its detriment. This is exactly the ambivalent position of powerful shamans and politicians vis-à-vis the community. They could easily 'bite' their countrymen, but do not. This is the supremely social component of their behaviour. They are the defenders of the community from evil and chaos, for their souls are hot (*k'išin*), analogous to the essence of the Sun/Christ deity, the original life-giving force who has the power to destroy as well as to create and maintain the social order. It is significant that the soul animals of the humble are believed to be 'colder' than those of the powerful. This follows from the fact that they are more distant from the symbolic essence and example of the Sun/Christ deity than, say, the jaguar. Related to this belief, it is generally known from narratives that smaller animals were created later, usually in the Second or Third Creations. They have thus had less experience, as species, with the enduring cumulative truths of the universe, embodied in the sun. They are in this way 'junior' (*ʔie'mal*) to the more senior soul animals (*bankilal*), and so have less heat. Thus, much of human difference, inequality and diversity of fate and fortune are related to deep-lying principles of cosmos which derive from sun-primacy in Chamula thought.

VI

Conclusions

With this, we come full circle and may note briefly in conclusion that Chamula concepts of selfhood, destiny, health and individual personality share an evaluative vocabulary with many other aspects of Chamula life. This shared evaluative language focuses upon the key hot/cold value syndrome which is derived in part from the place of the sun deity in the Chamula cosmos. From economic success to active sexuality, from high alcoholic content of rum to canons of beauty in the spoken word, from political power to the qualities of the elderly which command respect and deference, from views of emotion to ideas about personality, there is a key metaphor—high energy, or heat, contained by form, i.e., rule-governed behaviour—which subsumes these diverse evaluative domains. The higher the energy, the greater the potential for goodness *and* the greater the possibility of destructive, asocial behaviour.

Given the social reality which this evaluative scheme apparently has for Chamula society as a whole, what do individuals think about their overall condition or state of well-being? It is precarious, at best. Just as the sun might at any moment destroy the Fourth Creation, so the most powerful shaman might resort to witchcraft or a spark from the hearth might burn down a thatched house. This fundamental uncertainty, combined with the complex set of forces which determines individual fate, makes what Westerners might call 'spiritual peace' or 'general happiness' a state of mind which does not occur in the Chamula repertory of emotion. A person is constantly buffeted about by at least four spheres of fortune, described above, which are not really under his control. Even power and wealth and glory are thrust upon one, and, even then, the condition may be ephemeral, vulnerable to sabotage by a witch or subject to a capricious decision on the part of the Sun/Christ or his associates. The Fourth Creation might even suddenly come to an end. The best one can expect is, perhaps, to have a 'contented heart' (*lek yo?nton*), to know that, for the moment, the several forces acting upon one are in an equilibrium state, that one is apparently coping with the niche one is destined to occupy. This term is also used to describe a pleasant, agreeable person. It is interesting in this regard to note that the state of the 'contented heart' is most often described by Chamulas in festival situations, times of ideal ritual statements of the normative order, times of abundant tobacco, rum, incense, fireworks and music—all symbols of heat and all susceptible to direct sensory experience of participants. All people—not just those naturally endowed with a jaguar soul, a long, fat candle of destiny and a favoured position in the eyes of the Sun/Christ—may experience for the moment of the festival the 'contented heart,' an ideal state of hot equilibrium, when all is as it should be, jaguars and opossums treating one another with due respect, knowing their respective places in the world of the sun. It is surely not unrelated to this discussion that the Chamula Tzotzil word 'festival' derives from a proto-Maya word (*kinh*) meaning 'sun, heat, time and divinity' (see León-Portilla 1968: 34–40).

By far the more common condition of emotion which people describe is to have a 'sad heart' (*šat yo?nton*). This condition is best translated as 'uncertainty.' It need not be accompanied by sickness or even explicit worries; it is simply a 'normal' state of anxiety about whether one is coping with one's niche in life. Except for strong shamans and other powerful people who are believed to have special insight into the mysteries of selfhood and individual destiny (shaman, *h?ilol*, actually means 'seer' in Tzotzil), it nearly always happens that one learns the 'truth' about one's personal situation only after good or ill fortune has already struck. For ordinary people, fate is intelligible only in retrospect; hence, Chamulas' fatalism and apparent passivity before the inevitable.[8]

NOTES

This article was originally prepared for a symposium on Images of Body States, organised by Shelley Errington for the 1973 meetings of the American Anthropological Association, in New Orleans.

My fieldwork (1968–9) in Chamula, state of Chiapas, was undertaken as a part of the Harvard Chiapas Project under the directorship of Evon Z. Vogt I am indebted to him and to many members of the Project for background ethnographic work and for many years of ideas, encouragement and criticism, which are the good things about a long-term regional project. Dane Archer, Shelley Errington, Triloki Pandey, Thomas Rohlen and Victor Turner have

read drafts of this article, and have provided much useful comment and criticism. I, of course, remain responsible for the article as it stands. Detail on Chamula ethnography and cosmology may be found in Pozas (1959, 1962) and Gossen (1971; 1972; 1974).

1 'Selfhood', as used in this article, refers to the body of Chamula folk beliefs pertaining to the nature of self in relation to society.

2 The problem is not unlike that considered by Evans-Pritchard in his classic account, *Witchcraft, oracles and magic among the Azande* (1937), yet the Chamula case differs in that it involves witchcraft as only one of a configuration of factors which impinge upon the fate and fortune of the individual.

3 It appears that Saint Jerome acquired this role in the syncretic response to Catholic missionisation because Spanish images of him typically include a lion lying at his feet. This probably made him the most appropriate saint for filling the slot in the pre-Columbian pantheon for lord or patron of animal souls. That St Jerome is still known as Our Father Jaguar suggests that the jaguar deity filled this slot in the pre-Columbian period, a likely case since the cult of the jaguar figures very prominently as a motif in Meso-American religious art and sculpture since at least the time of the Olmec Civilisation in the Meso-American Pre-Classic (Coe 1965).

4 This junior/senior ordering principle is well known in the Maya area of Meso-America It is used to classify phenomena all the way from sibling terminology to mountains. Generally, older, larger, richer, more distant and more powerful are 'senior' qualities. Younger, smaller, poorer, closer and less powerful are 'junior' qualities (See Vogt 1969: 230–45)

5 It is worth noting that Ancient Maya numeration and calendrical reckoning, as well as most modern Maya counting systems, are of the base-20 type (see Coe 1966). For example, the modern Tzotzil word for 'forty' is *ča?vinik* ('two twenties' or 'two men').

6 'True recent narrative' (*bae'i ?ač'k'op*) is one of two genres of serious prose narrative which the Chamulas recognise. The other genre is called 'true ancient narrative.' Both are believed to be true reports of past events, the distinguishing attribute being time of occurrence, 'true recent narrative' reports events of the present, Fourth Creation, while 'true ancient narrative' deals with narratives about the First, Second, and Third Creations. See Gossen 1971 and 1974 for more detail on Chamula narrative.

7 This text was told to me in 1968 by Salvador Guzmán Bakbolom, age 30, of Milpoleta hamlet. He originally heard the tale from his godfather, who was then a practising shaman The godfather was trying to persuade a neighbour to hire him to cure a member of the neighbour's family who was ill The text is typical of nearly all Chamula narratives in that its performance context was one of providing evidence and supporting anecdotal data for a specific social situation. In other words, neither this text nor hundreds of others which I heard and recorded were told, when in proper social context, for 'entertainment value' alone

8 On a more speculative note, it is certainly possible that the remarkable intellectual superstructure of the Ancient Classic Maya may have been an effort to reconcile, at an institutional, 'state' level, some of the ambiguities in their view of individual being. Authorities describe Ancient Maya astronomy, mathematics, calendrics, writing and art as modes of calculating and recording the pattern of divine temporal cycles which converge to 'influence' or 'cause' events at a given moment (Coe 1966). Miguel León-Portilla has called Ancient Maya religion and philosophy a 'chronovision,' a systematic astrological scheme which could locate, in fact predict, the pattern of divine temporal cycles which served as the guiding 'sign' of each given moment of human existence (León-Portilla 1968). The theocratic state may, in a general sense, have legitimised its existence to the populace by providing a full-time group of chiefs, priests and astronomers who had the gift of prophecy—providers of information which the population at large needed to guide its everyday life. This information included such items as propitious dates to plant maize and cut timber, as well as proper times and modes for appealing to the deities for continued well-being After the fall of the Classic Maya and the spasm of the Conquest, the chronovision no doubt continued among the Maya maize farmers as a headless astrological system, a fatalistic view of body and person without a professional prophetic class whose job it was to clarify and control destiny. Chamulas' view of individual being has most of the qualities, such as preordination of fate, of the ancient chronovision with only a few of the institutionalised media which may have provided clarity in Maya antiquity.

Turning to the future, also on a speculative note, it seems clear that planned social and economic change, such as that charted for Chamula by Mexico's Instituto Nacional Indigenista (Department of Indian Affairs) will surely encounter insurmountable resistance in this and other Indian communities if planners do not acknowledge and reckon with the chasm which separates indigenous concepts of destiny, selfhood, self-determination, free will and future orientation from those of the West. In fact, it is entirely possible that the relative cultural conservatism of the Maya area of the New World (amounting to some 3,000,000 persons in

Mexico, Guatemala and Honduras) may be in part explained by the remarkable cultural insulation provided by the Maya concepts of self and destiny. Always apparently incorporated into domestic ritual practice (such as curing), rather than into public ceremonial affairs, concepts such as animal souls may have provided a secure reservoir of Indian identity and social separateness even in the midst of colonial and twentieth century European influences.

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THE LEFT-RIGHT SUBCASTES IN SOUTH INDIA: A CRITIQUE

Review Article

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BECK, BRENDA E. F. *A study of right and left subcastes in south India.* 334 pp., illus., plates, tables, bibliogr. Vancouver: Univ. of British Columbia Press, 1972

I

This is a fine book, one of the best ethnographies of South Asia I have read. It is a rich social structural study of a region in South India, the Konku Natu, and in many ways represents a unique approach to ethnography. In general anthropologists have favoured the intensive study of a single community: the study of the region is at best secondary. Brenda Beck's approach is to study a series of interlocking territorial based social structures important in indigenous thinking: the larger region Konku Natu, one of the five traditional regions in Tamilnadu (Madras state); the subregion; the revenue village; the hamlet (*ur*) and the individual household (Kutumpam). Each unit is described in great detail, and its position in the larger setting is contextualized. Thus the five regions (NATU) are described in general terms: and the Konku in depth, illustrating the place of the specific region within the larger administrative unit. So it is for the other units: the region has twenty-four subregions, but the author studies in detail the *Kankayam natu*, furnishing in-depth information on this subregion; and so on. Each unit is dealt with in an orderly and systematic way in a single chapter. The social units are presented in a diminishing order of size, the smaller the unit the more intensively is it studied. Thus the fifth unit—the household is studied microscopically as it were, in very rich detail.

It is a highly innovative approach, and is a 'model' for approaching the study of the larger region within which is contained the local community. But it is obviously no easy task and requires a thorough familiarity with the language and culture of the whole region. An extraordinary amount of patient research has gone into the book: tables and maps with the pertinent sociological information are presented to the reader every step of the way, so that the book, though difficult to read, has a clear, systematic argument running through it.

The book has two main theoretical approaches: on the one hand in its empirical detail the work is in the best tradition of British social anthropology, in particular the *internal structure* of the hamlet and household units (chapters 4 & 5) where marriage alliance, descent groups, and familial units are discussed. The detailed study of caste precedence based on a Guttman scale (and influenced by MacKim Marriott's work) is also of this order (160–74). Over and above this level of analysis is a struc-

tural one, in the French sense of the term: a preoccupation with oppositions, and binary categories. On this level the author transcends grubby empiricism altogether. The major structural feature is the division of the social groups (subcastes) of the region, village and hamlet into two overarching social divisions: right and left (table 1, page 5). This is an indigenous classification, which the author claims, is central to understanding the empirical reality described by her. Thus if the society is hierarchically organised in terms of castes (or subcastes), as it is all over India, they are also organised into left:right divisions, in this region. The several *different* castes in each binary division have *common* attributes manifest at every level of social organisation in opposition to the subcastes of the other division. Cutting across the *jati* and *varna* structure is the dualism of right and left, which is also crucial in determining status, style of life and religious orientations of each division. I find the social anthropology consistently excellent; by contrast I have some misgivings regarding the 'structural' argument which I will pin-point in the following pages.

II

The proper understanding of left:right dualism, according to the author, requires a philosophical perspective on Indian civilisation. Both *varna* and the dualistic scheme depend on Indian notions of *power*, she says. Thus the Brahmin has power over the gods and the cosmos; the Kshatriya over mortals, the Vaisya (and economically affluent castes) over material things. I find this notion of power too diffuse: if everything is power, then nothing is power also. It is not then useful for analytical purposes unless of course this idea of power is an indigenous notion, conceptualised in language (which is doubtful). Purity is power also. ...'For the Hindu ... purity is linked to the ultimate power that human beings can achieve, control over the elements of the universe through control of one's intimate physical processes' (7). I know of no Hindu who believes in this preposterous idea, except perhaps in fantasy, and furthermore such an interpretation seems unwarranted in terms of Hindu philosophy. This vague definition of power is central to her argument, since the left:right distinction is based on it. 'The bifurcate body serves as a metaphorical expression of how rivalries for status develop in an actual social setting, given this concept of social hierarchy based on different types of power' (8).

How does this process occur? The Brahmins are axiomatically on top of the social ladder; once this is granted the allocation of prestige is difficult and the Kshatriya-landowner category must stand out in opposition to them. Subcastes bound to the landowner subcaste by *jajmani* ties will align on that side: the right. The other groups move to the *left*, emulating the Brahmin scheme of values. Strictly speaking, she argues, the Brahmin (and the Narunikar Pillai subcaste of scribes) are neutral or may be viewed 'as forming the "head" for that social order' (xvi). Thus the image is that of a 'body social', with the head Brahmin, and the two sides, right and left.

Several difficulties arise at this point.

(a) The left:right classification is widespread in India (and elsewhere), but the 'head' and 'body social' idea is Beck's own. Now the Indian idea of the body politic is embedded in the *varna* scheme, head: Brahmin; arms: Kshatriya; belly:

Vaisya; feet: Sudra. The right:left distinction in other contexts is never associated with the body politic idea in India at all. Brenda Beck takes the body-politic idea in the *varna* scheme and superimposes it on the right:left scheme, viz.

head

left right

Contrary to Beck it seems to me that the indigenous distinction is unequivocally left:right, and is altogether a different type of classificatory principle from the 'body social' one.

(b) The head (Brahmin) is somewhat tilted to the left since it is a model for the left castes. The left castes, according to the author, emphasise Brahmanic values, ritual purity, textual learning and the great philosophical traditions (234), whereas the right division is less particular on all these dimensions There is I feel something fundamentally wrong here. In almost every other context the left:right distinction is associated with ideas like impure:pure; unpropitious.propitious. For example, in Indian thinking circumambulation clockwise is an auspicious *right* movement; and anti-clockwise circumambulation is a *left* and inauspicious movement, associated with death, and the kind of inferior ritual noted by the author herself (206). Finally the left hand is polluted, whereas the right is the noble one.

This being the case it seems strange, in Hindu society, that the very castes who emphasise ritual and sacred tradition should be classed as inferior (left) in contrast to those who are more given to meat eating and non-traditional practices (right). Furthermore the author's claim that the left division castes emphasise these Hindu values are contradicted by the data: the most polluting castes, like leatherworkers, are *also* on the left, and it is inconceivable to me that five inferior left castes (C., D and E. in table 1, p. 5) would emphasise the Hindu *philosophical* traditions. Clearly, the dualistic system has to be explained differently.

III

The first methodological error of the author is that her splendid ethnographic data *of the present day* are reclassified in terms of a left:right scheme of a historical *past*. Though one may dispute Burton Stein's claim that the intense factionalism and open conflicts between the two divisions in the post-fourteenth century Vijayanagara period indicate the decline of this dualistic system, even Brenda Beck admits that its viable presence was in 'earlier centuries' (xiv) (Stein 1969: 196). The custom had badly declined in the early nineteenth century when Abbé Dubois was writing (Dubois & Beachamp 1906). The author's own information was collected from the memories of six informants who could not agree among themselves (xv). Furthermore the author admits that the left:right division is not altogether clear: one of the largest on the left, the Mutaliyars in fact have it both ways (233), and have 'divisional ambivalence' (239). In fact, as you go down the status hierarchy the divisions get blurred (8), which means that most of the castes in fact have 'divisional ambivalence'. The methodological problem is, how can one order present empirical reality according to categories no longer employed by the people themselves? The likelihood is that the categories are no longer employed because they are *inapplicable to present day society*. The sociological problem then is why the categories are no longer employed, the very opposite of the author's. This must in

turn bring us to modern demographic and socio-economic changes which the author, given her synchronic structuralist orientation, simply does not discuss. Also, in order to study its onetime operative significance, one has to see the evolution of this dualism in historical perspective.

One sociological characteristic of the left: right division is reasonably clear from the author's work, and Hutton (1961: 70) and Stein (1969: 187), namely the distinction between artisan groups (left) and agriculturalists (right). Furthermore, Beck shows that in general the right groups are involved in traditional service (*jajmani*) relationships with the dominant Vellalar Kavuntars, whereas the left are not. The left do not refer disputes to the dominant caste council (77). The evidence in chapter one, and from the myths (91–93) is that the Vellalar Kavuntars, the present day dominant caste of Konku region, were the first settlers in that area, followed by the Natars. The lower castes formed a stable core round the dominant Kavuntar in a *jajmani* network, as the author herself recognises. From the fourteenth century onwards there were several waves of immigration into Konku, culminating in the seventeenth century when the first references to the division 'began to appear in the historical material concerning Konku' (32). If so it seems to me that the left. right division historically viewed was a mechanism for incorporating these immigrant, alien castes into the Konku regional and local social structure. Wherefore? The traditional groups revolving round the dominant Kavuntars were, as everywhere, oriented to the *varna* scheme. With the massive entry of immigrant castes the problem of incorporating these immigrant castes, generally artisans of indeterminate status, into the pre-existing operative *varna* scheme of the region would be difficult, yet it has to be done. One way to do this is to employ another well-known Indian classificatory device, the left: right scheme, so that the castes involved in the existent pattern of traditional services are 'right', and the immigrants are the 'left'. Historically therefore the division is immigrant: settler; sociologically the division provides a charter for incorporating immigrant castes, isolating those castes involved in traditional ties with the dominant caste from the outsiders. Since power is with the dominant caste, its side can arrogate for itself the higher, propitious division (right).

The processes of immigration culminating in the formation of the left-right division in Konku in the seventeenth century, probably occurred much earlier in other parts of South India. Stein says that the Vellalars were dominant in many parts of South India from the ninth century (Stein 1969). Thus the system of land and *jajmani* relationships was an ancient and enduring part of South Indian society. Immigrant groups—of all sorts—were pressing on this area, for a variety of social and historical reasons, and the dual division provided a mechanism for incorporating them into the local society *without incorporating them (or most of them) into the* JAJMANI *system*. In other words these patterns of immigration were an early and continuing process in south India.

IV

Viewed in this historical perspective the distinction is not between the Brahmanic orthodox, ritualistic orientation of left and the mundane power interests of the right. In India, the Brahmins and the dominant caste, as Dumont has noted, have always been involved in orthodoxy, and the *varna* scheme (Dumont 1958: 55–8).

The Brahmin head seems to me tilted to the *right*, contra Beck. Left religiosity is oriented differently: *not to the Brahmin but, perhaps, to the* SANYASIN *ideal*.

Left groups, I suggest, are oriented to a non-Vedic religiosity, more in keeping with artisans, and uprooted immigrant groups generally. In this sense, left castes are non-traditional both in terms of service (*jajmani*) relationships as Beck states, and also in religious terms (which she denies). But this perspective fits better the Indian idea of left-ness and enables us to understand why the left side is inferior both sociologically and in terms of religious orthodoxy.

This model of religiosity—the *sanyasin* model of the left—makes sense in relation to the data presented by the author herself.

(a) The religious leaders of the left groups were wandering *gurus*, who travel around for most of the year, unanchored to fixed places of worship (74–7), thus better fitting the *sanyasin* model.

(b) In fact the headmen of left subcastes are called *sanyasins* (*canyacis*), 'whose life style stresses celibacy and dependence on the alms of the devotees' (13). This is not an imitation of Brahmin behaviour as the author thinks (12).

(c) If left groups were textually oriented and orthodox they would emphasise hierarchy, which is a prime orthodox value. But in fact they stress the opposite: 'all devotees are treated as equals at left caste festivals' (16). Such behaviour is heterodox, more in keeping with the *sanyasin*, rather than Brahmanic ideal.

(d) The *sanyasin* is preeminently someone outside the *varna* scheme and the status system: orientation towards this form of religiosity would appeal to immigrants who are not involved in traditional agricultural production. Also the *sanyasin*, like the Buddhist monk, is in touch with pollution, but his religiosity transcends it (Yalman 1962; 1973). Hence Untouchables can be attracted to, and incorporated into, this scheme which is more tolerant of their status. It is interesting to note that the untouchable caste on the *right*—the Paraiyars—are in fact more concerned with purity than the left leatherworkers, a finding more consonant with my interpretation than the author's (168, 170). I use the term '*sanyasin*' advisedly, to refer to various types of heterodox religiosity, in opposition to Brahmanic orthodoxy. Historically the left has probably incorporated the anti-Brahmanic religiosity of the South.

(e) Finally my interpretation better accords with a similar religious division in northern India: the left: right *tantra*, where the left (relatively speaking) represents a more heterodox religiosity than the right. However it is likely that left religiosity is not a product of its 'leftness' *per se* but rather a reflection of the heterodox religiosity of artisans and uprooted peoples.

The author presents data to show clearly that the castes on the two sides have not only opposed religious orientations, but also differ on certain crucial sociological dimensions, *viz*. different marriage rules, descent organisations, residence and so forth. Let us consider the argument on residence.

The author states that since the left groups are more attuned to traditional law books they will be 'more sensitive than their right division counterparts to the patrilocal ideal set forth in the classical law books' . . . (234). They will also be less inclined to uxorilocality which is unclassical, and more towards neolocality which supports the classical position (234–5). In fact the author's statistics in table 5.7 re-analysed do not bear out her own contention. Here we find that 83.3 per cent. of

the unclassical right practise patrilocality as against 66.6 per cent. of the left, which supports the opposite position! (p. 235, table 5.7). The data on uxorilocality are not opaque either because the right wing Natars seem to be an exception having six times the uxorilocality as the two castes who come next, the dominant Kavuntars (right, 6.0 per cent.) and Ácari (left, 6.2 per cent.). It would seem that high uxorilocality among Natars is not paralleled in any caste, left or right, and hence seems to be determined by economic factors specific to that caste of palmyra palm tappers. If we exclude the Natars there seems no significant difference in left/right uxorilocality. Also, the dominant right caste, Kavuntars whose numbers constitute 74 per cent. of the right division are low on uxorilocality. However it is true that neolocality is high among the left groups, but this indicates, contrary to Beck, its *untraditional* nature, for who ever heard of neolocality enjoined in classical law books?

I think the implication is sufficiently clear. The various social structural similarities on each side, are based not on left-ness or right-ness *per se*, but on socio-economic factors. Thus the author says joint living is due to left leanings but adds: 'These general right-left differences in attitude toward joint living are obscured, however by the tendency for all higher ranking communities to have a higher proportion of joint families . . .' (210) i.e. socio-economic variables seem the operative ones. Again: 'right division communities . . . make a distinction between paternal and maternal grandparents', but later she recognises that this may be due to variations in the descent principle (229). It seems to be that many of the sociological features of the two divisions (see pages 70–108) rest on the economic dominance of the Kavuntar, and their relationships with the rest of the right section, and the lack of roots of the left communities in the traditional agrarian economy. Thus the higher rate of literacy of the left is due not so much to their orientation towards classical learning (212, 264) but due to their emancipation from the agricultural process and *jajmani* system, their mobility, and greater orientation towards achieved statuses (76–77).

Perhaps a true test of whether the left: right division indicates a classical orientation or not would be to take those sociological variables which may be partially influenced at least by classical values: attitudes to divorce, sexual morality, widow remarriage. However the author does not present this evidence. My own guess is that these norms would be applied in a more flexible manner in the left side of the division.

V

To sum up, the major methodological weakness is the unhappy alliance between empirical social anthropology and structuralism. The imposition of a structural orientation does produce aesthetic elegance so that the 'whole is like a set of Chinese boxes, each unit having roughly the same shape, while fitting neatly inside the previous container' (208). What is *inside* the boxes constitutes excellent social anthropology, but unhappily the focus on the neat fit of the boxes sometimes tends to obscure historical factors and socio-economic and demographic variables which are to me the significant and operative ones. Nevertheless, the book is a truly fine piece of research, and should be on the shelf of every methodologically minded anthropologist.

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CORRESPONDENCE

Demographic models and female infanticide

SIR,

Schrire and Steiger (*Man* (N.S.) **9**, 161–84) examine the hypothesis that female infanticide was practised by the Eskimo to regulate population size or sex ratio. A stochastic computer simulation model is used to impose varying rates of female infanticide on a small population characterised by specified mortality and fertility. Since all population trajectories appear to be leading towards extinction, the authors conclude that very little systematic female infanticide can be tolerated by hunter-gatherer populations and that the practice would not be an effective population control mechanism.

Apart from the assessment of the effect of mis-aging of adolescents, with which we fully agree, we find ourselves in substantial disagreement with the procedures and conclusions of this article. The outcome of a computer simulation is determined entirely by the assumptions and parameters of the model, which in this case are not entirely realistic. Mortality and fertility functions are selected without adequate evaluation of the extent to which they are representative of real hunter-gatherer populations.

The fertility rates used in the model yield an expected 7·2 live births per female who reaches menopause The authors contend that this is higher than was typical for Eskimos and is conservative in the sense that it makes the outcome of extinction less likely than would a lower fertility function. However, this claim is based on ethnographic sources which give the number of live births or the number of children per woman, including women who had not yet completed their reproductive years. For example, one report which was cited (Rasmussen 1931: 140–1) shows an average of 6·4 live births per woman in a sample of eighteen Netsilik women. If one considers only the eight women estimated to be forty-five or older, the average is 9·0 live births per woman. Hence, Schrire and Steiger may have made a low or accurate approximation of Eskimo fertility rather than the high estimate that they claim In further support of this interpretation, a sample of forty-three Caribou Eskimo women at least fifty years old in 1973 reported an average of 7·6 live births (Acker, field notes); this is a minimum estimate because of the possibility of unreported births.

The fertility function in the model may be criticised for showing maximum fertility rates at ages 27–34, in contrast to real populations, which show peaks in fertility around age 20 or 25 and begin to decline by age 30 (Weiss 1973: 33). Correction of this assumption would tend to increase the growth rate of the assumed population, since more women are alive at these younger ages.

More obviously problematic than the fertility function is the very low infant mortality rate, derived from death certificates in which under-reporting of infant deaths is likely Correcting this low infant mortality rate to better agreement with real hunter-gatherer populations exaggerates the reported outcome of population decrease, a trend which would be only slightly offset by shifting the years of maximum fertility to younger ages. If the simulation model is correct, one would be forced to conclude that very low rates of female infanticide (probably less than 5 per cent.) would lead a population to extinction. This is startling in view of the fact that much higher rates have been observed (Rasmussen 1931: 140–1; Birdsell 1972 355, Townsend 1971: 21).

The inconsistency between this conclusion and ethnographic data led us to attempt to verify the outcome of the published population trajectories with a deterministic model. Fortunately the deterministic model can be solved analytically without computer simulation. It differs from the stochastic model in assuming that the population variable is continuous, i.e., that populations may increase or decrease by fractions of an individual. In the stochastic model, populations change only in integral units (individuals) according to specified probabilities which allow for random fluctuations. The outcome of the deterministic model is the mean or expected outcome of a large number of trials using the stochastic model.

Under these assumptions, one can use the published mortality rates to find a survivorship function. That is, if a cohort of 1000 females was born in any given year and subject to these mortality rates throughout their lives, l(x) of them would survive to age x. One can then multiply the number of females who survive to age x by the probability that a female has an offspring at that age to find the number of children born to mothers aged x. The sum of these over all the reproductive years yields the total number of off-

spring born to a cohort of 1000 females. According to the mortality and fertility rates and fraction of female births that Schrire and Steiger used in their simulation, a cohort originally numbering 1000 females at birth would produce 3821 offspring of which 1855 would be females. This indicates a population which in the absence of infanticide would almost double in each generation. This population could afford to lose 46 per cent. of all female offspring to infanticide and still remain constant in total numbers. We do not mean to imply that real hunter-gatherer populations could sustain so much female infanticide, only that the deterministic analysis of the mortality and fertility functions used in the model yields a result which differs radically from the outcome of the computer simulation. We are forced to conclude that the programme did not run the way it was supposed to run.

An improved procedure would be to test the hypothesis with a deterministic model using several sets of mortality and fertility functions representing the range or variability in known primitive populations. Having made a general statement about the direction and degree of expected population changes, it becomes meaningful to run many trials under the stochastic model to find out to what degree this outcome may vary in small populations

It is not only the population parameters but also other assumptions of the published model which are unrealistic. Mortality, fertility, and infanticide are treated as independent variables. Infanticide is simply aggregated to infant and child mortality from other causes, although the rational practice of infanticide would remove infants who are judged at birth to have a less-than-average probability of surviving. In a further oversimplification with similar implications, fertility is held constant regardless of the rate of infanticide. The authors cite Rasmussen to indicate, 'Since babies had to be breastfed for several years females were often killed at birth in order to save time between successive births and in the hope of producing a male next time,' (p. 161), but this is not incorporated into the model. A one- to three-year reduction of the birth interval following each infanticide would increase total fertility, substantially offsetting the 'genocidal' tendencies of infanticide practised to adjust the sex ratio.

Many of the weaknesses of the published model may be traced to the author's predilection for viewing infanticide as a population policy rather than first of all as a family planning policy, which then has implications for the population as a whole. In order to construct a model based on realistic assumptions it is essential at some stage of the research to consider the Eskimo family making a decision either to raise a child, or give it out for adoption, or allow it to die. This decision is influenced by the spacing between the infant and an older sibling as it affects the life chances of both children and by the desire to assure a son for the support of the parents in their old age and not, more generally, to assure an adequate ratio of males to females in the population.

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SIR,

We are grateful to Townsend and Acker for raising certain issues concerning our article One point is especially important, since it allows us to reiterate some of the methodological issues that apparently have not been adequately handled in the paper

We will deal with each of six separate points raised by our critics:

1 The model uses a 'low or accurate approximation of Eskimo fertility rather than the high estimate . (claimed)'.

We based our estimates upon the impressions of ethnographers, most of whom visited the Arctic more than half a century ago (Schrire & Steiger 1974 180, footnote 4). We preferred to deal with somewhat unsophisticated material that related to relatively unacculturated groups of Arctic hunters, rather than attack a possibly better body of data concerning the highly acculturated Innuit groups of today (cf. Acker, field notes 1973) This strategy enabled us to avoid making allowance for the undoubtedly major effects of modern settlement patterns, diet, welfare and medicine upon modern Innuit demography.

Some of our data, in particular the famous Rasmussen series, are of course questionable Townsend and Acker are perfectly correct to reduce his meager count of eighteen women to a mere eight post-menopausal souls, but the sample, which was always statistically

small, now approaches uselessness. Furthermore, it does not now, and certainly never did, reflect the figure we were really after: namely, the average number of live births for *all* women in a *population* of say, 500 souls. As such it remains a rough, unchecked guide to fertility, providing a figure *far* in excess of the one we are after for our model, and certainly, far in excess of reality.

2. 'The fertility function in the model may be criticized for showing maximum fertility rates at ages 27–34, in contrast to real populations, which show peaks in fertility around age 20 or 25 and begin to decline by age 30' (Weiss 1973: 33).

This point is well taken. We agree that ours is unrealistic and that a right-skewed function that peaks earlier would generate a more vigorously growing population, even for the same average number of offspring per woman. However, a strength implicit in our method is that its validity does not depend on the shape of the fertility curve, but only on the fact that model function exceeds the true function

3 'If the simulation model is correct, one would be forced to conclude that very low rates of female infanticide . would lead a population to extinction This is startling in view of the fact that *much higher rates have been observed*' (our emphasis).

Simulation models are what their names imply· they are neither right nor wrong but simply approximations of a particular situation. It is therefore nonsensical to insist that the model satisfy a condition it is trying to determine, namely, the degree to which systematic female infanticide may be tolerated.

The 'startling' nature of our conclusions are carefully related to the inferences made by Rasmussen and Birdsell: there is no need to reiterate them here. The actual wording of Acker and Townsend's criticism merits one comment: neither Rasmussen nor any of Birdsell's sources *ever* observed the practice of infanticide as a constant feature from which a rate of practice might be inferred. For that matter, they never even witnessed a single case of female infanticide. Our data and theirs are all derived from hearsay or inferred from population figures, and it is crucial that this fact be fully appreciated in order to grasp the difficulties implicit in analysing the practice of female infanticide in the Arctic.

4. '... the deterministic analysis of the mortality and fertility functions used in the model yields a result which differs radically from the outcome of the computer simulation.'

We are grateful for the remarks concerning deterministic growth rate since they drew our attention to the substance in the correction note (see table). At the same time however they reveal misconceptions that force us to reiterate the delicacy needed to compare the deterministic and stochastic evolutions of a population governed by a given pair of rate schedules.

Acker and Townsend have drawn conclusions about stochastic evolutions using the deterministic trajectory. This is not permissible. If the rates are stationary over time, it is not difficult to calculate the expected population size (and even the age distribution) at some given time, T, in the future. This however is not the same as the size of a deterministically changing population at time T. Furthermore, a single trajectory will probably differ markedly from the trajectory of its expected values (one may increase while the other decreases, for example) so that in general, the type of inference drawn by Acker and Townsend would be unsupportable.

5. ' . other assumptions of the published model ... are unrealistic Mortality, fertility, and infanticide are treated as independent variables.'

Acker and Townsend have missed an important thrust of our argument. We naturally expected infanticide rates to vary in response to environmental pressures. Indeed, we devised our target sex ratios with this aim in mind (Schrire and Steiger 1974: 175 sqq.). With this admittedly over-simplified scheme, stresses imposed on the group are reflected through its *actual* sex-ratio. Higher F/M ratios were used to signify conditions under which male hunters would experience difficulty in feeding a larger body of female consumers. In seeking a target ratio through an infanticide rate that varies in response to fluctuations within the population, the net mortality is non-stationary. This leads us to the more interesting situation in which simulation is the only general method to reveal the evolution, be it deterministic or stochastic, of the population. Acker and Townsend's recommended 'improved procedure' does not apply to the really challenging case, and as such is not interesting to us.

6. 'Most of the weaknesses of the published model may be traced to the author's predilection for viewing infanticide as a population policy rather than first of all as a family planning policy, which then has implications for the population as a whole.'

Obviously infanticide is usually decided on an individual family basis, and not according to dictates that are articulated by the group as a whole. Nevertheless, it may be that the policy can be better understood from the global viewpoint in which families are aggre-

Correction. TABLE 3. Age specific mortality function.
(Age i means at least i but younger than i + 1)

Age	Conditional probability of dying		Age	Conditional probability of dying		Age	Conditional probability of dying	
	Male	Female		Male	Female		Male	Female
0	0·250	0·241	24	0·016	0·015	48	0·024	0·023
1	0·180	0·173	25	0·016	0·015	49	0·025	0·024
2	0·128	0·122	26	0·016	0·015	50	0·026	0·025
3	0·083	p·080	27	0·016	0·015	51	0·027	0·026
4	0·042	0·040	28	0·017	0·016	52	0·028	0·027
5	0·010	0·010	29	0·017	0·016	53	0·030	0·029
6	0·009	0·009	30	0·017	0·016	54	0·032	0·031
7	0·008	0·008	31	0·018	0·017	55	0·034	0·033
8	0·008	0·008	32	0 018	0·017	56	0·035	0·034
9	0·007	0·007	33	0 018	0·017	57	0·037	0·036
10	0·007	0·007	34	0·019	0·018	58	0·040	0·038
11	0·008	0·008	35	0·019	0·018	59	0·042	0·040
12	0·008	0·008	36	0·019	0·018	60	0·044	0·042
13	0·009	0·009	37	0·019	0·018	61	0·046	0·044
14	0·010	0·010	38	0 020	0·019	62	0·048	0·046
15	0·011	0·011	39	0 020	0·019	63	0·050	0·048
16	0·011	0·011	40	0·020	0·019	64	0·052	0·050
17	0·012	0·012	41	0·021	0·020	65	0·054	0·052
18	0·012	0·012	42	0·021	0·020	66	0·060	0·058
19	0·014	0 013	43	0·021	0·020	67	0·067	0·064
20	0·014	0·013	44	0·022	0·021	68	0·074	0·071
21	0·015	0·014	45	0·022	0·021	69	0·081	0·078
22	0·015	0·014	46	0·023	0·022			
23	0·015	0·014	47	0·023	0·022			

gated into one unit. The relationships between the group infanticide rate and other ecological variables may then be studied even though they may be totally obscured on the family level. Nevertheless, the issue is not important to us as it is unrelated to the problem with which we dealt in the article.

What is important along these lines however is the intrinsic nature of the data at hand. The ethnographic sources supply no individual family histories so that in any case our study was forced to adopt the overall population viewpoint

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William Lee Steiger

Rutgers University
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Elementary and complex structures

SIR,

In his article on 'The debt of the maternal uncle' in *Man* (N.S.) **9**, 609–19, Luc de Heusch says (p. 618) that an Omaha system of kin terms is characterised 'by the adoption of a unique term to refer to the totality of the members of the same sex of the lineage of the maternal uncle' He says further that, 'It is, in such a system, as if at each marriage alliance, time became fixed precisely in order to avoid it being repeated. This phenomenon is a sign of the passage from an elementary structure of kinship to a complex structure.' The idea that the lineal features of Crow-Omaha terminologies prevent the repetition of marriage alliance derived from Lévi-Strauss (1966: 19) and has been discussed in *Man* by McKinley under the phrase 'dispersed affinal alliance' (*Man* (N.S.) **6**, 228–47; **6**, 408–26)

Two points should be made in response to de Heusch. The first of these is that the Omaha, themselves, do not, in fact, adopt 'a unique term to refer to the totality of the members of the same sex of the lineage of the maternal uncle.' The members of this lineage are either *itiga* (e.g., MFF, MF) or *inegi* (e.g., MB, MBSS). That is, two terms are used for this line, and from this fact it follows that it is not exactly correct to say that the terminology ignores the passing of generations.

Secondly, if we take as crucial the fact that MB and MBS and other members of the line

are called by the same term, then we must conclude that it provides no evidence of structural change of any kind. The terminologies of the Gilyak, the Purum and the Kédang all have this feature. They must be reckoned to have elementary structures in Lévi-Strauss's terms, positive marriage rules, and, at the least, no prohibition on repeated marriages. Furthermore, the feature presents no obstacle, either in principle or in practice, to, for example, marriage with the MBD. If societies with elementary structures already make lineal equations in the mother's line, how can this feature be taken to indicate transition to a complex system? In the original lecture, Lévi-Strauss also refers to the lack of equations between males of mother's line and those of wife's line. This is, indeed, a crucial difference between asymmetric prescriptive and Crow-Omaha terminologies, but it was not to this feature that de Heusch was referring. If we concede though that the argument about lineal equations is only meant to apply when this second feature is present, then we are faced with the apparent paradox that these lineal equations serve one purpose in an elementary system and precisely the opposite purpose in a Crow-Omaha system. Even if this were possible (functions do change), the equations would not in themselves indicate a transition from one form to the other.

It is also questionable whether in these societies there is any sense in saying that time has been fixed or frozen by the relevant term At the most, one might say that no definite reference to time is made. On the other hand, it might be argued that lineal equations neither ignore time nor fix it, but that they make (or imply) a positive statement about the relation between past and future. I doubt that this statement says that time may be reversed, but this question should perhaps be left to resolution by the study of the representation of time in particular ethnographic cases.

R. H Barnes

University of Edinburgh

Heusch, Luc de 1974 The debt of the maternal uncle contribution to the study of complex structures of kinship. *Man* (N.S.) **9**, 609–19.

Lévi-Strauss, Claude 1966 The future of kinship studies. *Proc. R. anthrop. Inst. 1965*, 13–22.

McKinley, Robert 1971*a* A critique of the reflectionist theory of kinship terminology: the Crow/Omaha case. *Man* (N.S) **6**, 228–47.

——— 1971*b*. Why do Crow and Omaha kinship terminologies exist? A sociology of knowlege interpretation. *Man* (N.S.) **6**, 408–26.

SIR,

Dr Barnes criticises a point (secondary albeit) of my argument. I grant that my speedy definition of Omaha systems is incomplete. I should have specified that the time (of alliance) is fixed by the terminology starting from MB. Often in fact MF is designated by a term different from that used for MB, MBS, MBSS, but it can happen that the totality of males of the lineage be assimilated to MF, as is precisely the case with the Thonga.

I am well aware that the presence together of MB, MBS, MBSS in the same terminological class is not sufficient to define an Omaha system or to mark the transition to complex structures. I realise that the elementary structures are capable of using classifications of this kind. In order that one might speak of Omaha systems it is necessary that the women of the maternal lineage, starting with the mother, are assimilated to mothers, thus marking the impossibility of repeating the marriage alliance. I hope to be able to return in greater detail to the emergence of Omaha systems among the Bantu people in South Africa in order to indicate more precisely how it constitutes a remarkable indication of transition from elementary to complex structures when found together with the privilege of the nephew and, of course, the interdiction on marriage with the true matrilateral cross-cousin.

L. de Heusch

École Pratique des Hautes Études

Radcliffe-Brown and Daisy Bates

SIR,

Since my biography of Daisy Bates has been quoted in the correspondence between Dr Rodney Needham and Professor Scheffler, may I add some details to the claim made by Daisy Bates that her manuscript had been 'mutilated beyond repair' by Radcliffe-Brown.

Before I do so, I should like to say how delighted I am that Professor Scheffler is engaged in the long overdue task of editing the Bates manuscripts held by the National Library in Canberra. I am sure there are difficulties, especially in view of the general attitude of scepticism towards her which I found to be prevalent among the older generation of anthropologists in Australia. As Professor Scheffler says, she felt ill-treated by the Australian anthropological community in her li[illegible]e and so would be gratified to discover th[illegible]ect accorded to her by the younger gen[illegible]who, I found, were ready to back up so[illegible] the most controversial of her statements[illegible]n fact, as her biographer, it was difficult to avoid the suspicion that much of the prejudice from which she suffered—

though due in part to her later eccentricities—stemmed from her contemptuous disregard of the 'father' of Australian anthropology, Radcliffe-Brown.

To get at the root cause of the antagonism between them I found that the Canberra Collection was not enough. The story of those early years between 1904 and 1910, when some of her most important work was done, is recorded in the letters to John Mathew, sent me by his son, and, in the Government files kept in the Battye library of Perth in Western Australia. Here can be found the detail of work done and reported to the Government Statistician, Malcolm Fraser, the diary of her extensive travels throughout the state, and, especially significant, letters between the Government and the visiting young anthropologist known then as Mr A R Brown.

Here I found a copy of the first long letter she received after Radcliffe-Brown had been sent her chapter on social organisation by Andrew Lang, and written to commend her on the 'valuable' material she had collected. Since Lang's letter to Brown is part of the Canberra collection, Professor Scheffler may have seen his advice that 'a red pencil would be needed.' It is surely unnecessary to point out that a red pencil is used by an editor in cutting his material Professor Scheffler also concedes that, in the first weeks after his arrival in Perth, Brown may have worked with Daisy on her manuscript In these files he would see the letter in which she asks that an office be made available to her for the purpose, reporting to Mathew that she had found 'Mr Brown' a 'keen enquirer.' He would also find a letter written by Brown to Mr North of the Colonial office, dated April 19th, 1911 and containing the following paragraph

'a great deal of the work of revision has been completed but some further work is necessary on the material relating to the South West District and the Murchison and Eastern Goldfields. When the revision is completed the material collected for the Government by Mrs Bates will be in a suitable form for publication.' And, a few weeks later, on 6 May 1911, on writing to thank the Government 'for the kind extension of the period for which the services of Mrs Bates are placed at my disposal' he finished with the promise that 'Mrs Bates' work will be published by me free of cost to the Government in such form as is deemed most suitable by the Cambridge Committee for the Ethnographical Survey of Australia, under whose direction I am acting.'

But, by the time Brown left for England they had quarrelled and, Daisy reported to John Mathew, 'the late Government handed over the only copy I had to Brown and that was mutilated beyond recovery. The present Government has relieved Brown of the responsibility of publishing the book, returns it to me, but will not pay for its publication ... the mutilated portions are useless.'

Surely the above quotes tell their own story? Radcliffe-Brown, encouraged initially by Daisy herself, worked on her manuscript, revising it, cutting it and generally preparing it for publication by the Cambridge Committee for the Ethnographical Survey of Australia, 'under whose direction he was acting.' The result was the 'mutilated' manuscript handed back to her before his departure

Were the portions cut out the ones he intended to use, or to discard? There is no evidence with which to answer the question But surely the correspondence quoted lends its own validity to Daisy's charge of plagiarism? Since Radcliffe-Brown was prepared, by his own stated intention, to have her work included in his report, and since that work, again according to his own statement, was so nearly concluded, would it have been possible for him to have eliminated it entirely from his published account? Even Professor Scheffler admits that some of their material dealt with the same groups but hastens to add that the difference in their accounts shows that Brown had 'got his own information' I am sure there is no doubt that Radcliffe-Brown, a trained anthropologist, improved on the work of Daisy Bates. But did this fact absolve him from the responsibility of acknowledging it? In her own two accusations of plagiarism, Daisy Bates was quite specific. Brown's report included the legend of the Ngargalulu spirit babies A letter written to John Mathew before the advent of Radcliffe-Brown evidences Daisy's enthusiasm for this legend, first encountered during her visit to Broome in 1902. It comprised one section of her chapter on Aboriginal myths read by Radcliffe-Brown on his arrival in Perth.

The Canberra collection of the work of Daisy Bates (prepared, by the way, in Adelaide and not in Melbourne) is an impressive record of her life work. The Government files in the Perth library provide behind-the-scene insights which, to her biographer, were an illumination.

Elizabeth Salter

London

Functions, contingent and universal

Sir,

In a recent article (*Man* (N.S.) **9**, 423–35), P. G. Rivière comprehensively examined the social practice of the couvade. He successfully, I think, showed that it can be viewed as one of the many ways of dealing with the dualism of man, i.e., that man perceives his person to be composed of a body and a soul. Dr Rivière's article is a major contribution to

anthropological theory, but I see no reason why we must accept his contention that the couvade cannot, at the same time, have other functions of equal importance to cosmological ideas. Rivière says, 'it is not altogether unlikely that the couvade is a means by which a man lays claim to being a child's pater. However, this function can never be more than a contingent aspect of the phenomenon' (Rivière 1974: 433). Why is this so? Why cannot an institution have more than one cause? Is it merely that functionalism is not presently popular in anthropological circles?

Rivière shows that at birth the parents in societies with beliefs about the necessity of human action in the creation of a child's soul are faced with a 'second' creation, in addition to their initial efforts. He links the couvade to cosmological beliefs. The ritual practice would seem to vary with a variance in beliefs about: the nature of the soul substance; when the soul and the body are united; and the role of the parents and kinsmen in the process of soul-creation.

In the ethnographic literature there has been an unfortunate dichotomy between those writers who describe institutions without reference to cosmology and those who make the opposite error. In the past British ethnography has reflected the former tendency and Rivière has attempted to correct this imbalance. He has rightly shown that there is a significant correlation between cosmology and the social practice of the couvade as well as the institution of *compadrazgo*. He suggests that other institutions may also be involved with ideas about soul-creation. I wish to show briefly that his pregnant ideas about the couvade may be considered fertile when applied to the ethnography of a west African people.

The Sisala are a patrilineal group of horticulturists numbering about seventy-five thousand. They live in the Tumu District, Upper Region, northern Ghana. Among the Sisala children are greatly valued and the birth rate is high since without offspring a married couple is never considered mature. They cannot become ancestors without offspring to perform the necessary rites of transition for them. Since the death rate is also high there is pressure on having large families because an ancestor's spirit will disintegrate if his offspring expire and there is no one left to propitiate the ancestor at the family shrines. Consequently the birth process is very important in Sisala belief and practice.

The Sisala have no couvade as such, but they do have ritual post-partum practices which serve to enable the parents and kinsmen to participate in the creation of a child's soul. These practices are some of the various ways devised by man to deal with the conceptualised dualism of the person. Rather than being a form of the couvade, both the couvade and these practices briefly described below, are part of a more general phenomenon: soul-creating rituals.

The Sisala believe that the body is created by the act of sex. It grows inside the body of the mother until it is born, at which time it is not yet considered a human being. This is so because it has no soul. Three important rites must be performed by the father before the child becomes a full person. It is significant that if the child dies before these rites are performed, the body is merely thrown on the trash heap or into the bush and no burial or funeral takes place. After these rites are performed the child is thought to have a body and a soul and is, consequently, a true human being. As such, a proper funeral must be performed in order to convert the soul (*dima*), which becomes a wandering ghost (*nedima*) at death, into an ancestor (*lelung*).

The first rite is the obtaining of the *kantong luri* (fairy medicine) by the pater from his affines. Upon the birth of his child the father sets off to the compound of his wife's father to secure a container of fairy medicine which is required to wash the body of the child. In the second rite the father must approach a blacksmith to get him to make a special bracelet (*nadima*) which represents the child's soul. This is placed on the child's right wrist and the father builds a small *dima* shrine for his child in the family compound. This is the most important personal shrine for an individual during his lifetime. In the final rite the father must perform an elaborate naming ceremony for the child. This completes the initial social creation of the child's soul, although the soul is a fragile thing and may require further social acts by the child's kinsmen during his lifetime. These rites take place at his soul-shrine (*dima*).

Once these rites are performed the child is a fully-fledged member of the lineage. He is entitled to a proper burial and funeral. Upon his death his *nadima* bracelet is removed from his wrist and placed on the lineage ancestor shrine, which is a pile of such bracelets of the deceased members of the lineage.

These three rites point to the fact that the creation of the soul is a social event. The pater is the primary actor in the necessary ritual activities. Moreover, the child's matrilateral kin play a role by providing the fairy medicine and the patrilineal kin play a central and continuing part in creating and maintaining the delicate bond between body and soul. At any time during the child's lifetime an untoward event may cause a temporary separation of body and soul. When this happens it is incumbent upon the patri-kin to perform the restorative rites.

Only a brief sketch of these rites can be given here. Suffice it to say that they are integral to Sisala cosmological beliefs, e.g., a belief in a reservoir of soul-matter used to create and recreate souls; a belief in a volatile, unstable soul; a belief in the necessity of a 'secondary birth', or rituals of soul-creation; a belief in the soul as the energy-force of the body; a belief in the relationship between a person's soul and his name; and various beliefs about the nature and fate of the soul after death.

Rivière has added to our understanding of the relationship between cosmological ideas and social practices; the former provide the 'why' of the practices and in social reality one rarely occurs without the other. Practices like the varying forms of the couvade, post-partum applications of medicines, the creation of charms, the naming of children, exist in a correlative relation to ideas about the nature of the composition of the person.

Parents everywhere are faced with the creation of a new human being through the birth process. However, where there exists the necessity of a 'secondary creation' of the child's soul by human agents, soul-creation rituals will be found. Rather than studying these rites as a unique phenomenon, as has been done with the couvade, we should attempt to see them as a more general effort to bring action into congruity with beliefs about the dual nature of the person.

In conclusion I wish to add that rites that establish paternity and rites that are thought to create the soul may empirically be the same rites. Both are ritual statements directed to a public; the community. The creation of a child's body is a private matter. Only the woman knows who the genitor is, and sometimes even she may be unsure. There is a need for a social announcement. Soul-creating rites easily serve this need because it is normally the pater who is required to carry out these rites Like the couvade, Sisala rituals of soul-creation are public announcements about paternity and institutionalised practices linked to their beliefs about the soul. While two people can create a body by an act of sex, it takes a community to create a social persona. Both the couvade and Sisala soul-creating rites are public ceremonies and, as such, are public announcements. When the pater walks in ritual silence to his affines' compound to get the fairy medicine, he is making a public statement. When the pater creates his child's soul-shrine, he does so under the observing eye of the community. When the naming ceremony is held, the pater's affines and kinsmen gather around to participate. Thus, Private sex : Public rites : : Body-creation : Soul-creation : : Genitor : Pater : : Natural : Spiritual . : Parents : Community, and what Dr Rivière means by the contingency of any given cause should be clarified. What are the criteria of the universality or contingency of a cause?

Eugene L. Mendonsa
Ahmadu Bello University

Rivière, P. G. 1974. The couvade: A Problem Reborn. *Man* (N.S.) 9, 423–35.

Dr Mendonsa's direct invitation to me to clarify what I mean by the contingency of any given cause I hope will be accepted by readers as adequate justification for writing to myself.

The statement that Dr Mendonsa queries is: 'it is not altogether unlikely that the couvade is a means by which a man lays claim to being a child's pater. However, this function can never be more than a contingent aspect of the phenomenon'. My answer is that 'claim to paternity' is not a universal feature or function of the couvade, as an examination of the relevant ethnography will readily reveal. Accordingly it is a contingent aspect of the phenomenon in the following two senses of the word: (1) 'not determined by necessity; free', (2) 'true only under existing conditions; that exists in dependence on something else' (S O.E.D).

In the first sense the relationship between an institution and its function(s) is contingent because either can change independently of the other. The Sisala material provides a perfect example of the second sense: it can readily be seen that the pater-identifying function of the rites described is contingent on (exists in dependence on) Sisala cosmology and social organisation

Peter Rivière
Oxford University

Bibliographies of E. E. Evans-Pritchard and W. Robertson Smith

Sir,

The demands of inclusive bibliography are difficult when this involves compilation of the works of scholars as productive and far-ranging as Evans-Pritchard and Robertson Smith. Recently I published what were then hoped to be definitive bibliographies of both men (1974a, 1974b). Unfortunately both already require revision

The bibliography of Smith, though the most reliable and complete to date, doubtless still suffers from minor omissions since apparently there was no list kept by Smith of his writings. Smith was inclined to respond to any and all requests for information, and, as a result, his letters appear in a wide range of journals and newspapers and snippets from his advisory correspondence crop up in the scholarly works of others. Below is a further reference of this latter sort:

1891 Two excerpts from a letter of 11 August, 1888, pp. 113, 120, in John G. Bourke, *Scatalogic rites of all nations*. Washington: W. H. Lowdermilk.

My bibliography of Evans-Pritchard was begun with his encouragement and help; he himself insisted that it go to the printer before September, 1973, and suggested that any subsequent writings could be listed posthumously elsewhere. Evans-Pritchard had urged this September date so that he might see the bibliographical pamphlet in print before he died; unhappily this was not possible. Furthermore, it was not clear after his death what further items might appear since it was not his practice to inform me of all writings he had submitted for publication but rather to send me notice of such work either in the form of duplicates of printer's proofs or as offprints. Finally, in the case of one bibliographical entry, his foreword to my own study of Robertson Smith's great book, a change in publisher's policy led to that study being published as a separate critical booklet on Smith rather than as an introduction to a new edition of Smith's classic work on religion, as initially listed in the published Evans-Pritchard bibliography. All this has unavoidably led to confusion. Below are the corrigendum and addenda to the Evans-Pritchard bibliography using the listings employed in that booklet. I intend no further additions to either bibliography.

Corrigendum

1974*b* Foreword, pp. vii–viii to *William Robertson Smith and the sociology of religion*, by T. O Beidelman. Chicago University of Chicago Press.

Addenda (new works)

1973 Some Reflections on Fieldwork in the Twenties. *Anthropological Quarterly*, **46**, No. 4: 235–42.

1974 Four Zande Texts. *Bulletin of the School of Oriental and African Studies*, **37**, Part 1: 41–51.

Addenda (reprints)

1933*a* Reprinted *Journal of the Anthropological Society of Oxford*, **4**, No. 3: 123–42.

1933*a* Reprinted, Bobbs-Merrill A-486.

1936*e* Reprinted *Journal of the Anthropological Society of Oxford*, **5**, No. 1: 1–21.

1940*k* Reprinted extracts from Introduction, pp. 10–14, in Elizabeth Hegeman and Leonard Kooperman (eds.): *Anthropology and community action*. New York: Anchor paperback, 1974.

1956*g* Scattered extracts, pp. 53–99, in *Anthropology of folk religion* (ed. C. Leslie), Vintage paperback, New York, 1960.

Bibliography of the Writings of E. E. Evans-Pritchard. London: Tavistock Publications, 1974; *William Robertson Smith and the Sociology of Religion*. Chicago: University of Chicago Press, 1974.

T. O. Beidelman

New York University

In search of meaning in African art

SIR,

My friend, Mr John Donne, has left my hobby-horse well up in the air in his review of *Primitive art and society* (Man (N.S.) **10**, 163). Will he not administer the *coup de grâce*, if he can?

In selecting for comment the final quarter (nearly all a quotation from my *Nigerian images*, 1963, long out of print) of my article 'In search of meaning in African art', he at first raised my hopes of some brief discussion, but then dashed them again by resorting to unsupported rejection. May I quote a short passage from the end of the paper which I wonder whether he has sufficiently considered?

'By way of prophylaxis [against the growth into a dogma of a hypothesis about the meaning of exponential curves], I have always tried to collect against the grain, seeking facts and specimens above all which appear to conflict with my hypothesis, and begging others to do likewise. But alas, none of the objections which I have thought that I had found have survived closer scrutiny, and as to others, they have either tended to give more or less mild approval, or have confined their criticisms to a generalised amusement. To these last, especially, I would like to say sincerely that I am anxious to test my hypothesis, or have it tested by others to destruction, in the confidence that something better will follow.'

I would not like to be thought a contester of reviews, and all that I want in this case is just a few words more beyond 'generalised amusement'. My puzzlement is all the greater because I am not conscious of having applied (or, therefore, misapplied) D'Arcy Thompson's work, which I would not dare to do, arguing only that he brings much needed enlightenment to the urban student unfamiliar with nature as am I.

I am quite prepared to be parted from my hypothesis, but only by argument. I am hoping against hope, therefore, that Mr Donne will produce the rational basis for his opinion and let me sleep easily one way or the other.

William Fagg

London

SIR,

I should be most distressed if I thought that I had caused Mr Fagg any loss of sleep on account of the two, too brief sentences to which I limited myself in my account of his contribution to a volume containing fourteen papers on very diverse subjects. Mr Fagg has been propounding theories regarding the concept of increase expressed as exponential curves in African art for twenty years or more, and yet never, so far as I am aware, have they ever been seriously discussed in print, though they are often referred to in seminars and in conversation. I should be grateful, therefore, for an opportunity to state my own thoughts on the subject, in the hope that this will lead to a wider debate.

The first reference to the exponential curve and African art that I have come across appeared in the *Allen Memorial Art Museum Bulletin* in 1956 (Fagg 1956: 44–61). This was later reprinted in a slightly abridged form (1960: 458–73), but retained the following passage which is worth quoting at length:

> I believe that the morphology of African sculpture may be usefully studied from a quite different point of view [Fagg had just been discussing Wingert's theories concerning African art and human anatomy], namely by reference to mathematics. I do not mean to suggest for a moment that the carver is interested in mathematics as such; but he seems to apprehend certain facts of nature which we define mathematically. The most important of these is perhaps the curve of growth, which we call the exponential or logarithmic or equiangular curve. Curves of this kind such as the horns of rams and of antelopes, the tusks of elephants and the shells of snails, are among the most obvious manifestations of growth or increase in the African world, and that their significance is not lost upon the priests and artists is made perfectly plain by the constant use made of these excrescences in art, either by actual incorporation (as when a horn of a buffalo or of a duiker antelope is inserted in the head of a Basonge figure) or by carved representation, or again by the use of a horn or a tusk as the raw material of a carving (Fagg 1956: 59; 1960: 471).

There follows a reference to Sir D'Arcy Thompson's great work *On growth and form* (1942), which, Mr Fagg states, 'is indispensable to anyone trying to pursue this line of study'. Already I begin to follow Mr Fagg with uneasiness, because I wonder whether there is value in pursuing this line of study, or whether it may just lead to a dead end. He puts his case baldly and succinctly in 1958 thus:

> Though mathematics and its concomitant philosophical concepts play no part in the making of African sculpture, they may yet be valuable aids in the abstraction from it for the better understanding of Europeans of the mysterious elements of African dynamism (Elisofon & Fagg 1958: 25).

Perhaps at this point I may be excused further quotations which would of necessity be lengthy, and refer the interested reader who wishes to pursue the matter more deeply to the following: Fagg 1961: 414–24 (with particular regard to pp. 420–1, 'Content or subject in African art', which did not appear when the article was first published as Fagg 1953); Fagg 1963: 122–3 (quoted, with a slight omission, in Fagg 1973: 163–7); and Thompson 1942 (especially Chs. 11 and 13).

My own reflections on Mr Fagg's hypothesis are as follows. Firstly, D'Arcy Thompson was concerned with the study of organic form, which he expressed in mathematical terms. For example, he shows how certain types of animal horn grow in a logarithmic spiral (first understood mathematically by Descartes), in which the radius increases by geometric progression, as expressed by the equation:

$$r = a\theta$$

where r is the radius, a is a constant, and θ is the total angle through which the radius has passed (Thompson 1942: 752–5). If one now turns to the illustrations which Mr Fagg offers as examples of the exponential curve in African art (1973: 158–9, 162), it appears (though I dislike judging from photographs) that we are in fact being shown curvilinear forms which could not be expressed in exponential terms. Indeed, if Mr Fagg had ever written to the effect that 'After reading D'Arcy Thompson's work it is interesting to note how the African artist continually diverges from the natural growth curve and the logarithmic spiral and produces curvilinear forms which are much more personal', I might well have gone along with him, but I would still have wondered why it was worth being distracted by reference to D'Arcy Thompson.

Secondly, Mr Fagg interprets what I prefer to call 'curvilinear forms' as manifestations of growth and increase. This may well be true in certain cases, but I do not believe that it can be accepted as valid for much west African art. For example, the Mama mask which Mr Fagg illustrates (1973: 158) is a Nigerian extension of a morphological type found throughout west Africa (but not in the Congo), denominated by our mutual friend, Professor Roy Sieber, 'the tripartite mask'. Indeed, it does consist basically of three parts: usually a helmet mask, a monstrous jaw, and backward-sweeping horns. It is best known in the form of the Senufo *poniugo* mask, commonly called 'the fire-spitter'. The distri-

bution of this type of mask extends as far north-west as the Baga of Guinea (*viz.* the *banda* mask of the Simo society) and as far south-west as the Kalabari Ijo in the Niger Delta. These horned masks are often of deliberately frightening aspect and their function is usually to terrify. In the case of the *poniugo* mask, one of its purposes is to scare away evil spirits. Among the Bambara, there is a particular type of mask of the *komo* society, similar to the Senufo *poniugo* mask, to whose monstrous jaws are attached real antelope horns pointing forward like menacing tusks. Again, the purpose is to instil fear in the neophytes. Rather than being concerned with growth or increase, many of these horned masks appear to imply 'brute force' by incorporating as many dangerous, powerful or fearsome attributes from the animal kingdom as they can, often arranging them in such a way as to form a humanised face.

Among the same peoples, those masks which are employed in performances associated with increase or fertility, are usually basically human and overtly female. Among the Baga, the *nimba* mask, which appears notably at the onset of the rice harvest, consists, in its largest form, literally of a huge female bust which is supported on the head of a man who can just see out through peep-holes set between the vast, lactiferous breasts. Among the Senufo, the *gpelihe* mask, whose functions include performing before boys during their instruction and initiation into the *poro* society as well as providing public entertainment, represents a beautiful woman, often with her hair done up in the shape of a hen—itself a symbol of fertility. Although these masks sometimes bear a pair of apparent horns, the latter have been interpreted as indicating a particular hairstyle worn by young women after undergoing excision (Himmelheber 1965), and thereby prepared for childbearing.

To sum up, I would argue that, in west African art at least, there exists a widespread variety of curvilinear styles which develop out of, and away from, and perhaps even deliberately avoid, those forms in nature which we can express in mathematical terms. Secondly, that those objects which incorporate multiple animal features, such as horns, tusks, elephants' ears, leopards' spots, snakes, etc., more often represent strength, power, ferocity, than increase or growth. Thirdly, that increase (I much prefer Mr Fagg's term with its wide range of meaning to the use of 'fertility' with its mainly sexual connotations) is associated most often with female, and generally human, features, sometimes combined with procreative or sexual symbols such as the hen and the cowrie shell. And finally that, since Mr Fagg himself has stated that 'mathematics and its concomitant philosophical concepts play no part in the making of African sculpture' (*v. supra*), discussion of the exponential curve in African art—whether this occurs or not—will distract us from an understanding of the underlying African meaning by the very fact of introducing European lines of thought.

Mr Fagg's hypothesis deserves consideration at much greater length, and by others than myself, but I hope that for the time being this will enable him to sleep soundly, undisturbed by dreams of Cartesian mathematics.

J. B. Donne

London

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Correction

SIR,

In my article 'Bedyal pottery: a painted ware made in Iraqui Kurdistan', published in *Man*, Vol. 47, 1947, I gave on the top line of p. 48 the identification of the large *butm* tree as the Terebinth tree, *Pistacia Terebinthus* Linné. While the name Terebinth seems to be a collective botanic name for the whole genus, the specific name I gave was incorrect. I have just now obtained a revised name for the particular species in question from the authorities of Kew Gardens as *Pistacia atlantica* Desf, a synonym of which is *P. mutica* F. & M.

W. A. Macfadyen

Tenterden, Kent

C. S. Wake: biographical materials

SIR,

I should like to report the location of certain unpublished materials concerning the life of Charles Staniland Wake (1835–1910), the first Director (1871–72) of the [Royal] Anthropological Institute.

A biographical record of Wake has recently been published in *Studies in social anthropology: essays in memory of E. E. Evans-Pritchard*, ed. J. H. M. Beattie and R. G. Lienhardt (Oxford: Clarendon Press, 1975), pp. 354–87. The extensive correspondence on which this study was largely based, comprising 200 items, has been bound under the title 'Wake' and has been placed in the Western MSS. collections of the Bodleian Library, Oxford. These materials are not listed under Wake's name in the main catalogue but are included in the manuscripts catalogue, in Duke Humfrey's Library, under the shelfmark: MS. Eng. hist. d. 420

Rodney Needham

Merton College, Oxford

BOOK REVIEWS

Physical Anthropology

DYER, K. F. *The biology of racial integration*. xiii, 446 pp., illus, maps, tables, bibliogr. Bristol: Scientechnica, 1974. £12·50

Race crossing in man, to judge from the few major works devoted to it, is a topic about which there is most talk and least fact. Yet in the literature there is indeed a large number of relevant papers written on particular topics or particular populations, ranging in size up to the great monographs of Fischer on the Rehoboth and Rodenwaldt on the Kisarese. Dyer's book is therefore timely, for in it he draws together much of what has been constructively written on race mixture.

The difficulty of definition of the word 'race' is made clear in chapter 1, pointing out the incompatibility of the older, more rigid typological entity with the modern concept emphasising genetic variability maintained in dynamic equilibrium states. It stresses first the transitory nature of the biological entity, so that any racial classification is essentially an attempt to describe something of the ongoing evolution of the human species, and secondly the difference of the biological definition from that frequently encountered in social, political, and other cultural contexts, for the latter do not depend on the precise identification of the biological differences between the groups concerned.

Chapter 2 deals with the nature and distribution of human genetic variation. Here, since an essential section concerns the processes by which gene frequency differences are brought about and maintained, and since their theory is primarily elaborated in respect of simple diallelic systems, one might have expected rather more attention to single gene characters in man and the intrinsic mechanisms of maintenance of neutral and simple equilibria before delving into the intricacies of polymorphism. Much of the discussion here is in fact of continuously varying and epigenetic characters, and the chapter ends with a discussion of the amount of genetic difference between major geographical races.

Chapter 3 examines the prehistoric evidence on racial origins, attempts to relate it to the known present distribution of man—and succeeds in showing that 'how old are the races of man?' is a meaningless question. Their genetic constitutions are assigned in part to early differentiation of continental groups but with everywhere more recent infusions of genes of modern man, who during his rapid spread throughout the world in the last 100,000 or so years incorporated the greater part of the pre-existing gene pools on the way. Again perhaps here one would have hoped to find some statement of our totally inadequate knowledge of the rates of gene frequency change, or indeed measurement of the processes by which these are brought about. But though the epigrammatic conclusion that 'the history of man is the history of racial integration' is self-contradictory, it is abundantly clear that gene flow between human populations has been a major feature of man's past, characterised as it was by recurrent periods of migration and outbreeding and periods of stability.

The heart of the book comes in chapters 4–6 Here the biology of race crossing is explored, the genetic basis of heterosis is explained, and the evidence for its occurrence in man reviewed. After an exploration of the formal genetics of race mixture, the evidence on biological characters in human hybrid samples is summarised, primarily in terms of physique, superficial characteristics, intelligence, and such characters as fertility and mortality and morbidity, and this is followed by a more detailed discussion of particular hybrid populations.

The final chapter discusses future trends in race crossing, where all the evidence points towards continuing increase in the absolute and relative size of hybrid populations. The conclusion is that the effect of race-crossing in man is quite compatible with that predicted from the theory of population genetics, and that the genetic difference between races is not sufficiently great to produce any gross biological maladjustment in hybrids, and indeed that hybridisation has no harmful biological effects in the long or short term on the resulting populations. But the long term consequences may be of considerable evolutionary significance, for hybridisation increases the amount of genetic variability released into an enlarged gene pool, and the rearrangement of genetic variability through the species alters the potential for rapid evolution in subsequent generations and thus is a stimulus to further evolution.

This book is a thoroughly useful review and in conception deserves to become a classic. In reality, it falls a little short of excellence in that it would be better pruned of some of the

matter that is extraneous to the argument, and there are too many errors of proof reading (e.g. pp 53, 115, 131, 138). But when one considers the amount of confusing and confused information that exists in the literature one can only congratulate the author on his critical and balanced appraisal of so much of it, his careful documentation, and skilful presentation.

D. F. ROBERTS

University of Newcastle upon Tyne

COMAS, JUAN. *Antropología de los pueblos iberoamericanos* (Bibl. univ. Labor 38). 223 pp., illus., plates, tables, bibliogr. Barcelona: Editorial Labor, 1974

When Professor Comas speaks we listen, when he speaks about his own continent we do so with special care. His latest discourse is about the Amerinds who live in the lands from Mexico to Tierra del Fuego, though he extends its range somewhat in its last part.

Three-quarters of his text is a summary by an informed critic of all that is known or has been asserted about the peopling of the Americas and the present metric and genetic characters of their indigenous inhabitants. It begins with a brief consideration of the last (Wisconsinal) glaciation as a necessary preliminary to the discussion of the routes by which their ancestors arrived; and proceeds to the archaeological and osteological evidence bearing on the progress of these ancestors southwards.

The author's verdict on the evidence is threefold. First, the present Amerindian population is a variety of *H. sapiens sapiens* distinct from the Asian, European, African and Australian types, although having local sub-varieties due to causes at present unknown. Secondly, the bulk of its ancestors came by the Siberio-Alaskan route during a recession of the last glaciation; the exodus from Asia began 40–35 thousand years ago, but did not reach South America until 8,000 years ago or so. Thirdly, one cannot dismiss the possibility of a Malayo-Polynesian contribution to the most southern part of the continent; this could have come by a series of stages, trans-Pacific to begin with, but later along the northern edge of Antarctica during a recession of the glaciation there.

The last third of the text is devoted to the miscegenates due to the arrival both of the Iberian conquerors and of their later African slaves; and, more lately, of Chinese and Levantines. This part concludes with a temperate discussion of existing racial discriminations both between those of wholly Iberian descent and the miscegenates and within the miscegenates themselves. It should be of special interest to social anthropologists.

The clarity of the author's prose makes the book easy to read and, indeed, enjoyable. The maps are very clear; and the 41 photographs of Amerinds from all over the vast region studied are literally beautiful. Incidentally, were it not for their colour, a large number of the faces shown could be found in western and southern Ireland, an opinion shared by others who have seen them.

There is an extensive, useful bibliography and two good indices of names and places. The book can be recommended to all with even a moderate knowledge of Spanish.

It does not seem improper here to recommend the use of the word *Amerind* for all the truly native peoples of America. The pedantic can derive it, if they please, from a Platonic 'Americo-indigenates'!

M. A. MACCONAILL

University College, Cork

D. BROTHWELL & B. CHIARELLI (eds): *Population biology of the ancient Egyptians.* v, 380 pp. illus., maps, tables. Academic Press, 1973. £7·00

This is a collection of thirty-two papers delivered (for the most part) in 1969 at a symposium held in Montaldo Castle, near Turin There is a prologue by Il Chiarelli and an epilogue by Dr Brothwell. Most of these papers have already appeared in the *Journal of human Evolution*, the others are newborn.

The title is misleading. The word 'population' normally connotes a numbering of people and changes thereof. But of the present set of writings fifteen are medico-dental; seven are qualitatively historical; six are exercises in physical anthropology; two are dietary: and only two are attempts at a demography.

Any one of the articles will be useful to several sorts of people, few to all. Two articles are outstanding, partly because of their matter and partly because of their form, because both rise above the rather plodding prose of all the other earnest scholars who have disclosed their labours to us.

One is that of Satinoff on the medical biology of the early Egyptians, based on 1950 specimens. When discussing their oral pathology he is humane enough to remark that 'The overall dental health of the ancient Egyptians was quite appalling.' His paper is eminently readable, as befits a medical historian at University College London.

The other exception is by D. M. Dixon, also at U.C.L. After an irrelevant but informative prolixity of some eight pages, he gives us five pages of text on the things that the Egyptians chewed but did not swallow. His tale is enlivened by a humorous account of the Greek jokes against the Egyptians because

they used these things—including a delightful pun that he explains neatly to those who have not taken Greats at Oxford.

The book is well printed, with few errors in spelling. But it is the worst produced book the reviewer has ever read: for once its publishers must be chided. The several reprints of which it is composed are literally stuck together, so that any attempt to open its pages fully is likely to fracture its backbone.

An incomplete effort has been made to arrange the articles in a logical sequence. But the original pagination of each of them has been left unchanged. Consequently, the Table of Contents is like the game of Snakes and Ladders. You pass up a ladder from p. 3 to p. 65, up another from p. 72 to p. 137. The progress is orderly after that until you meet a snake at p 499 and slide down it to p. 31. And so on. But although there is a page numbered 499 the total number of pages is only 380, of which 33 are blank.

It is difficult to assign a market to this book. Even the libraries of many learned institutions could be reluctant to take it, because of the motley character of its contents and the fact that 29 of its 32 articles are already to be found elsewhere.

M. A. MacConaill

University College, Cork

Archaeology

Hawkes, Jacquetta (ed.) *Atlas of ancient archaeology*. 272 pp., illus., maps, plans, bibliogrs. London. Heinemann, 1974

The introduction to this book states that 'now that we have all shared in a moon's eye view of our little Earth it has become more nearly inevitable to treat its history as a whole'. These are admirable sentiments, and indeed the book would be an excellent guide to a swift-travelling archaeologist from another planet. It divides the world into seventeen regions and then deals, in one or two pages each, with 176 sites or groups of sites. Each local section has a brief introduction. If the coverage favours Britain at the expense of Europe, and the European area at the expense of other continents, this is perhaps a fair reflection of the volume of work on man's past and the distribution of sites worth visiting.

The book is meant for students and travellers, but both categories will require more information than this book provides, and this reviewer feels that the sixth-form library and the coffee-table will prove to be more appropriate destinations for it. What the book has gained in breadth of vision it has lost in superficiality; in almost every case adequate exposition is impossible. Some of the selections seem quixotic; at the risk of unfairness by selectivity it should be pointed out that Kerlescant, the Grotte des Fées and Bryn yr Hen Bobl are hardly the most typical or interesting of European funerary structures, that three Anglesey chamber tombs are three too many in a sample of only nineteen British topics, that Trelleborg is an odd inclusion in a book which spurns post-prehistoric sites in general, and that Skara Brae, Mousa and Maiden Castle are an inadequate sample of British prehistoric settlement sites, and so on. The implications of radiocarbon calibration are ignored, except as a note in the glossary, which is very odd practice for a book published in 1974. Messrs. Faber and Batsford have published good, fairly up-to-date guide-books to the archaeology of some of the areas covered, and serious-minded students as well as travellers are advised to seek these out.

Andrew Fleming

Sheffield University

Camps, Gabriel: *Les civilisations préhistoriques de l'Afrique du Nord et du Sahara*. 366 pp., illus., plates, maps, tables, bibliogr. Paris: Doin, 1974. F160·00

This challenging work is divided into seven main parts, passing from the Palaeolithic through the Epipalaeolithic and Capsian eras to examine in detail the question of the neolithisation of north Africa and the various Neolithic civilisations. The Age of Metals and Berber origins come in as the conclusion, although one feels that another whole chapter on these barely (in Africa) historic times would not be amiss.

A book devoted to so ambitious a subject must inevitably possess drawbacks, not least of which will be the difficulty of including the most up-to-date material to hand. A number of paragraphs in small print bear silent witness to apparent attempts to fit in, at the last minute, as many late arrivals as possible, although it may be that some are ideas. At the same time the problem of obtaining recent information on some areas or periods is amply demonstrated by the small space devoted to them, like, say, the Neolithic of West Sahara; here any author will find himself severely hampered, in the case of Spanish territory at least, by the mute testimony of virtual nothingness contributed, in respect of their African domain, by this usually garrulous nation. Indeed, we see reference to publications dated 1944 and 1946 on rock art, illustrating the enormous task of keeping abreast of the spate of modern scientific publications, of which just a few actually do deal with the territory.

While the bibliography is barely exhaustive,

a number of spelling mistakes in foreign-language references mars the presentation of this otherwise painstakingly compiled masterpiece, which also has a somewhat distressing tendency to publish tantalising photos without indicating on which page of the text the object portrayed is described in detail; thus one is forced to read through all references given in the index until hitting the right one. However topics like the non-feasibility of Saharan chariot-routes and enigmatic Capsian rock art vie with one another in maintaining reader interest.

Putting oneself in the place of a student faced with the study of the Saharan Neolithic, it is unfortunate that another book, published during the same year by a second eminent French professor, should contain differing nomenclature. Had these two specialists put their heads together, regardless of varying age, the results might have been even more spectacular. But there again, it may be that each of their fine books is a separate product of 'Vive la différence'.

MARK MILBURN

G.I.S.A.F, Hallein

SCHÄFER, HEINRICH. *Principles of Egyptian art*, edited, with an epilogue by Emma Brunner-Traut; translated and edited, with an introduction by John Baines. xxviii, 470 pp., 109 pls., 330 figs., bibliogr., indexes. Oxford. Clarendon Press; London · Oxford Univ Press 1974. £10·00

John Baines's translation of the idiosyncratic German of Schäfer's *Von ägyptischer Kunst* makes this work accessible for the first time to a large English-speaking audience. It also allows this monograph to take its place alongside the major generalising works of Erman, Breasted, Frankfort, and Groenewegen-Frankfort, which deal with ancient Egypt but have considerable potential interest for anthropologists. Schäfer took his doctorate under Adolf Erman in 1892 and became a member of the 'Berlin school' that reigned supreme in Egyptology until the 1930's. He published the first edition of *Von ägyptischer Kunst* in 1919 and continued to revise and expand it for thirty-eight years. The fourth German edition, on which this translation is based, was edited by Brunner-Traut and appeared posthumously in 1963.

Schäfer sought to examine with 'Germanic thoroughness' the rules by which ancient Egyptian artists had sought to portray reality. Although he dealt with reconstructing the mental processes of artists, he rejected as unscientific the notion that anyone can understand intuitively the feelings that art aroused in the ancient Egyptians. He therefore sought to understand these processes intellectually. Schäfer classified Egyptian art, along with that of modern children and primitive peoples, as 'pre-Greek'. In this opinion, the invention of perspective (foreshortening, shadow, etc.) was a unique historical event that took place in Greece c. 500 B.C. Where perspective art occurs in later cultures, it does so because of diffusion. Egyptian art is dominated by a concern for outline and for representing each part of a subject without distortion in terms of its most characteristic image. Schäfer describes this procedure as *geradvorstellig* ('based on frontal images').

Schäfer respected Egyptian art too much to dismiss 'pre-Greek' art as necessarily primitive or inferior to art based on perspective. He argued that mankind is universally aware of perspective as a characteristic of vision; hence the ancient Egyptians must have chosen deliberately not to employ it in their art Schafer concluded that Egyptian art was no less faithful to nature than art based on perspective. Whereas artists using perspective seek to represent the world as it appears in their eye, the Egyptians sought to capture objective reality. One approach results in no more or less distortion than the other. Although Schäfer was a staunch nationalist, the racist concepts that coloured nationalism throughout his most productive years found little place in his writing. Indeed, his ideas about 'pre-Greek' art are far more in line with Adolf Bastian's earlier concept of psychic unity.

Schäfer's theoretical approach, combined with his painstaking analysis, provides an internally consistent and convincing explanation of the representational procedures of Egyptian art in both two and three dimensions Nevertheless, his formulations are not so tightly integrated as to constitute a 'grammar of Egyptian art', as Brunner-Traut enthusiastically would have it (p. 421). Nor does it appear that Schäfer would have regarded such rigour as consistent with the actual procedures followed by Egyptian artists. While it is fascinating to have so many examples analysed in detail and to learn about the development of Schäfer's concepts, the book is prolix. Its basic concepts could be expounded clearly in a longish article.

Brunner-Traut's 'Epilogue' proposes the term *aspective* as a more acceptable alternative for *geradvorstellig*. She also argues provisionally that an aspective viewpoint dominates not only ancient Egyptian art but also Egyptian abstract concepts and language generally. She maintains that only with a perspective world-view can man perceive an object as a dynamic unity related to its environment. I regret that an excellent translation of a work of such great historical interest should have included

Brunner-Traut's unsubstantiated and probably unverifiable speculations, particularly when these are so alien in temper to Schäfer's own objective studies.

BRUCE TRIGGER

McGill University

KELLER, CHARLES M. *Montagu cave in prehistory: a descriptive analysis* (Anthrop. Rec. 28). x, 98 pp., illus., maps, tables, bibliogr. Berkeley: Univ. of California Press, 1973. $8.50

During the course of the last thirty years, our knowledge of the Early Stone Age has advanced from the collecting of specimens from gravel terraces and an ordering of 'stages' according to the presence of type-specimens, to attempts to reconstruct aspects of the life-ways of early Stone Age people by finding occupation and butchery sites, carefully analysing the total artefactual material in them and its areal distribution, and relating it to the ecology of the area and known ways of resource exploitation by observed hunters and gatherers In east Africa the sites of Olorgesailie, Olduvai, Isimila and Kalambo Falls had furnished valuable data of this kind, but the published descriptions of Acheulian occupation site material from South Africa were not in a form to facilitate ready comparison. Accordingly the author of the monograph under review set out to re-examine Montagu Cave, which had been shown by the diggings there of Haughton and Barnard in 1919 to contain Acheulian material, published by Goodwin in 1929

An area approximately 30′ by 30′, with another cutting of 10′ by 10′ at the back of the cave, was excavated over a period of 6½ months; visual stratigraphy was well marked and so it was possible to excavate by stratigraphical units; five such layers were recognised. No 1, nearest the surface, belonged to the Late Stone Age; the assemblage from Layer 2 was almost identical with that from Howeison's Poort, Layer 4 was sterile, derived from disintegration of the wall rock of the cave, but showing no action by wind or water and not to be taken, as previously, as indicative of climatic change Layers 3 and 5 contained similar Acheulian aggregates. Seven visually identifiable occupation surfaces were separately plotted and recorded in Layer 2 and four in Layer 5.

There are detailed analyses and full illustration of the lithic assemblages by means of good clean line-drawings, and comparisons with other East and South African sites. The Late Stone Age deposit of Layer 1, which produced a radiocarbon date of 7100±45 B.P., is compared with Wilton industries, agreed to be in an unsatisfactory state of definition and description. Five radiocarbon dates from the Howeison's Poort industry of Layer 2 range from 50,800 to 19,100 B.P., are described as earlier than expected but can now be seen to be more in accord with the revision of the dating of the South African Middle and Late Stone Age which has recently seemed necessary (Vogel & Beaumont, 1972, *Nature* 237, 50–1).

The description of the Acheulian material follows the scheme proposed by Kleindienst (1961) for East African material; the Montagu Cave assemblages are compared with the Types A–C of Howell & Clark (1963) for Isimila, Olorgesailie, Mufo and Broken Hill, and with those from Kalambo Falls. From these comparisons it is clear that the Montagu Cave Acheulian does not conform to the classifications for other sites in Africa; Montagu Cave differs in having i) a higher proportion of broken bifaces ii) a high frequency of 'minimally trimmed pieces' iii) a high frequency of 'other bifaces' that do not fit Kleindienst's categories. (The latter point illustrates the difficulty of achieving comparability of description by different workers on different sites: one wonders whether Kleindienst would have created this category if she had been analysing the Montagu Cave material or whether she would have included those specimens in the various members of her existing categories.)

Keller's conclusion, which certainly seems correct, is that Montagu Cave was used as a factory site by 'Acheulians' paying frequent visits to obtain raw material from the stream bed nearby. The conclusion is in the best modern tradition of careful analysis of artefacts leading to interpretation in terms of behaviour. The author is to be congratulated on a meticulous piece of work and on making a real contribution to our knowledge of the South African Acheulian in terms which allow of comparability.

The only criticisms are minor ones of presentation: one wonders what was the merit or necessity of orienting the plan of the cave (fig. 2) with north at the bottom of the page, and the plan of the features found in Layer 1 (fig. 7) with north on the left-hand side; as there are no north-points given in the 19 plans of Surfaces I to XI, one can only discover what their orientation is by reference to the area designations given in them and in fig. 7, as they are not given in fig. 2. This is a matter of some importance as, in order to 'read' and interpret the plans of the surfaces aright, one needs to know on which side is the cave opening. (This faces east, but the fact is not mentioned in the description.) No pages are numbered after p. 98, although the table of contents indicates that the figures begin on p. 99 and the plates on p. 131; there is no

pagination of the 'prelims', although the table of contents lists the preface as being on p. vii. The 'plates', being line-drawings, would normally be called figures; the frontispiece, being a photograph and thereby alone qualifying in the normal way for the status of a 'plate', has no caption.

THURSTAN SHAW

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HAMMOND, NORMAN (ed.). *Mesoamerican archaeology: new approaches.* xxiv, 474 pp illus., plates, maps, tables, bibliogrs. London: Duckworth, 1974. £10

The symposium on Mesoamerican archaeology held at the University of Cambridge Centre of Latin American Studies in August 1972 attracted a wide range of stimulating papers, both invited and volunteered. Twenty-five of the most significant of those papers have been gathered together in this volume which seeks to illuminate new approaches to the field of Mesoamerican archaeology. Inevitably any volume of this sort is uneven and impossible to summarise in a brief review. Particularly is this the case when the phrase 'new approaches' can be taken to mean such different things: novel theoretical approaches, the application of new models or techniques to investigate patterns in archaeological data, new directions of approach from iconography or ethnohistory, or even simply the application of traditional models and techniques to little known areas of Mesoamerica. Indeed each of the possible meanings of 'new approaches' is in evidence in the volume, and the result is a useful indication of the diversity of approaches now being employed in the field.

A number of the papers falls generally into the descriptive-historic vein. Lee's discussion of Middle Grijalva ceramic chronology has established a long sequence and correlated it with similar sequences in the Valley of Chiapas and on the Gulf Coast, thus helpfully closing a regional gap. But as Willey has observed there are many such gaps in Mesoamerica and 'in fact, there are more holes than cheese in the overall archaeological picture.' William's paper on external influences and the Upper Rio Verde drainage basin likewise tackles an area where current systematic information is scanty. Some of the papers in this genre are more ambitious and far-ranging; a notable example is Gifford's 'Recent thought concerning the interpretation of Maya prehistory,' an attempt at a cultural-historical synthesis of southern Mesoamerica which argues for seeing ceramic horizons as population movements.

Two areas which have resulted in much controversial writing in recent years are the Olmec problem and the question of long-range diffusion. The former is addressed by two conflicting papers, one by Grennes-Ravitz on the Olmec presence at Iglesia Vieja, Morelos and the other by Grove on the Highland Olmec manifestation. Of the two papers on long-range diffusion, that by Kelley, 'Eurasian evidence and the Mayan calendar correlation problem', will doubtless create the most controversial response. By pointing to the similarities between Mesoamerican calendars and those of China, India, and south-east Asia and by insisting that these calendric linkages must date from 350 B.C. or later, he has posed a weighty problem.

A number of the papers apply iconography and ethnohistory to Mesoamerican material with fruitful and suggestive results. Brotherston's essay, 'Huitzilopochtli and what was made of him,' is a nicely argued demonstration that the re-writing of history in the interest of the national image is scarcely a modern trait Both the contributions of Miller and Furst make important contributions to iconography. By examining the twisted cord enframing borders of the interior painting of Structure 5 at Tulum, Miller is able, by identifying it as a representation of the umbilical cord and by relating it to other sources, to make a strong statement about the underlying unity of Mesoamerican iconography and ideology. Furst's case for the presence of a morning glory symbol in Teotihuacan art is made more intriguing by his suggestion that the flower was used as a source of hallucinogens.

Three papers make useful contributions to the question of settlement patterns. Adam's attempt at an estimate of Classic Maya palace populations at Uaxactun suggests an unusually small aristocratic stratum, only 1-2 per cent. of the total population residing in the centre proper; the methodology he employs is interesting, but as he readily admits, full of potential pitfalls Puleston's investigation of the density of house-mound distribution in intersite areas in the vicinity of Tikal and Uaxactun contains a number of interesting and occasionally surprising suggestions. Hammond's study of the distribution of late Classic Maya major ceremonial centres in the Central Area answers a long-felt need for a broad view of Maya lowland settlement. It is a study that raises a number of key questions, particularly in view of the concentration of major ceremonial centres in the north-east Peten. Such questions range from why this concentration should have existed to the implications of such a concentration for the emergence of larger territorial states.

Space forbids mention of all the papers in this volume, though note should be taken of two papers relating to Teotihuacan, Millon's

study of urbanisation and urbanism there and the related paper of Cowgill on quantitative studies of urbanisation at the site, a useful if modestly stated indication of the application of computer procedures to archaeology. Willey's paper on the Classic Maya stelae hiatus goes beyond a description of the hiatus phenomenon by seeking to explain it 'as a result of the termination of a Mexican highland-Maya lowland symbiotic relationship, a relationship in which trade had probably been a crucial factor.'

The volume has been handsomely produced by the publishers. Though the unity afforded by the title 'new approaches' is in some ways more illusory than real, it provides a valuable perspective on the current (or at least 1972) state of studies. Methodology and approach vary widely, but one is conscious in reading these pages of the energetic development of Mesoamerican archaeology and of the way in which old techniques have been merged in a number of cases into the fruitful formulation of new problems.

ROGER HOWELL, JR.

Bowdoin College

PARSONS, LEE A. *Pre-Columbian America: the art and archaeology of south, central and middle America* (Handbook for the Exhibition Hall, Milwaukee Public Museum, Publ. Anthrop. Hist. 2) 193 pp., illus., plates, map, bibliogr. Milwaukee. Public Museum, 1974. $7.50

A small, comprehensive survey of New World high cultures is contained in this handbook. The organisation is admirably clear. An introductory section of 40 pages on the origins and characteristics of New World civilisations is illuminated by comparative statements and illustrations, and it forms a basis for the later exposition.

The next sections are similarly organised. The main parts of each are divided into major culture areas with a topical and artefactual survey. The latter summary is, of course, where the artefacts found in the exhibition hall are illustrated and discussed. Perceptive commentary enhances the value of these sections. A short reading list rounds off the book.

Any such catalogue-book has at least two functions. One is the obvious instruction and explanation of exhibited items. The other is the illustration of unpublished items, especially as may be found in relatively inaccessible regional or state museums. A third function is that such publications afford an informal but stimulating medium for professionals who can express new interpretations and ideas without the constraints demanded by the canons of scholarship. The book is a success on the first two counts, but aside from organisation, is very traditional. There are few surprises, but then, perhaps this is best in the treatment of such a comprehensive and yet thin collection. No one specialist can speak with equal authority and depth on such diverse materials. Parsons has chosen a good way out of the dilemma and has done it with style.

R. E. W. ADAMS

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HELMS, MARY W. *Middle America: a culture history of heartland and frontiers.* xvi, 367 pp., illus., maps, bibliogr. Englewood Cliffs: Prentice-Hall, 1975

This is a competent review of the published information in the field, drawing its claim to publication from being 'the first book to introduce the culture history of Mexico and Central America in a single volume, and the only one that covers this history from early man to the present'. This may or may not be true at the text-book level of this volume, but certainly there are other and better introductions to the prehispanic culture history of the area, and to the individual regions within it. The book is not useless, but is certainly lifeless and totally unoriginal; its intellectual mediocrity is matched by the format, with a confusing amalgam of several reference systems in footnotes, and the dim, undersized illustrations. I would not condemn it as an introductory text, but I would not buy it either.

NORMAN HAMMOND

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CAUVIN, JACQUES. *Religions néolithiques de Syro-Palestine* (Publ. Cent. Tech. Ecol. Préhist 1). 140 pp., illus., map, bibliogr. Paris: Librairie d'Amérique et d'Orient, Jean Maisonneuve, 1972. F36

After a first chapter in which the author sets out his views on the proper interpretation of prehistoric material for religion, i.e. burial customs, figurines or statuettes and sanctuaries or shrines, the material is treated chronologically in four subsequent chapters dealing with the Natufian, the first and second Pre-pottery neolithic periods and the pottery neolithic of the sixth and fifth millennia B.C. In a sixth and final chapter an evolutionary sketch of Syro-Palestinian religion from the ninth to the fifth millennium is attempted. A bibliography and a catalogue of figurines are appended.

The author duly emphasises the fact that, with the exception of burials, the evidence for neolithic religion from the Syro-Palestinian

area is very limited; indeed for the Pre-pottery neolithic A period it is virtually non-existent. Two figurines for a thousand years is certainly meagre evidence. Moreover, incontrovertible evidence for the existence of shrines or sanctuaries has not yet been presented with one exception, 'La maison des morts' at Byblos. Since the book was written two possible cult buildings have been excavated at Çayönu northwest of Diyarbakir and at Mureybet itself by the author. It is their peculiar decoration—coloured pebble mosaic floor and a wall painting respectively that sets them apart from ordinary dwellings of the eight millennium to which they appear to belong.

By linking the evidence for religion with the changing pattern of the economy and the ecology of the sites concerned, the author stresses the background for the religious changes observed. Evidence derived from comparative religion is rarely invoked, and the method of recognising ancestors of later gods in the prehistoric material is carefully shunned, rightly in my opinion.

The sequence which Jacques Cauvin builds up is certainly interesting. In the final food gathering Natufian culture, not yet neolithic in the sense that it did not produce food, the old representation of deities in the form of animals as in the Upper Palaeolithic Franco-Cantabrian art are still found, but they have apparently been reduced to a single animal: the gazelle. Gazelle horns and beads of gazelle bones accompany the dead into their graves. Gazelle statuettes are carved in the round, and gazelles decorate reaping knives, a new tool devised it would appear for reaping wild cereals. At the most advanced site, however, Ain Mallaha, the gazelle is missing and a number of asexual human figures appear. Others are also found elsewhere, such as at Wadi Mughara or at Ain Sakhri, where a statue shows an embracing couple.

In the following Pre-pottery neolithic A period (PPNA), gazelle sculptures have disappeared, and the only figures we have (two from Wadi Fallah) are clearly female. If some of the seated clay figurines of large breasted women with headdresses, recently found by H.de Contenson at Tell Aswad near Damascus may belong here (see *Annales Archéologiques Arabes Syriennes* 1972, p. 75 sqq, figs. 4–7 and BSPF 70, 1973, pp. 253–6) the author's case for the introduction of the seated female figure in the PPNA is greatly strengthened. This is of course the period of the introduction of agriculture, whether of cultivated wild crops as at Mureybet II and III or of domestic crops as at PPNA Jericho (ignored by the author) and of full sedentarisation. It is tempting to see a link between hunting and animal gods and between settled agricultural communities and seated female deities, but such links can be overdrawn.

In the next period (PPNB) the seated goddess continues, e.g. at Tell Aswad Beidha, Munhata, and less clearly at Jericho. Male figures also occur at T.Munhata. Yet the most striking feature of this period is the evidence for what is probably an ancestor cult in the form of human skulls with plaster masks from Jericho, Beisamoun and Tell Ramad and the numerous examples of headless burials at Abu Gosh, Beidha, Khirbet Sheikh Ali and other PPNB sites.

Changes in culture are evident with the coming of the Pottery neolithic, although I do not believe in a hiatus of 1500 years in Palestine. The plastered skulls disappear and also the practice of secondary burial, common since the Natufian. Seated female figures with strange elongated headdresses from Tell Ramad III show some similarities with similar figures in the Samarra levels of the Hassuna site of Yarim Tepe I in North Iraq. Figurines with a frightening aspect are characteristic of the Yarmukian of Munhata, Tel Aviv etc , but at the same time simple pebble figurines of a more archaic aspect still predominate at Byblos and at Shaaarhagolan. Clearly in these cultures the goddess is supreme, no males or children accompany her. Set against the Palestinian economic background of semi-sedentary occupation one wonders whether the 'terrifying goddess' from T.Ramad and Munhata represents a new deity or aspect from the steppe, harsh and intolerant like many of her successors (Istar, goddess of war, pestilence, underworld).

To sum up, in this the first comprehensive study of Syro-Palestinian neolithic religion, the author has paved the way for further research. He has clearly singled out the peculiarities of these Syrian deities vis-à-vis their Anatolian counterparts and this is important as most archaeologists are inclined to think in terms of one 'Mother Goddess religion' spread throughout the Near East, instead of many.

A few inaccuracies: p.65 monumental reliefs of the Anatolian goddess at Çatal Huyük are not on the north wall but the west; and plastered pillars have nothing to do with the reliefs but support the roof. On fig. 7, p. 35 Çatal Hüyük is placed much too far to the southeast; on fig. 2 caption 11, read Oumm, not Ounm ez-Zoueitina. These small errors hardly detract from the value of the book. If one may utter a wish, however, it is that much more material must be excavated and published before we can really form a definite opinion about Syro-Palestinian religion in the neolithic period.

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TUGGLE, H. DAVID & P. BION GRIFFIN (eds), *Lapakahi, Hawaii: archaeological studies* (Asian Pacif. Archaeol. Ser. 5). xxii 329 pp., illus, tables. Honolulu: Univ. of Hawaii, Social Science Research Institute, 1973, $4.50

For the most part, this volume is a collection of student papers describing the results of individual research projects. For this reason some of the reports are more readable than what we usually call professional archaeological studies. The fieldwork was done during the summers of 1969–70, on the island of Hawaii, in a land division known as Lapakahi which is being developed as a state park. The goals of the project were ambitious: to test models of human adaptation through archaeological research on an island. Apparently, we shall have to wait for the results, for the directors conclude:

> The present study is largely a preliminary attempt to examine the problem of population growth and social change with an ecological orientation. It is, however, very incomplete. It fails to have adequate control over the quantifiable variables of the human populations or over the relevant variables of the natural environment (p. 61).

Still, enough data were collected to provide us with a summary of the prehistory of Lapakahi. The earliest human habitation was around A.D. 1300. Two significant developments happened around A.D. 1450–1500. One was an expansion of agriculture into the upland area. The other was the elaboration of the coastal settlement of Koaie, including the construction of numerous stone platforms and 'the Great Wall.' The amount of labour involved in construction and agricultural activities suggests the establishment of Lapakahi as a distinct social and political unit around A.D. 1500. By the late 1600's coastal populations had expanded into less favourable locations. This was the result of both population growth and the production demands of a stratified social system. By 1778 (Capt. Cook's arrival) the system had reached its fullest production. There was intensive exploitation of marine resources. The cultural breakdown and population loss that characterised Hawaii after European contact also occurred in Lapakahi. Most of the area was abandoned by the early 1800's.

The various individual authors describe their excavations in meticulous detail. We learn their problems and disappointments. In my opinion, this is the most important contribution this book makes to students of Hawaiian archaeology. It gives us a good introduction to the kinds of data we can expect to get from Hawaiian sites, and the kinds of technological problems to be solved before finding the answers to the questions we have been asking. What is it? How old is it? These questions are hard to answer. Hawaiian sites often consist of little more than a few rock structures and the soil underneath. We find no pottery or arrowheads. Relative dating and functional analysis are difficult, if not impossible.

Some of the reports have special interest. In a brilliant piece of analysis Denis Callan shows how the percentage of cowrie shell weight co-varies with total midden weight. This discovery provides us with one kind of yardstick for measuring site variability. In another enlightening study Michael Kaschko explains how certain trails had the specialised function of separating Hawaiian land units; thereby giving us a means of determining prehistoric political divisions. Caren Arlas's study of one of the upland sites discusses the first excavated evidence of sweet potato in Hawaii before European contact. Joe Kennedy's report contains a good discussion of platform burials, and proof of crypt re-opening. Carol Sugiyama's analysis of upland agricultural features is a good all-round introduction to Hawaiian ethno-botany.

All in all, these reports are chock-full of detailed descriptions of Hawaiian archaeological features, and their excavation. They let us know what was found. What does it all mean? Analysis and synthesis await new data, new techniques and new strategies. While not for the general reader, this book is valuable reading for anyone planning to dig in Polynesia.

EARL NELLER
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Social Anthropology

NEEDHAM, RODNEY (ed., intro.). *Right and left: essays on dual symbolic classifications*; foreword by E. E. Evans-Pritchard. xxx, 449 pp., illus., port., tables, bibliogrs. Chicago, London: Univ. of Chicago Press, 1973. £9·75

When, after long preparation, a volume is published on the classical theme of lateral symbolism, considerable expectations are inevitably aroused. To such expectations the present work returns a curiously enigmatic response. Dr Needham tells us that when the idea of this book was originally conceived, in 1962, he planned a collection of essays which would provide a 'manageable conspectus' of the field, preceded by an analytical introduction which would put 'a general theoretical construction' on the various contributions. The latter project, unexceptional as it might appear, was subsequently abandoned in favour

of a 'more ordinary' introduction, which would allow the collection of essays itself to 'serve rather as a mine for theoretical exploration'.

It thus seems to be Needham's intention that the reader, and the reviewer, should perform the work of interpretation and evaluation that customary usage allots to an editor. In Needham's own words, the work is 'a kind of symbolic handbook'. The question then is what meaning we are to read into the symbolism implied in the editor's selection of the essays here presented. Needham furnishes us with a few ambiguous clues, which should be considered. Significantly, and no doubt wisely, he refrains from claiming that these chronologically ordered essays on dual symbolic classification exhibit any kind of general theoretical development of the theme of right and left. He does, however, suggest that the sequence demonstrates the 'increasing intensity of investigations'. Well, the discussions by (for example) Beidelman, Rigby and Middleton on lateral symbolism in the African communities where they did long field research, and Beck's study of right and left in south India, are undoubtedly supported by an incomparable wealth of ethnographic *minutiae*, lucidly and skilfully ordered. But surely none of the later contributions is informed with anything like the passion, the burning intellectual energy, evident in every line of Hertz's original formulation of the theme. Even the more notable later contributions, such as Evans-Pritchard's seminal discussion of Nuer spear symbolism, inevitably appear less 'intense' than the masterwork. Needham's contention about 'intensity' seems therefore to be peculiarly inapposite.

Several of the contributions rehearse Hertz's contention that the quasi-universal ascendancy of the right hand reflects a social reinforcement of a putatively slight organic asymmetry. But they all leave the problem exactly where Hertz left it. Littlejohn's discussion of Temne right and left includes some interesting speculation on the significance of the vertical 'median line' of the human body as a vector from 'high' to 'low' and its possible relation to the right/left distinction. The possibility remains unexplored, however. Nor, most surprisingly does the editor offer any comment on a translated essay by J. Chelhod, first published in 1964, which seeks to explain the pre-eminence of the right hand in Arabic culture as a result of the diffusion of a solar cult, thus radically controverting Hertz's thesis. Then there is Middleton's statement (p. 369) that there is 'little value' in listing examples of such a widespread phenomenon as dual symbolic classification abstracted from their cultural context. So much for Hertz and his brilliant colleagues! Middleton's dogmatic proposition would, however, seem to apply with some force to a little known article by Wieschhoff on right and left in African cultures, an ethnographic ragbag which might perhaps have been allowed to remain in a decent obscurity rather than achieving 'symbolic' representation here.

The greater part of Needham's introduction consists of a rather weird non-dialogue with a certain 'critic' whose mystical potency is apparently such that the editor takes the precaution, surely unprecedented in scholarly debate, of not once mentioning his name (though internal evidence suggests that the source of what the editor calls 'deleterious predispositions' and 'grudging derogations' about the analysis of lateral symbolism is none other than *John Beattie*). Needham's interesting exegesis of Nyoro classification and his well known re-analysis of the Meru ethnography both find a merited place in the collection, their coat-trailing *panache* contrasting the more sharply with the quietistic tone of the present introduction.

This book confronts the reader with a series of restatements and elaborations on the Hertzian problematic. Let me offer a generalisation of the kind which Needham so pointedly eschews, to which his eloquent silence lends, I suggest, a special significance. Something has gone wrong. What appears in Hertz, and nowhere else, is an ultimate and passionate concern with *our* culture, above all with the possibility of a unified, harmonious development of the whole body and mind (what Hertz called 'the organism'), right and left together. In so far as subsequent work has dealt exclusively with 'other', non-Western cultures, and has emphasised the disjunction between right and left, it constitutes a one-sided, partial development, a scholastic diminishment of Hertz's grandiose, humanistic vision.

R. G. WILLIS

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KUPER, ADAM. *Anthropologists and anthropology: the British School, 1922–1972*. 256 pp London: Allen Lane, 1973. £3·50

Dr Kuper has written an entertaining and consistently intelligible account of a half-century of British social anthropology. His audacity in desacralising the godlike founding fathers of our discipline and presenting them as human beings, warts and all, has predictably aroused fury in some quarters: it was nonetheless a necessary and salutary action.

In fact most of what the author has to say about the formative years under the contrasting reigns of Malinowski and Radcliffe-

Brown is by no means radical or controversial but soundly based and commonsensical. The first three chapters are written with authority spiced with wit—as when he aptly compares the received version of Malinowski's 'creation' of the exciting field-research-centred anthropology to a classical 'prophet' myth. From then on the book, while remaining readable, suffers an intellectual deterioration. Probably this is partly due to the fact that it is much less easy to write coherently of a subject which latterly, according to some of its most distinguished exponents, is characterised as a totality by its incoherence.

It would be absurd to chide Dr Kuper for his failure to perceive just where social anthropology is going. There are however some general criticisms to be made of this book. One is that, surprisingly in the work of a social scientist, it seems to reflect a 'great man' theory of history. The author concentrates on the 'stars' of social anthropology, but makes no attempt to situate the body of knowledge constituted by the discipline within the context of British culture and society of the period. The brilliance of the inter-war generation of British anthropologists thus remains an inexplicable quirk of nature. This lack of sociological perspective has some mildly comical side-effects. Dr Kuper talks of the 'immense power' of the 'professoriate', and of key departments being 'captured' for social anthropology In the words of the late Joseph Stalin, 'How many divisions has the Pope got?'.

R. G. Willis

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Cohen, Abner (ed.). *Urban ethnicity* (Ass. social Anthrop. Monogr. 12). xxiv, 391 pp. map, tables, bibliogr. London: Tavistock, 1974. £4·50

Perhaps—as the publisher suggested several years later—we should have called this a series not of monographs, but polygraphs, since in each the diversity of approach and subject matter is more evident than the coherence. In this one there is a core of argument to which most of the eleven authors contribute in some degree and which gives it more unity than is at first apparent. The core consists in the authors' reactions to the editor's earlier claim that ethnicity is essentially a political phenomenon, and to the assumption that urban ethnicity can usefully be studied in isolation from more general social processes and structures. Social anthropologists frequently start by trying to interpret the behaviour of known individuals and if these are members of nation-states in which every one is expected to have an ethnic identity, it is easy to assume that ethnicity is a primordial basis of definition and that it is unanalysable. But in African cities ethnic bonds have sometimes acquired a greatly enhanced significance, giving rise to the phenomenon that has been labelled re-tribalisation and supertribalism. What Clyde Mitchell would apparently call ethnicity as an emergent property is continually changing.

Unfortunately Mitchell in his contribution does not pursue this aspect but concentrates on interpersonal relations, using quantitative data from Zambia to differentiate between commonsense and analytical notions of ethnicity. He offers a snapshot taken through a microscope while time stands still. The other authors are less precise but more venturesome. Ulf Hannerz, writing about the U.S., makes the important point that the ethnic group is not intrinsically defined by a position somewhere in the urban opportunity structure, though it may seek monopoly control over such a position; any kind of group which has gained such a control may be expected to utilise it and defend it by political means. Badr Dahya, in an important paper about Pakistanis in Britain, shows that their social patterns cannot be explained adequately as responses to racial discrimination. David Parkin introduces concepts of congregational and interpersonal ideologies in seeking to explain the extent to which ethnic minorities (Luo in Kampala and Nairobi, Hausa, Temne, Kru) organise corporately. R. D. Grillo, in writing about a Kampala housing estate presents ethnic ideologies as substantially independent of the political relations embodied in the situation he studied. Enid Schildkrout, however, stresses the importance of structural factors and the irrelevance of culture for explaining the persistence of ethnic identities among urban immigrants in Ghana. P. C. Lloyd describes the ways in which ethnic distinctions were utilised in a Nigerian town in the mid 1950's. Edward Bruner in his discussion of Indonesian material ranges wider than the British-trained anthropologists and sketches the macro-sociological factors influencing the ways in which the expression of ethnicity changes. In many parts of south-east Asia an individual has more freedom to choose his ethnic affiliation than is usually possible in Africa. Among immigrants in Israel, to judge from Shlomo Deshen's study, some manifestations of ethnicity are primarily cultural and irrelevant to problems of competition. Like Grillo, Charsley, and some of the others, Deshen urges us to see ethnicity in terms of individual strategies, and he speculates about processes in the Israeli situation which have reduced its attractiveness in the political sphere. David Boswell's article, based on an analysis of the social network of a young

black Rhodesian in Zambia, connects with Mitchell's in bringing out the emergent properties of ethnicity in political and domestic contexts.

The editor's introduction is the one read at the opening of the conference. The reader will regret that Professor Cohen was unable to use this opportunity to reply to his colleagues, reflect upon the discussions, and give us a more complete presentation of the issues. Fortunately, however, S. R. Charsley's concluding paper brings together several of the main themes. He was invited to report on ethnicity in a rural part of Bunyoro as a 'control case' throwing into relief aspects of urban ethnicity, but what he has to say about the formation of ethnic groups goes far beyond that. He contends that in *The Kalela dance* Mitchell compared the outward-facing aspect of urban tribalism and the internal aspect of rural tribalism. Ethnicity always has both outwards and inwards facing aspects (which are not quite the same as the structural/cultural, political/primordial). In his field material Charsley has cases of ethnic group formation without the sharing of a common culture, and cases that cannot be explained simply in terms of the sharing of interests.

What may appear to be a simple phenomenon turns out to be very complex and it would be premature to expect a satisfactory synthesis. With Bruner, I believe that it will not be possible to make sense of the micro data unless we 'raise our sights and deal with the larger system', but that is the direction in which many of the contributors are turning. They have given us a volume that is as good as any in the series.

MICHAEL BANTON

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FRIEDL, JOHN & NOEL J. CHRISMAN. *City ways: a selective reader in urban anthropology;* foreword by J. Clyde Mitchell. xii, 458 pp., bibliogr. New York: Thomas T. Crowell, 1975. $6.75

Yet another 'urban anthropology' reader. This one consists of twenty essays arranged in six parts devoted to the 'Unit of analysis', 'Scope and method', 'Categories of urban dwellers', 'Specialized communities', 'Urban adjustment', and 'The culture of poverty'. Aside from the editors' opening chapter and comments on each part, all the papers have been published elsewhere, mostly in journals, though some appeared originally in yet other collections. There are annotated suggestions for further reading with each part, and a comprehensive bibliography at the end. However, as in so many recent collections, there is no index. This inevitably detracts from any value a volume such as this might have.

But what is its value? There is little point in assessing the individual contributions which have appeared previously. Their quality varies as in any collection—though the overall standard is impressive. What are the virtues of the volume as a whole? It is, of course, useful to have certain pieces in a handy form, though some, e.g. the papers by Wirth and Gans, have been anthologised before. Equally such a collection draws attention to work which the reader might otherwise have missed, especially that outside his main area of interest. One welcomes, therefore, the range of material presented. Welcome, too, is the fact that many of the papers are quite recent, though more venerable pieces such as Wirth's are also included. The volume may also serve, as the editors intend, as a teaching resource—but no teacher will find all the articles equally useful or wish to be constrained by the editors' selections. Indeed few of the papers are of such general significance that they would inevitably constitute the core reading for any course. Beyond these considerations is the claim that the volume makes a 'coherent statement about the content and practice of urban anthropology'. Unfortunately it fails to achieve this ambitious goal, and one questions whether a collection of essays orginally prepared with very diverse ends and audiences in view could ever provide the material for such a statement. The editors also state that 'the theme of continuity of tradition with adaptive change underlies the articles selected for this volume'. Fair enough, if the theme were pursued systematically and we were given some assessment of the state of knowledge. This is not the case. In fact, as the Preface suggests, the editors had several different ends in mind, and what started as a modest enterprise inspired by the needs of teaching acquired more grandiose and ultimately conflicting aims. Finally, the basic premise for the volume—that 'urban anthropology' constitutes a distinct field of inquiry—is hardly questioned. The sceptic wishing to be convinced of that will not find the answer here.

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HODGEN, MARGARET T. *Anthropology, history and cultural change* (Viking Fd Publ. Anthrop. 52). viii, 108 pp., maps, bibliogr. Tucson: Univ. of Arizona Press for the Wenner-Gren Foundation for Anthropological Research, 1974. $4.50 (paper)

Margaret Hodgen is a well-known authority on the early history of anthropology and an expert on technological innovations in the past. She is therefore well qualified to discuss the difficult problem of the interrelation

between the disciplines of anthropology and history. Her argument in this work may be summarised in an over-simplified way as follows. Anthropology is 'usually considered a natural science, descriptive, morphological and classificatory' (p. 2). This is a view she shares. Those anthropologists such as Evans-Pritchard and Kroeber who think to the contrary are wrong, partly because they have *not* 'felt it necessary to deal in depth with the essential nature of dated historical inquiry' (p. 26). We should prefer the views of the discipline put forward by Dr James Prichard in 1839 and E. B. Tylor in 1871 (pp. 2–3). While anthropology is a 'science', traditional history, as expounded by its practitioners, is an art. It consists of narrative descriptions of 'unique' events. It is not, for example, susceptible to that tool of all genuine sciences, mathematics. The authoress is convinced, however, that the two disciplines should converge. This means either that the scientific pretensions of anthropology must be dropped, or a new kind of history, different from that practised by historians, must be created So far there has been little success in the attempts to unify the disciplines. Those anthropologists who have used historical data have altered anthropology into an art, rather than turned history into a science. We are told that none of the 'ethnohistorians' has 'found reason to fault the procedures of their colleagues in departments of history or to question the assumption of the uniqueness and unclassifiability of events ... Although scientists by allegiance and tradition, they have accepted without comment the lineal, literary form imparted to the recovery of the human past by the practising historiographer...' (p. 22). In contrast, Margaret Hodgen hopes to develop a new and 'scientific' history, namely one based on the taxonomic methods of naturalists, and thereby to show that 'history ... is a far broader subject than that envisioned by narrative and literary historians' (p. 41). The first three chapters revolve around this discussion. They are based on a number of recent books and articles on the philosophy of history and anthropology and discuss in some detail the status and methods of the two disciplines.

The next two chapters show the method in action. The approach is outlined as follows: 'first, the acquirement of an appropriate and inclusive collection of more than one category of dated cultural changes or innovations, secondly, the separation within each category of differences or similarities, and, thirdly, in the presence of similarities of event-marked innovations in hand, the search for common antecedents or conditions' (p. 42). The two innovations selected for demonstration purposes are the introduction of early Christian churches in the years A.D. 1 to 400 and the establishment of printing presses between 1450–1500 These two phenomena are examined in detail with the aid of maps and tables. Although there is too little discussion of the accuracy of the sources used for the studies, it seems likely that for those particularly interested in early Christian churches or printing presses, the discussion would be of considerable interest. The conclusions emerging from the application of this new 'scientific' approach can only be indicated very briefly here. We are told that 'these earliest Christian congregations ... *were not the outcome of some 'natural' pinciple of cultural 'growth' or evolution, operating in unsolvable mystery on the institutions of the local religions*. They were demonstrably hard-won consequences of the personal effort of wandering, migrating, sometimes namable initiators' (p. 80, original emphasis). We are furthermore informed that 'Neither the printing presses nor Christianity appeared spontaneously out of a prior technology or religious system without the active intercession of active innovators, alien individuals, or small disaffected human groupings' (p. 82). Such conclusions would rejoice the heart of any 'traditional' historian. Indeed, except for the presence of more maps than one might normally expect, as well as the rather eccentric conjunction of the two phenomena selected, the account would not surprise ordinary historians in the slightest. Without the discussion in the first three chapters, they would find it difficult to detect in what way the analysis was any more 'scientific' than that carried out by any modern cultural or economic historian. Once again the sands of the past have engulfed the stream of rigorous methodological intentions.

ALAN MACFARLANE
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BERNARDI, BERNARDO. *Uomo, cultura, società: introduzione agli studi etno-antropologici.* 420 pp., illus., tables, bibliogr. Milano: Franco-Angeli, 1974. L7,000

Anthropology in Italy is an amalgam of physical, archaeological, material-cultural, cultural, structural and linguistic knowledge none of which seem to be taught in the same course or the same institution. Still it grows, and Professor Bernardi has produced a text book which could be used by most students wherever they find themselves. The first part covers nature and culture, the nature of man, cultural dynamics and research methods. The second covers schools and theories, the third *etnemi sociali*—social ethnemes which are: kinship, politics, economics, and religion-magic.

Reading it, you get pre-echoes of innumerable *viva voce* examinations in hot dusty rooms, where it will be recited by heart. Prof.: Cosa significa il termine 'Nuova Antropologia'? Stud.: La cosidetta nuova antropologia è un termine inventato dal Ardener nel 1971 per distinguere un corrente che viene denominato 'etnoscienza' in America. I principali scopi sono . . .

J. Davis

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Shorter, Aylward. *East African societies* (Library of Man). x, 155 pp., maps, bibliogr. London, Boston: Routledge & Kegan Paul, 1974. £3·20 (cloth), £1·50 (paper)

In this little book Dr Shorter is concerned to correct the old-fashioned anthropological view of east Africa (Kenya, Tanzania, Uganda) as 'a conglomeration of small (p. 1) socieites that are static and discrete'. He aims to show that this was never so and also to show something of the social and cultural changes that have occurred during the last century, principally as a result of Western influence in the region. The book takes the form of short chapters (about ten pages each) on ecology, history, pastoral societies, chieftain societies, urbanisation, rural revolution, marriage and family, education, religion, spirit possession and witchcraft. It is something of a mixed bag in which the basis for selection and emphasis are not clear. There are odd exclusions: for example, that whole range of east African societies which were and/or are neither pastoral nor with chiefs, earlier economic regimes, recent political processes at the local level. The focus tends to shift uneasily between 'traditional' and contemporary. For instance, on 'chieftain societies' (Shorter's term) there is a main concentration on history and traditional forms, with almost nothing on the special problems associated with colonial indirect rule or the modern abolition of chiefs. Similarly the chapter on witchcraft is almost entirely drawn from the traditional sources, although the significance of and recourse to witchcraft have often changed a good deal in recent decades. Probably because of space limitations, most references and examples are quite brief and, to a non-specialist, seem likely to be confusing. One soon perceives, moreover, that almost all the references are in tribal terms—the X do this and the Y do that—despite Shorter's admonitions on the difficulties of defining tribe or ethnic group. These references are not helped by a poor and sometimes inaccurate ethnic map.

One must sympathise with the author. He has a nigh impossible task in so small a book. For an ecologically varied region containing a couple of hundred ethnic groups with a range of experience during this century, he tries to cover history, traditional culture and modern change. It is a worthwhile task, certainly, when—as may be presumed—the result is directed towards high school students, young undergraduates and interested laymen without time or energy to go to original sources. It may satisfy many of their needs and it will probably give them a better picture, *in parvo*, than they can obtain in any other book. They will probably learn a little anthropological theory along the way. But there is little or nothing in the book which is of much interest to anthropologists—no fresh insights or new theoretical and ethnographic perspectives—and they would be well advised to go to the original materials if they wish to learn east African ethnography, old or new.

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Levine, Donald N. *Greater Ethiopia: the evolution of a multiethnic society* xvi, 229 pp, illus., map, tables, bibliogr. Chicago, London: Univ. of Chicago Press, 1974. £6·25

The purpose of this book is to examine those 'internal characteristics of Ethiopian society' which were 'essential conditions for the viability of the Ethiopian state' in the face of the 'Scramble for Africa', an examination which leads the author to develop what he calls a 'holistic conception of the Ethiopian experience'. After describing various 'conventional' and 'scholarly images' of Ethiopia, all of which he finds wanting because they are not 'holistic', Levine proposes a new 'image', that of 'Greater Ethiopia'· 'a vast ecological area and historical arena in which kindred peoples have shared many traditions and interacted with one another for millennia'. This 'congeries of loosely related units' was transformed into 'an emerging single societal system' thanks largely to the capacity of the Amhara for political domination and of the Galla (or Oromo) for 'selective affiliation' (i.e. adaptation to other cultures). Three chapters deal respectively with the book of legends known as *Kibre Negest*, and with the 'main structural features' of the Amhara and Oromo 'systems'. The Amhara and Galla capacities just referred to are then explained by means of certain cultural characteristics, and in a final chapter the author suggests that Ethiopia may be becoming 'a radically pluralistic community' with 'an internally specialized central sector' and 'a number of groups with little internal differentiation' providing 'living examples of kinds of human

experience and values which may give deep satisfaction to modernised Ethiopians'.

I find this book puzzling, for four main reasons. Firstly, the central thesis—that there is a single cultural area appropriately called Greater Ethiopia—seems tautologous. It is not surprising that grounds can be found for treating as a unit a number of contiguous peoples whom one has already classified as 'Ethiopian' on the basis of present-day political boundaries (Map 1, p. 36). Secondly, in view of the author's criticism of the 'image' of Ethiopia which he regards as distinctively anthropological—that of an 'ethnic museum' —it is odd that he should present, at the end of his book (and as the last quotation above indicates), a picture of the future Ethiopia which seems to qualify for just such a designation. Thirdly, the explanations of the 'capacities' of Amhara and Galla for 'political domination' and for 'selective affiliation' respectively also seem tautologous. This is most striking when these 'capacities' are related to the Amhara's 'passion to rule' and to the Galla's tendency towards 'associating with orders on a friendly basis'. Finally, the language used in the book is often puzzling. At precisely those points in the argument when one is especially looking for clarity of expression, one finds the way forward blocked by such metaphorical phrases as 'the Ethiopian experience', 'the societal enterprise', and 'an historical arena', which serve only to befuddle.

The book is probably just not long enough to achieve its ambitious purpose. On a less ambitious scale, however—as a brief, up to date and mainly historical introduction to Ethiopian studies for the general reader (to be read, perhaps, alongside the same author's well known work on the Amharas, *Wax and gold*), it can be recommended. The bibliography, which contains many recent titles, is particularly useful.

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STEWARD, JULIAN H. (ed.). *Three African tribes in transition* (Contemporary Change in traditional Societies 1). xiv, 519 pp., illus., maps, tables, bibliogr. Urbana: Univ. of Illinois Press, 1972. £2

The hardback edition was reviewed in *Man* Vol. 3 No. 1. I presume that the paperback has followed in the hope that it will be a useful teaching book in an area in which cheap books are few. At first glance the collection seems promising for that purpose. It contains an Introduction entitled 'Perspectives on modernisation' by the late Julian Steward and three monographs. The declared focus of the research was on the ways in which historical and economic influences, principally colonial rule and capitalism, had affected the tribal peoples selected for study. The first section (146 pp.) by Winter and Beidelman has one half on Tanzania: 'The national level', and the other on 'Ukaguru: the local level'. Both halves rely heavily on secondary sources and their combination has produced a narrative social history which is balanced, correct, contains some wry asides, but few new data. It is worthy but rather dull. Manner's contribution (154 pp.) on the transition by the Kipsigis from pastoralism to model farming explains how and why it happened, but has very little on the consequences the changes had on social relations. In both these sections most of the field observations have dropped out. Stanley Diamond's section (145 pp.) is on the Anaguta, a small tribe living very close to Jos, whose people have suffered so much that they have chosen, he argues, to go under rather than to change. It is written with some fire and feeling and sometimes achieves an old-fashioned charm of exposition but, again, there is more on what the culture has lost and on what the people do not do than on what actually goes on. The situation of the Anaguta, like so many peoples of the Jos Plateau, is heartrending but it is the author's shock and despair which is communicated rather than that of the people.

I doubt, then, if this volume would be useful as a teaching book. The data are too slight or too general to enable students to make their own analyses. The different theoretical approaches used are not brought out clearly nor do they inform the narratives. Even conceding that cultures may be comparable, those here are too disparate for useful comparison. But especially, a book used for teaching must provide refreshment for the teacher each time he returns to it. I shall not re-read this one.

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COLE, PATRICK. *Modern and traditional elites in the politics of Lagos* (Afr. Stud. Ser. 12). xii, 297 pp., bibliogr. Cambridge: Univ. Press, 1975. £6

'I have spent the better part of my life in Africa . . . But after 29 years and after nearly 12 years as Governor here, I am free to say that the people of Lagos are the lowest, most seditious and disloyal, the most prompted by purely self-seeking money activities of any people I have ever met.' Thus wrote Lord Lugard to his wife Flora in 1916. Compared with the feudal splendour of the Hausa-Fulani emirates, the parish pump politics of Lagos, elevated to become national issues, sorely

tried the colonial administrator. Yet there is no reason to believe that Lagosians, in seeking their own goals and best advantage are in any way different from peoples elsewhere in Nigeria or Africa. In this admirable book the author deftly steers us through the issues that made Lagos politics in three-quarters of a century—from the early years of colonial rule to the years immediately prior to the second world war. It is a fascinating story of competition between different interest groups.

The picture drawn is indeed a complex one. The original settlers of Lagos were Yoruba but the town later came under Benin influence, perhaps by conquest. Four grades of chiefs vied for influence over the *Oba*. This traditional political structure has been maintained until the present but as Lagos itself has changed out of all recognition from a fishing/farming settlement to a national capital so have the rights and duties of chieftaincy altered and become subject to reinterpretation. To the indigenous population were added, in the nineteenth century, not only immigrants from the Yoruba interior—maintaining their ties with their homelands and often organised on an ethnic basis, but also two groups who dominated the town's social and politicial life—Saros, liberated slaves from Sierra Leone, many of them educated and evangelised by the C.M.S. and the Brazilians, returning from Brazil with craft skills and Roman Catholicism. Counterpoised to the Christian groups was a large Muslim community. With the development of trade a strong indigenous trading group emerged, often contesting power with the traditional office holders.

By the end of the century, Lagos politics polarised in an opposition between blacks and whites and nationalism was born. But among the educated Lagosians rival groups emerged. The one sought identification with and assimilation into the European community; the other rejected this. The first was epitomised by Sir Henry Carr, successively Resident of Lagos and Director of Education; the second by Herbert Macaulay, the father of Nigerian nationalism; both men came from prominent Saro families. Macaulay intervened in a number of local issues—rights of traditional Lagos chiefs to land, the election of and power to depose the Oba, the ranking of chiefs, the payment of water rate—travelling on occasion to London to put his case before the metropolitan government and people and creating embarrassment for the Lagos administration.

Dr Cole has given us a book which is both pleasant to read and a work of immense scholarship; footnotes and references occupy a third of the book; there can be little of relevance that he has not read and incorporated into his study. Nevertheless after reading his account one pauses to wonder how involved the humbler citizens of Lagos became in these issues; how significant to them were the wrangles over kingship and chieftaincy. Again, the government tends to be seen as monolithic—were there not more marked divisions of opinion among the administrators, and did not the expatriate commercial community exert influence on any issue? These thoughts, however, imply no criticism of this excellent work—a real contribution to our knowledge of Lagos.

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FORTIER, JOSEPH (ed.) *Dragons et sorcières. contes et moralités du pays Mbaï* (Class. afr. 14). 365 pp., illus., map. Paris: Armand Colin, 1974

In an earlier collection, no. 6 of this series, Father Fortier, S J. translated those narratives of the Mbaï people of Chad which concern the adventures of their mythical hero-trickster Sou; their gods and spirits provided the *dramatis personae*. In this later but companion volume he has edited forty texts in which human and animal characters predominate, among them many old friends—the orphan who achieves success, the hyena outwitted by the squirrel, the animal wife secured through theft of her skin. These narratives allude very definitely to traditional morality, concluding formulae usually pointing a recommendation. For example, the story of that well-known social nuisance, the beautiful girl who rejects all suitors ends 'C'est pour cela qu'une fille qui veut se marier doit écouter les conseils de son père et de sa mère'. Arranged marriages and parental authority are reinforced. Father Fortier's notes on Mbaï society give sufficient information for meaning to emerge but at times (and particularly in the three appendices on marriage procedures, genealogy and divination) one would wish that more anthropological data were provided, especially in the case of sorcery which runs as a thread through these narratives as through Mbaï daily life But it is in the notes which consider the texts as literary structures that there tantalisingly hovers the ghost of another book. Whereas the introductions to each group of narratives refer the Mbaï material rather simplistically to 'international folklore', the notes perceptively indicate differences in provenance and diffusion, presence of archaic material, even a suggested psychoanalytical interpretation of the first narrative both restrained and plausible. Conversion of an origin legend into a 'modèle réduit' is linked to the obsolescence of the lion-men society which acted as its social anchor. A chart analyses, in a method

reminiscent of Lévi-Strauss, a common structure to four tales, a narrative string reading 'séduction—déception—moquerie—femme dévorée—monstre éliminé'. It is such insights that make one hope that Father Fortier will not always limit himself to 'objective scholarship' (i.e. texts, translations, footnotes) but write a more developed work on Mbai narrative from the anthropological and literary analyses implicit here. In the meantime this is a very welcome addition to the library of African oral literatures.

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RIESMAN, PAUL. *Société et liberté chez les Peul Djelgôbé de Haute-Volta: essai d'anthropologie introspective* (Cah. Homme N.S. 14). 261 pp., plates, bibliogr. Paris, La Haye: Mouton, 1974

As the title 'society and freedom' suggests, this work is an attempt to bridge the gap between the disciplines of social anthropology and psychology. Although we are presented with a social anthropological study of a Fulani community, the main interest of the author lies in how the individual relates to society. The theme that runs throughout the work is that of personal freedom from the constraints of society.

The work is based on a doctoral thesis presented to the University of Paris. It is the result of two years fieldwork among Djelgobe Fulani in the Djibo area of Upper Volta. The author was accompanied by his wife, and this may be of significance when considering the wealth of material concerning women and children that emerges throughout the work.

The book is divided into two parts: the first part is an ethnographic introduction to the community under study. The second part is a more thorough analysis of certain Fulani concepts, patterns of behaviour and aspects of culture, which struck the author as being significant in understanding the Fulani approach to life. For example, the author discusses to what extent various emotions are expressed or repressed among different groups within the society, and the significance of 'shame'. He also considers the way in which individuals conform to their roles in society, and what happens in certain cases when they do not. Again, he discusses the meaning of freedom, and especially how it varies according to one's sex: a married woman achieves personal freedom through giving birth to a child; whereas a man achieves personal freedom through possessing cattle.

The author's practice differs from that of many anthropologists, who tend to play down their own personal involvement in the society under study; and one is often left wondering whether the account reflects the society or the anthropologist? On the contrary Paul Riesman considers it of utmost relevance to describe his own personal reactions to fieldwork situations, especially those he found difficult, or shocking. This study comes across as a very honest attempt to analyse just what it means to be a Fulani, as opposed to a European or a member of another local ethnic group.

CLARE OXBY

SOAS, London

HALL, EDWIN S JR. *The Eskimo storyteller. folktales from Noatak, Alaska*; drawings by Claire Fejes. xvi, 491 pp., illus., map, bibliogr. Knoxville: Univ. of Tennessee Press, 1975. $18.50

The core of this book is the repertory of 176 narratives recorded in 1965 from an elderly Eskimo, Paul Monroe (Pulungum), of Noatak village, northwest Alaska. To these are added fourteen narratives from an Eskimo woman, Edna Hunnicutt (Iraahuruk), of the same village. Hall's avowed aim is to present the range and extent of these individual repertories. The texts are indeed of great interest, especially if read in conjunction with Spencer's collection from north Alaska, (Bull. Bur. Am Ethnol. 171, 1959). Hall himself continually stresses a serious limitation of his own collection—that the texts were recorded not directly in Eskimo but in English translations provided line-by-line by bilingual Eskimo intermediaries. An equally serious limitation, however, is his apparent lack of training in literary analysis for which an uncritical use of Thompson's motif-index is no substitute. The superficial if wordy discussion of the texts as literature reveals at times a startling ignorance, e.g. the section on literary conventions (pp. 419–20). Further, there is a naive unwillingness to see that even the very lengthy repertory of *one* storyteller (or even two) cannot constitute an adequate basis for establishing individuality of repertoire; for that one requires a sample of storytellers large enough for comparative analysis. However, even one repertory can yield through content analysis interesting data on the social significance and functions of the narratives. Here Hall is on much firmer ground—conflict and co-operation between kin, shamanism, the prevalence of murder, etc. This part of his discussion is more assured and perceptive, but apart from the 190 texts the book offers little to the student of oral literature.

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SCHNEIDER, DAVID M. & RAYMOND T. SMITH. *Class differences and sex roles in American kinship and family structure*. x, 132 pp., bibliogr. Englewood Cliffs, N.J.: Prentice-Hall, 1973

This book is a companion volume to Schneider's *American kinship: a cultural account* (1968 New Jersey) and is subject to much the same critique. In their preface, the authors say that the present volume reports the findings of their study of fifty-nine lower-class families in the Chicago area. Of the fifty-nine Afro-American, Southern White, and Spanish-American families included in the study, only forty are said to have provided relatively full information. This is hardly a representative sample of lower-class Americans, even in the Chicago area. Materials on middle-class American kinship and family structure come from their experience of living in the United States and from Schneider's earlier kinship studies. For both middle and lower class, they can also draw on Smith's work with Afro-American kinship (*J. comp. Fam. Stud.* 1, 55–70). None of this material is reported in detail here.

The slender data base may not be a major handicap to the present work since Schneider and Smith deal with models rather than behaviour, usually write in the ethnographic present, and produce their conclusions as givens. They refer to their interview materials in specific terms only twice: once in a brief quote from one informant and a second time to describe the kinship network of one of the women in the sample. This reflects a primary interest in kinship as a symbolic rather than an action system.

Schneider and Smith start with the assumption that there is such a thing as an American kinship system, defined as a set of concepts. This is its 'cultural aspect'. Readers of Schneider's earlier book will already be familiar with its features. Americans are said to emphasise a basic dichotomy between 'blood' and 'law' in defining kinship and to give primacy to the nuclear family as the basic kinship unit. The ethnographic facts of the 1970's may not fit this model, but it is not meant to describe how people behave or the particular terms in which they discuss kinship relationships. Rather it reflects an interest in discovering concepts of a high level of generality comparable to those that organise linguistic phenomena. Here Schneider and Smith are working in a fashion common to much contemporary work on symbolic systems, although ultimately their approach also owes much to Talcott Parsons and the structural/functionalists.

The chapter on 'Class and ethnicity', which includes a brief history of the development of the highly stratified and multi-ethnic population of Chicago, is a good summary but has little to do with the rest of the book. In the subsequent chapters, the authors return to ideal models of middle- and lower-class patterns. They largely ignore both ethnic variations and the differences between working class and those who have little hope of ever obtaining permanent employment.

Schneider and Smith argue that it is possible to distinguish 'pure kinship' from kinship in practice. All Americans, they maintain, share the same 'pure kinship' system, though not all Americans adhere to the same family patterns or remember or interact with the same range of kin or apply kinship terms in quite the same fashion. They argue (p. 42) that this is because those who occupy roles as relatives 'are differentiated by sex, age, occupation, and many other criteria, over and above their pure kinship status, and it is these factors that lead to variations in solidarity patterns.' For some reason, not clear to this reviewer, Americans of different classes and ethnic background agree about what kinship is, but are able to disagree about definitions of appropriate behaviour for men and women or members of different age groups. All share the kinship role 'father', but since each class and ethnic group attributes to those who occupy the role of 'father' specific age and sex attributes, 'fathers' in practice differ from each other. Attributes of occupational and other roles also affect performance as do such objective factors as wealth and poverty.

The argument is interesting but at this point must be regarded as a set of hypotheses which Schneider and Smith are using as guiding principles in organising their still unpublished data on kinship. For those who are interested in kinship relationships among working class and out-of-work Americans, good data are provided by C. Stack (*All our kin*, New York, 1974) and G. S. Rosenberg and D. F. Anspach (*Working class kinship*, Lexington, 1973).

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BOOKS RECEIVED FOR REVIEW IN *MAN* APRIL–JUNE 1975

ABD-AL GHAFFAR MUHAMMAD AHMAD. *Shaykhs and followers: political struggle in the Rufa'a al-Hoi Nazirate in the Sudan.* 170 pp., illus. plates, maps, bibliogr. Khartoum: Univ. Press, 1974

ALI, S. HUSIN. *A Malay peasant society and leadership* (E. Asian social Sci. Monogr.). xii, 192 pp., maps, tables, bibliogr. Kuala Lumpur: Oxford Univ. Press, 1975. £8·60

ANDREWS, GEORGE F. *Maya cities: placemaking and urbanization.* 468 pp., illus., plates, maps, bibliogr. Norman: Univ. of Oklahoma Press, 1975. $20

AUGE, MARC. *Théorie des pouvoirs et idéologie; étude de cas en Côte d'Ivoire* (Coll. Savoir). xxiv, 440 pp., illus maps, tables, bibliogr. Paris: Hermann, 1975. F 48

BANTON, MICHAEL & JONATHAN HARWOOD. *The race concept.* 160 pp., bibliogr. Newton Abbott, London: David & Charles, 1975 £4·50

BENNETT, JOHN W. (ed.) *The new ethnicity: perspectives from ethnology;* 1973 Proceedings of the American Ethnological Society. xiv, 334 pp., tables, bibliogr. St Paul: West Publishing Co., 1975. $7.99

BERRY, SARA S. *Cocoa, custom and socio-economic change in rural western Nigeria* (Oxf. Stud. Afr. Affairs). xiv, 240 pp., maps, tables, bibliogr. Oxford: Clarendon Press; London: Oxford Univ. Press, 1975. £7

BICKERTON, DEREK. *Dynamics of a creole system.* viii, 224 pp., tables, bibliogr. Cambridge: Univ. Press, 1975. £7·25

BIERHORST, JOHN (ed.). *Four masterworks of American Indian literature ...* xxvi, 271 pp., illus. New York: Farrar, Straus & Giroux, 1974. $12.95

BIOSCIENCES COMMUNICATIONS. Vol. 1, No. 1. 60 pp., illus., bibliogrs. Basel: Karger, 1975. $60 p.a.

BRITISH JOURNAL OF SOCIOLOGY. Vol. 26, No. 2. 133–262 pp., tables. London: Routledge & Kegan Paul, 1975

BUCK, HARRY M. & GLENN E. YOCUM (eds). *Structural approaches to South India studies.* vi, 225 pp., illus., tables, bibliogrs. [Chambersburg, Pa.]: Wilson Books, 1974. $6.90 ($4.90 by prepaid mail order)

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Made and printed in Great Britain by William Clowes & Sons, Limited, London, Beccles and Colchester

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THE JOURNAL OF THE ROYAL ANTHROPOLOGICAL INSTITUTE

NEW SERIES, VOL. 10, NO. 4 DECEMBER 1975

CONTENTS

INDEX OF BOOK REVIEWS

CHANGES IN PROPERTY TRANSFER AMONG GREEK CYPRIOT VILLAGERS

PETER LOIZOS

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(with an appendix by Christopher Langford,
London School of Economics and Political Science)

This article attempts to explain a specific instance of social change. It also explores economic aspects of marriage and presents some ethnography on marriage arrangement. Fifty years ago rural Greek Cypriots customarily provided their sons with houses at marriage, but since then it is increasingly the daughters who must receive a house at marriage. Explanatory pride of place is given to a shift in the numbers of men and women available at age of marriage. Other contributing factors include the declining importance of agricultural land, as compared with wage-labour, in the establishment of a viable domestic unit; and certain changes in the social position of women. The article attempts to relate family decisions to marriage market pressures, and both to general economic and demographic constraints. In an appendix a demographer examines the Cypriot censuses and discusses problems of interpretation and inference which bear on the main argument

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'Their love of family is most exemplary The continual care of parents is the settlement for life of their children, and for this purpose, as soon as their family comes to the age of maturity, they portion out their property . . .'

R. H. Lang (1878 203) *Cyprus.*

'When a peasant marries he takes his wife with nothing else than a box containing the few clothes she may have, and he is thought uncommonly fortunate if his father-in-law be able to give with her a mule or a donkey. The consequences of this misery are such as might be expected.'

Turner, in C. D. Cobham (ed.) (1908: 448) *Excerpta Cypria.*

I

The problem

Among the several aims of this article the first is to explain a specific change in the mode of marital property transfer among Greek Cypriot villagers, and to suggest that this change is by no means trivial for the economy of the wider society. A second aim is to give a limited ethnographic account of marriage arrangement in a single village. A third is to show the merit of interdisciplinary co-operation to produce an explanation which an anthropologist could not achieve unaided. Lastly the article will exemplify an analytic approach developed by Raymond Firth (1964: 30–58) and Frederick Barth (1966; 1967) which seeks to relate a particular type of social change to the decisions of individuals within a framework of cultural and economic constraints. Precisely because my example is a homely one—a structural change concerning which partner's family is expected to provide the marital house—it lends itself particularly well to such analysis,

demanding as it does a consideration both of individual behaviour and aggregate data.

The two quotations which head this article are tantalising hints at the issue of provision for children at marriage. More recently we are fortunate to have an account based on a survey conducted in most of Cyprus's 600 villages:

> ... the bridegroom provides lands, animals and a house, except in certain hill villages near Paphos and Limassol, five near Morphou, and the villages close to Nicosia where it is provided by the bride. The bride provides lands, furniture, cooking pots, bedding and personal clothing (Surridge 1930: 25).

Surridge goes on to say that over 60 per cent. of 'dowries' for the provision of a house, animals, and furniture, etc. are paid from loans obtained at a high rate of interest from local moneylenders, and the context makes it certain that he does not mean by dowry the marriage portion of a girl only, but also of a boy.

By 1968, when intensive fieldwork was carried out in a single Cypriot village, it was the custom in that village, but also as far as was established, throughout the island, for the house at marriage most commonly to be provided by the family of the *girl* (see table 1). To say something is 'the custom' is to describe a central

TABLE 1. Marriages in Kalo village by relative contribution of bride or groom's family to the house complex.

Period	*Total marriages*	*Groom built and mixed*	*Bride built*
Before 1930	20	15	5
1931–40	37	27	10
1941–50	45	19	26
1951–60	45	10	35
1961–69	43	10	33

tendency, and there are inevitably a number of exceptions for special circumstances, and of mixed situation which make classification difficult. In table 1 the direction of change over more than forty years is suggested for a single village. The method of classification adopted deliberately understates the change. Only when the bride's family have contributed at least two-thirds of the cost of house plot, house structure, and its contents, has the situation been classed as 'bride built'. All cases where the contribution was more balanced have been included in the 'groom built' column. As will be obvious, in no situation does one side provide every material item involved in the house property complex—the issue discussed here is always one of relative, not absolute, contributions. For the villagers the issue is less statistical than normative. They say village custom requires houses be built for girls; unlike anthropologists they do not do sums so much as make intuitive classifications of instances and central tendencies. Much depends also on the politics of affinity in a particular instance.

My casual inquiries in villages all over the island confirmed Surridge's reports that before 1930 the house had very commonly been provided by the *boy*'s family at marriage. Much of the article will consider why this change took place. How-

ever, before going further into the problem a number of comments on method must be made. Intensive fieldwork in a single village supported by brief inquiries in at least twenty-five other villages all over the island are the main first-hand sources used. The other main source is the national censuses published for the island since 1881. The gap between these two sorts of information could scarcely be greater, and there are many difficulties about any attempt to bridge it. The argument is sometimes phrased as if it applies to the island in general, but usually in terms of a single village. It is well known that the tendencies of aggregate data need never be mirrored in a particular local unit, and still less can data from a single unit be inflated to yield facts about aggregates. But one level of data and analysis may suggest interpretations of another, and may support the plausibility of particular explanations.

The article will discuss in separate sections some features of Kalo village; the traditional arrangement of marriage; and as it appeared in 1968; some consequences of the modern mode of property transfer; the role of numbers of men and women at age of marriage in leading to the change in mode of provision; the mechanics of the change at the level of the individual actor; and other factors which have contributed to the change. Langford, a demographer, will then analyse data taken from a number of censuses, suggest their limitations and indicate how far they lend support to my argument (see appendix). It is only fair to say that had his analysis failed to show what it in fact shows, my argument would never have been conducted in its present form, so in one sense Langford's work is prior to my main argument.

II

Some features of Kalo village[1]

In 1968 the estimated population of Kalo was 1430 persons, a threefold increase from the 1921 figure of 452. It is a compact, nucleated village, exploiting rich irrigated land through the cash cropping of citrus fruit, potatoes, carrots and many other legumes. The organisation of kinship is bilateral and the normal domestic group is a nuclear family. The Greek Orthodox Church of Cyprus does not permit marriage with any person related within five degrees of consanguinity, counting the link between siblings as one degree: a man must not marry a collateral second cousin, the daughter of a first cousin, and so forth. Village custom strongly supports Canon Law on the matter of exogamy, and even if the Church readily made dispensations to second cousins, their union would be incestuous in village eyes. In spite of this rule, table 2 shows that for more than forty years roughly four out of every five marriages extant in the village are between two persons born in it; this is supported by other data not given here, and is to be understood in terms of the increasing prosperity of the village making it a good place to stay in, combined with the fact that as the population increased it became relatively easier to find a marriage partner not forbidden by the exogamy rule. In fact table 2 shows that before 1940 the number of marriages involving outsiders was proportionately greater than more recently.

The transmission of property is also bilateral, and it is the strong wish of all parents to transfer to each of their children at marriage an appropriate share of the natal family property. This property usually has two important components—

TABLE 2. Extant marriages in Kalo village by origin of partners.

Date of marriage	*Total*	*Groom from outside*	*Bride from outside*
pre 1930	20	1	5
1931–40	37	1	5
1941–50	45	8	2
1951–60	45	7	1
1961–69	43	7	1

land, and houses.[2] The transmission of land to all children is accompanied by the dispersal of small plots. Typically, a domestic group may hold lands in a dozen different places around the village in small strips of a third of an acre. A single strip of land of two acres is considered a large plot; a holding totalling six acres is considered a large holding for a man, his wife and four children.

Over the last forty years the village has become increasingly integrated into a market economy, with more and more villagers earning most of their living from non-agricultural activities, but continuing to own and work some land (see tables 3 and 4). Not only is land no longer so important for household viability, but the

TABLE 3 Changes in primary occupations, Kalo village, 1923 to 1968

Primary occupation	% 1923–35[a]	% 1968/9[b]
Civil servants (teachers, police, firemen, clerks, etc.)	0	9·5
Fulltime farmers	58	41
Skilled manual workers (tailors, builders, carpenters, etc.)	15	8
Shepherds	7·5	4·5
Unskilled labourers	10	15
Muleteers	6	0
Motor vehicle owners/drivers	0	10
Others (coffeeshop keepers, small retailers, etc)	3·5	11·5

[a] Base number is 81 entries in the muktar's register of birth, between 1923–35. This unfortunate spread of 12 years was essential to produce a large enough number, since many persons had several children in this period, but could only be counted once. Nor did all villagers register their children's births in their own village—some were registered in a town, if born in a clinic

[b] Base number is 195 male household heads, whom I questioned in 1968–9

Table 4. Composition of employment in Cyprus, 1931 and 1967.

Sector	1931 (%)	1967 (%)
Agriculture, forestry and fishing	51	39
Industry	26	24
Commerce and services	22	37

Source: M. A. Attalides (1974).
Cyprus Census 1931 and Cyprus Planning Bureau 1968.

relative value of houses has increased. This is because before 1940 houses were made largely of mudbricks, which the villagers usually produced themselves. The services of such specialists as carpenters and masons were less used traditionally and when one asks old people how much their houses cost they usually mention the amounts paid for wooden roofbeams, and for the materials and labour of carpenters to put in doors and windows. Nowadays, houses are more luxurious and permanent, and require more specialist labour and more expensive basic materials.

III

The traditional arrangement of marriage

I shall now describe briefly the arrangement of marriage in a period continuing at least until 1930. The village economy was in the main one of cereal cultivation, relying on winter rain, oxen as draught animals, and light scratch ploughs. Sheep, goats and pigs were kept. Those without enough land either worked as shepherds, labourers, or craftsmen such as tailors, builders, and carpenters, or left the village altogether. Each nuclear family occupied a separate dwelling, and for farming families this house stood on a larger piece of land with outhouses for the draught animals and the storage of cereals and implements, the whole yard being enclosed by a mud wall topped with thorns. Elderly informants often mentioned the difficulty in finding a house-site on which to build for a child's marriage, since the favoured land within the settled nucleus of the village was all owned and expensive.

The preferred and commonest pattern was for a man to provide his son with the building land and help him construct a house on it. If the family was too poor, they sought for a woman (often a widow or an only child) with a house of her own. Elderly parents often gave up their large house to a child at marriage, and then lived in the same courtyard, but in a smaller, purpose-built one-room house. After some years they sometimes moved to the courtyard of another married child.

Marriages were arranged by parents, who discussed the matter between themselves and then usually communicated the agreement to their children as a *fait accompli*. Marriage was then, as now, an issue which reflected the prestige of the participating families. Men arranged marriages with other families they regarded as equals in wealth, health and honour; inevitably some people tried to marry up and sometimes managed to create alliances across slight differences of wealth, if they had more of something else, and the wealthier side less. One important comment male informants often made about this period was that: 'It wasn't so easy to get married in those days. You often went to several houses before they accepted you.' The actual negotiations or tentative soundings were conducted in secrecy, through intermediaries, since to be refused was to lose prestige.

In passing it is worth noticing that genealogies collected show a number of instances in which young women who had become widows, and possessed several children, were nevertheless able to remarry. The men they married were usually themselves widowers, or on some occasions very poor men, who could make no property contributions to the marriage.

Certain customs highlight the extent to which unrelated young people were segregated from each other in social life. One of these was the custom of having swings hung in houses on certain Sundays in winter, to which the whole village

went. Young men would sit in the swings and sing songs. Then young women did the same thing. Informants say that the excitement in this came from the opportunity to 'see' people of the other sex, although they did not speak to each other. It was apparently quite possible to have lived for fifteen or twenty years in a small compact village without an opportunity to have had a good look at an unrelated person of the opposite sex.

The following examples illustrate settlements characteristic of the pre-1930 situation and the transition period:[3]

a (C. 15) Zenon was born in 1905 In 1921 he was engaged, aged 16, to Morphini, who was 13. They married in 1926, when he was 21 and she 18. Zenon's father built them a mudbrick house, on a piece of agricultural land he had bought, near other houses. He paid £12 for the doors and windows, and Morphini's father paid £8 for the furniture. Zenon received 12 government donums of land, Morphini 10 donums. By village standards the new family had been well endowed. Both partners were from Kalo.

b. (C. 37) Andreas was born in 1904, and in 1926, aged 22 was engaged to Ourania. The go-between was Andreas' father's sister, who was married to a brother of Ourania's father Ourania was 13 at the time. In 1929, when the youngsters were 25 and 16 respectively, they married. Andreas received 9 donums, Ourania 8, but Andreas's land was encumbered by his father's debts The couple lived in the house of Andreas's parents, who themselves moved into another old house, that of Andreas's father's father. Both the youngsters were from Kalo.

c (G. 1) Evtychia was born in 1892, in Kalo village. She was rather plain. She received about 5 donums at marriage, but she married into a nearby village, to a man with 10 donums, of better land, and also a house Her elder sister Maria, and four younger brothers all married partners from Kalo.

d. (G. 5) Maria was born in Kalo in ?1874, married a Kalo man, Haralambos in 1896. She had 8 donums, he had 6. He died soon after the marriage, when she had borne him one daughter. In 1900 Maria married again, a man from the next village. She sold her land in Kalo, and moved to his village.

e. (C. 173) Nikolas was born in 1913. In 1932 he was 19 and was engaged to Sophia, then 17 years old. They were married two years later, he had no land and she only had one donum Nikolas worked as a bootmaker. At the time wages were 7½p a day, and at marriage they got from her father an old mud-brick house which was fixed up at the not inconsiderable cost of £10, half from Nikolas, half from Sophia's father Nikolas commented that his father-in-law had helped him over the house because he was poor. Sophia put it more strongly 'I was poor and had little land—that's why we offered a house'. Both were from Kalo.

IV

The arrangement of marriage in 1968

The situation in Kalo village now is that the parents of a girl usually agree during the engagement negotiations to build her a house. If she is rich, this will be built by a contractor and cost £3000, and more. If she is poor, then she and her family will supply a good deal of the unskilled labour, and will hope to keep the cash costs down to £1500 or £2000. Most girls in Kalo will also get with the house a small piece of land planted with seedling orange trees. If parents are prosperous enough to get their daughters engaged at sixteen they will do so. Poor girls may have to wait until their middle or late twenties, saving from ten or more years of wage labour enough to supply the essential house. The house is particularly and frankly requested by the parents of the boy. For poor families with daughters to marry off,

the custom is tyrannous, compelling many years of very hard work and thrift, and people complain of it in these terms, as if they are passive in the face of the situation. But a poor man who has in old age succeeded in marrying off several daughters honourably, each with her own house as custom dictates, is inclined to boast of his achievement, just like a wealthy man. It is accordingly difficult to say how the villagers see the custom. It is hardly correct to say that they approve of it; they experience its power in their social lives with deep ambivalence.

It is considered decent behaviour for a young man to take some contribution to the house, usually for certain things agreed by recent custom—the paintwork, the electrical fittings, the bathroom furnishings, and certain luxury items such as a refrigerator. He should also have several hundred pounds in his pocket towards the wedding expenses. A young man might have an outlay of £400 on a house costing £2000 or more.

It is by no means essential for a young man to own land or other property in order to get married. Young men say: 'A girl needs a house to marry, but we can get married with nothing but our trousers'. In another version they stress the actual contents of the trousers. This is not entirely flippant. When translated into cash value it is quite clear that girls bring to the marriage on average four or five times as much as boys. For the poorest families this means that a girl marries with no land, only a minimal, very cheap house, and her husband will bring nothing to the marriage except his physical strength, i.e. the power to earn a living by labouring. In traditional times a family needed both a house and some land to be viable; today a new family needs a house—following the cultural rule of neolocality—far more than it needs land, since it is now more easily possible to support a family without land. Moreover, these different contributions are calculated implicitly or explicitly by the two families during the negotiations.

By way of illustration, here are some examples of characteristic settlements in the period since 1941. All couples live in Kalo village.

a. (C. 45) Christos was born in 1918, was engaged in 1938, aged 20, to Angeliki, aged 15. They married in 1941. He got from his parents 2 donums and 24 sheep. She received 9 donums, and an old house from her parents which Christos paid £12 to modernise. (At this time a man could earn 75p for a day's labour as ploughman.) Her father also promised a donkey, but was still holding back until the hour they went to church. But finally he gave in, and both young people, each from Kalo village, received full title at the time of the marriage

b. (C. 51) In 1941, Petros, then aged 21 was working in Nicosia, when he received a letter from his brother in Kalo which told him the family had got him engaged. He was irritated, and said 'How did you know that I wasn't keen on someone else?' But he accepted 'so that my brother would not be insulted by others' The agreement was that the girl's father would give her 9 donums and a house; Petros himself, of poor family, would get nothing. She was 15 at their engagement. In the event, they did not marry until 1949 because her father could not persuade his siblings to yield the title to the building plot. Both were from Kalo.

c. (C. 34) Yannis was born in 1919; he was engaged, aged 24, in 1943, to Theodiki, aged 20. They married in 1946. He had 13 donums and she 12. He built the house on a plot from his father and it cost him £700. Her family put in about £100 of furniture and an ox. Yannis, though well off in land, was not considered a very personable man, being somewhat inarticulate and excitable. Both were from Kalo.

d. (C. 63) Polykarpos was born in Kalo in 1913. In 1947 while working in a Nicosia suburb he got engaged 'but my heart didn't warm to her' and after 3 months he broke off the engagement, paying a £200 fine at the Ecclesiastical Court. The girl was rich, and Polykarpos's

father and uncle (the go-between) were furious, but he said 'men couldn't be married off by force then', adding 'If you marry an ugly woman and then have four ugly daughters, where will your property go? No-one will want to marry them'. In 1948, aged 25 he got engaged to a Kalo girl, Zanthe, aged 23. They married two years later. He had 9 donums, she had 12. He was by village standards rather old (35), and had the handicap of the previous engagement against him. He fixed up an old house of his father at a cost of £320 Her family were not too well off at this time, and did not help.

e. (C. 68) Kostas was born in 1928, in a suburb of Nicosia. His family were very poor. In 1951, at 23 he got engaged to a girl in Kalo, Eli, aged 22. They married a year later. He had no land, she had 12 donums, and an old house from her parents, on which they spent £100 fixing up. There had been gossip about her sisters, which Kostas as an outsider, would have been unlikely to know.

f. (C. 95) Nikolas was born in 1928, engaged in 1951 aged 23 to a girl of 18. They married in 1953. He was an elementary schoolteacher, and got 9 donums from his family. She got 12 donums from hers, as well as a modern house costing £800. She came from one of the wealthier families of the village. Both were from Kalo.

g. (C. 118) Dimitris was born in 1939, engaged at 26, in 1965, to Maria, 22. Both were from Kalo, and he worked as a clerk in the capital. At the engagement their parents signed a settlement in which her father agreed to give her 7 donums, and build her a house, and Dimitris agreed to pay for the electrical system, the bathroom and the paintwork, to the sum of £500, he had no land. Her family put £1500 into the house Although the settlement agreed two years as the maximum period of engagement, in fact they ran over this by eight months, and in addition, her father was forced to mortgage her land to pay for the house. Wedding gifts to the couple from all the villagers came to £900, of which £300 remained after the costs of the wedding feasting. This Dimitris used to buy a refrigerator, a gas stove, and to put iron rails around the house.

V

Some consequences of the change in property transfer

In Kalo there are a number of immediate consequences of houses normally being provided for daughters at marriage. One is that girls are more likely to live after marriage within easy reach of their own parents and often next door to their own sisters, for actual building plots are scarce, and what was yesterday a small field must today be divided into plots for several daughters. It might be thought that in a compact village this would be unimportant, but differences which in one society seem trivial, in another are weighty. The houses furthest apart in the village are fifteen minutes' walk from each other, and those people forced to build this far from the centre complain about being 'out of things'. As a result a group of co-operating women in a neighbourhood are often also sisters or first cousins as well as neighbours.[4]

Another consequence, related to the first, is that few Kalo girls marry and reside outside the village. A few girls have houses built for them in the capital, but they are likely to be particularly wealthy or highly educated. Most marriages since 1950 involving an 'outsider' are those in which an outside man married a Kalo girl, and came to live in Kalo. This can be seen in table 3. Most of these men are poor men from poor villages. The Kalo girls tend to be those who have suffered some negative social judgement—either they are poor, previously engaged, 'spoken about' or some combination of these factors.

There is another important consequence of the present pattern, which is that the powers of parents to control the marriages of their children seem to be con-

timing, although in a modified form. Village children of both sexes (although girls less than boys) have begun to acquire a power of veto in the choice of marriage partners, and men even have the agreed right to suggest possible partners to their families, although very few girls as yet have done this.[5] However, the parents still retain very strong powers of veto, and for a child of either sex to go against strongly expressed parental wishes amounts to breaking off relations with the parents. I heard of only one case in which a girl did this. Young people are not allowed to spend any significant amount of time alone together, before agreeing to get engaged.

This situation continues in spite of secondary schooling for both sexes and the intrusion of the mass media, often employing British or American material. If the romantic love complex has won few open supporters among the young, it is not that they are indifferent. Young men often complained to me of the weight of tradition in this matter. Parental control over courtship appears to be changing more slowly than in other Christian Mediterranean countries (e.g. Boissevain 1969: 26–40) and this is because parents as arbiters and guardians of property enjoy strong power, as well as that authority they have from being a collectivity forming adult public opinion in the village. This power is described by both children and parents in terms of 'respect' but the fact is that children have no legal claim to a marriage portion while their parents are alive. Girls are in no position to go against their parents' wishes over marriage partners, since the ultimate sanction would be for the parents to refuse the crucial dowry house.[6] Girls being so constrained, it makes little difference what the boys feel about a more open form of courtship.

It was not possible to establish any more general changes in rights and duties among kin or affines, produced by the duty of parents to provide a house for their daughters. After marriage, a young man forms strong relations of co-operation with his wife's father, and wife's brothers, if the engagement has gone smoothly, and the building of the house has not caused too much open conflict. Such conflict in any case tends to be between the two sets of parents, rather than between the groom and the bride's parents. Nor can one say if, when houses were provided for sons, there was less co-operation between a man and his wife's father. In those cases where a woman moved to her husband's village at marriage, her husband would have had more opportunity of co-operating with his own father; this would also have been the case if his own father was living in the same courtyard.

One recent change in engagement customs is that during the years of betrothal, the girl's family expect the fiancé to dine with them virtually every night. They feel the need to supply better than normal food and drink for him. This period allows the girl's family to get to know the groom, but what is remarkable is that the couple do not dine nearly so frequently in the boy's home. Informants suggested that avoidance between the girl and her future mother-in-law might play some part, but they more commonly suggested that it is in the interest of the girl's family to 'keep the groom happy' by good food, and by allowing him to spend time in the company of his fiancée. The sense of their comments was that a young man who was not satisfied would threaten to break off the engagement, thus leaving the girl in a very difficult position, which it would normally require extra dowry, or an inferior groom, to put right.

When the groom is from another village, the custom is even more strikingly

lopsided, for not only does he dine every night with the girl's family, but sleeps at their home too. It is not regarded as part of this situation that he should manage to sleep with the girl, but young men said that in such a situation it was inevitable that the couple would find opportunities and that if they were discreet the parents would turn a blind eye to it. This change may have been produced by the general relaxation of female seclusion, rather than as a direct consequence of the dowry house coming from the girl. But obviously the need to 'keep the groom happy' is greater when girls find it hard to marry young, than when they found it easy to do so.

VI

The major cause of change: numbers of men and women available at age of marriage

During fieldwork, when enough data had been collected to suggest that the change in property transfer just outlined had actually taken place, it seemed natural to ask the villagers themselves why they thought it had happened. The younger ones were vague, and often unaware that customs had once been different. But older men and women said 'It's this way: men have got fewer. Once men were cheap, but then they got fewer and got more expensive'. In terms of the argument of this section, their answer is correct, though couched in a different idiom; so I am going to explain in a different way what the villagers know very well already.[7]

From the evidence available it appears that in Cyprus generally men have indeed 'got fewer' in some way. Table 5 (p. 521) shows a marked increase in the percentage of women who have never married aged 20–24 and 25–34 in census years 1921 and 1931 but no corresponding changes in percentages of men who have never married. In the appendix, Langford goes into more detail in interpreting and qualifying these and other data. For my purposes it is enough to use these figures to suggest that during a critical transitional period women were experiencing difficulty in marrying as early as they had previously done. It is very probable that the chief reason for this was emigration of males from Cyprus, but that is beyond the scope of this article.

For such a general change in numbers of men and women available at marriage to have the specific consequence of a change in patterns of property transfer, other cultural factors assumed to be operating continuously as constants must be mentioned. One is that there is no socially approved role for celibates of either sex: only a few positions in the higher ranks of the Church exist for celibate men, and the great majority of the male clergy—the village priests and parochial clergy—are married before being ordained. Very few women enter convents, and this is not considered by villagers as an honourable solution to the problem of an unmarried daughter. Another factor is that *early* marriage continues to be desired for children of both sexes, but particularly for girls, since family honour is deeply involved with the unblemished pre-marital chastity of women.

My argument may now be rephrased: when before 1921 there were more men than women available at age of marriage they were at a disadvantage in relation to women, if the assumptions just mentioned were operating social constraints, and the fact that they tended to provide the marital house is to be understood as an

inducement to women to marry them. When after this time there began to be more women than men available at age of marriage, they in turn came to be the disadvantaged sex in a bargaining situation, and after a transitional period of uncertainty came to offer houses at marriage to induce men to marry them 'early'. This change did not take place uniformly all over the island at the same time, and I wish to stress that the discussion is always of central tendencies, rather than of all marriages, or of particular marriages.

One point needs special emphasis: individual villages cannot be regarded as closed systems for the purpose of calculating numbers of men and women available for marriage. There is some reason to think that clusters of adjacent villages form *relatively* closed selection pools. Most marriages will probably be within this area, and numbers should fall off sharply as the distance from the home village increases. This of course is a commonplace of mate-selection studies. The island itself is not properly a closed system either, although it can for heuristic reasons be treated as one. In fact, the effect of a shortage of marriageable men at the national level is experienced differently in different villages. Some favoured places (towns and rich villages) will have surplus unmarried males, some especially ill-favoured places will have a much greater dearth of unmarried males than the national average, and some transitional zones will have local males moving away, and poorer stranger males taking their place. The fact that in different places the 'demand' for husbands might be greater would encourage very poor men to seek for wives offering houses in such places, and an economist with adequate large-scale data could put this argument in formal terms using market analysis.[8]

VII

The mechanics of the change at the level of the individual

Up till now the argument has been put in terms of trends in national census data, and is in the form characterised by Cohen (1969: 26) and Rosenfeld (n.d.) as macro-sociological. This section seeks to suggest how individuals responded to specific situations, and how over time their aggregate decisions may produce the sort of patterns suggested.

On the basis of discussion with elderly informants, analysis of genealogies and village household censuses, the actual change in house provision can be reconstructed: the first girls to experience difficulty in getting married were the poorest or those handicapped physically or socially. The difficulty was experienced by the fact that they failed to get married as young as they would have liked, while other girls succeeded. As demographic pressure against women began to mount, in the period 1920 onwards more and more girls (and not only the poorest ones) found it hard to marry as young as custom approved. For a long time, between 1930 and 1950, there were two sets of 'custom' about house provision operating (some families built for their daughters, others for their sons), but finally, as pressure against women as a sex persisted, one pattern came to be the dominant one. The logic of this situation may become clearer to the reader unfamiliar with the market-like character of marriage arrangement in many peasant societies, when some further description is given of the process observed in 1968.

In Kalo village, and those surrounding it there is a period when nearly all engage-

ments take place, in the three months from December to February. The effect of this special season is something like the effect of opening and closing hours on the Stock Exchange—it allows a period when something nearing perfect information is to be had by all families who wish to make decisions. Everyone knows roughly when the bidding will start and when it will close. Those who feel it urgent to arrange a match this year are able to adjust their sights accordingly. Those who are in no hurry and are seeking to make a superior match will feel they have time to turn down mediocre offers. Those who have their corporate mind on a particular alliance can send out gentle feelers. The existence of a *conventional* season focuses attention, forces decisions, and keeps information and manoeuvres manageable.[9]

A refusal even when made through a third party may give offence and it is part of the political culture of the village that one avoids offence and seeks to keep all options open. Thus the form of a refusal is often 'we are not getting our daughter engaged this year. She's a bit young.' The other reason for not accepting an offer this year is 'we haven't the means yet', which is usually a reference to the means to build a house. However, once such a reason has been given a family that wishes to preserve a reputation for straight dealing and wishes to remain on good terms with the people who opened the bidding will stick to its word. To accept in the same season another offer is to risk long-smouldering hostility from the rejected party, whose status has been slighted and strategies thwarted. A family that has waited for three or four seasons, and has received no offer from within the village will start to look for a suitable partner outside the village. To be safe, family members will tend to look in a poorer village, or for a man poorer than themselves.

The whole period of negotiations and all aspects of them are conducted with great secrecy, and are the subject of continuous speculation and gossip. People remember twenty years later who asked for whose daughter and was turned down. Part of this is undoubtedly a preoccupation with relative status, for the developmental cycle, focusing on the transfer of property at marriage, tends to link the status of a man less to his own direct achievements in early life than to those indirectly reflected in his mature years by the marriages of his children. From the village point of view the marriages a man arranges for his children *are* his achievements, but in any case it is these marriages which will determine his own final status.

During the actual bargaining over engagement, the relative contributions to the cost of house construction are important. There is a feeling that a completely instrumental approach to the matter is uncouth, and that the atmosphere in which a young man sends emissaries to ask for the hand of a young woman ought to suggest that his initial motive is attraction to her. This affects the bargaining in the following way: the father of the girl can hint more or less openly that since it is his daughter and not her property the young man really wants, the young man should agree to make a substantial contribution to the costs of the house. This, I believe, is often a last-minute manoeuvre, when the other side has openly committed itself to the match. Negotiations have been known to break down at this point and on this issue.

To conclude this section it is worth stressing that the house agreed on during negotiations means something different to families at different levels of wealth. The very poorest families offer houses to make sure their daughters get *any husband*

at all, even a poor man from a poor village far away.[10] A little higher up the scale of wealth and prestige, the value of the house is that it brings a groom from among the poor but honourable young men *from within the village*, which villagers prefer because they know the true standing of the other family, and the married couple will be near both parental families. A little higher still, the house (which is also increasing in value) attracts one of the *better placed* young men in the village. At the top of the scale the offer of a house has nothing to do with such mundane problems as getting a groom, or a groom from within the village. It is the appropriate portion of a young girl who will receive offers from the *best placed* young men, who have property or education or both. The girl's father may later boast 'Has any other father in this village built his daughter a house like I built mine?'[11]

VIII

Other factors involved in the change

Most of this article has been concerned with a single causal factor in the change in a particular custom of property transfer. There are a number of other weaker causal factors involved which can be discussed more briefly. None of them is either necessary or sufficient, however. First among them is the decline in the importance for villagers of agricultural land as the essential basis for a new domestic group. Table 3 suggests how for one village in the period discussed the number of household heads who are earning the main part of their income from non-agricultural activities has increased and table 4 gives some notion of the national trend. In the traditional village, some land was essential for household viability, and those who were without land seem most often to have left the village. Whereas a man could build his house with mud bricks and his own labour, land was something he either had or did not have. The increasing integration of the village into the national economy, by providing a large number of new jobs, allows families without land or with very small holdings to continue to live in the village. Thus the relative importance of land as the foundation for the domestic group has diminished. But the importance of a house has not diminished: in comparison with land provision of a house has become a more important contribution to a new marriage.

A poor man brings to his marriage the potential to earn a living through wage-labour for thirty or forty years. The lowest labouring wage for men is £1 per day, so estimating 300 working days a year at this rate a man may be thought to bring to his marriage a potential earning power of £9000 over 30 years, barring illness. Women are not expected to contribute by wage-labouring—although they frequently do—since their proper roles are child-bearing and domestic duties. The woman can be said to provide the marital house as an 'advance' on her economic contribution to the marriage but more particularly, when she is disadvantaged by the need to marry young and the scarcity of husbands, her house is an incentive to marry her *now*, and it balances in the bilateral system of property transfer the future economic contribution of the husband over his working lifetime. Both the house and the man's ability to earn are essential to the long-term viability of the domestic group, but in the context of Cypriot culture the house is the more pressing need because a couple 'should' start married life as a separate economic and residential unit if possible. Of course one cannot do a precise sum in which a house worth

£3000 today is the same as £9000 earned by a man during 30 years, and yet this is in a sense the sort of arithmetic involved and villagers sometimes make this sort of calculation.[12]

A second factor, while it could not by itself account for the switch in who provides the house, undoubtedly intensifies the interest in the quality of the house itself. This is the continual and dramatic rise in living standards enjoyed by villagers since 1940. Old men tend to express this in terms of having enough to eat, and particularly in the frequency of meat-eating: 'Before the war we were hungry. We worked from sunrise to sunset for a few copper coins to buy bread. We ate meat once or twice a year.' Now it is a disgrace not to eat meat at least once a week; and wealthy households in the village eat meat every day. Mud houses have given way to brick ones; pit lavatories to porcelain ones; wood fires to gas fires; oil lamps to electricity, and so forth. Children are unlikely to die, to have lice, or grow up illiterate. These and a host of other changes have occurred in the last thirty years in the village. Most of them directly relate to the house and its contents, and are closely associated with women's labour for obvious reasons.

The rise in living standards does not account for the specific switch in house provision. But once women start providing houses it is symbolically appropriate that competition for status be expressed through the luxuriousness of the house. The present and future living-standard of the bride is symbolised by her house: it is she after all who gets most practical benefit out of labour-saving devices and tiled, rather than mud-floored kitchens, although she passes this benefit on to her husband in her cooking and child care. If it were the custom for men to provide houses, one would expect the contents of the house and its level of luxury to be less important, and competition for status to be expressed more in things like motor cars, the man's education and his other personal property.

There is a third factor which contributes to the cultural importance of women providing houses. The position of women is changing throughout Cyprus in certain ways. More women receive secondary education than previously; they are less secluded, and through their earnings may make an important cash contribution to their households. There are signs that women are also attaining a degree of independence of family ties. One example is their recent acquisition of a power of veto in the arrangement of marriage. A girl, unless very poor or otherwise disfavoured, is now generally recognised to have the right not to be made to marry a man she does not want, although if her family think the match is very desirable they will try hard to get her to accept it. This power of veto is a recent acquisition, according to my elderly informants of both sexes.

The altered position of women is reflected in and complemented by their provision of a house at marriage. The actual provision is an immense economic burden, and one over which fathers and daughters alike bitterly complain, but once the house is built it gives a measure of security to the woman. For it will be in *her* name, and in the unlikely event of the marriage breaking up, it will be the man who must leave the house and return to his family.[13] The woman no longer needs to return to her family and in this special sense she is more her own mistress. In traditional times when men owned houses it would presumably have been women who left the marital home in the event of serious disagreements. Even without the special case of marital breakdown (which appears in the formal sense

to be rare in rural Cyprus) it is reasonable to assume that knowing she owns a house gives a woman a certain sense of security.

IX

Conclusion

An argument has been put forward about why a certain observed change took place; from the data it cannot be shown that an island-wide change occurred in the same way it occurred in one village, but it is plausible and reasonable to suppose it did so occur, until evidence to the contrary or a better explanation of the change are produced. It should be noted that the change is highly specific and any competing explanation will need to be fairly specific. A simple appeal to 'increased wealth' or 'increased female education' or 'an improvement in the jural status of women' or 'declining importance of agricultural land' is too general to explain why the vital house *changed sides.*[14]

This change is by no means a trivial one, and similar sorts of change have been reported widely. Friedl (1962; 1963) suggests that dowering in mainland Greece might have important consequences for the flow of capital from rural to urban areas; from all the information available the need to give houses to daughters is equally strongly felt by urban Cypriots and a great deal of Cypriot capital is taken up in the exigent or sumptuous construction of these houses. As Surridge noted forty years ago (1930: 25) the dowry house competes for scarce agricultural capital investment. One Kalo woman remarked to me 'These houses are eating up our fields . . .', and she might have been speaking for the Cypriot economy as a whole.

There are signs that some young people are struggling against custom, and in towns are buying flats through mortgage systems. But before such moves are widely emulated it is likely that many people will suffer very great hardship to provide for their daughters. There is a very obvious role for government initiatives by providing cheap public housing and low interest loans for buying houses. So far too little has been done by government in this area.

Langford's discussion of the demographic issues and the difficulties of using census material show that while social anthropologists are unlikely to become adept at the analysis of such data they are not justified in refusing to come to grips with the problems presented. It is not enough to say in an anthropological analysis that demographic factors have played some part in causing a situation, when data exist which can be used to refine such statements. As Homans has reminded us (1967: 18) if we are not willing to make statements on the lines of 'the more the x, the less the y . . .' then the power of our explanations is drastically reduced; we must, he insists, stand and deliver.

Changes in the size or composition of marriage payments are commonplace in African, Indian and Melanesian ethnographies. Structural changes of the kind described here are less common, but there are a number of instances from Mediterranean data, with a variety of causal factors suggested. Campbell (1964: 302) for the Sarakatsani, Davis for the South Italian town of Pisticci (1973: 73–91) and Lineton (1971) for the Mani district of Greece describe changes which are in some ways comparable to those described here. Gillian Webster (1967) has drawn attention to changes in Serbia from bridewealth to dowry.

A number of authors have assumed that changes in the pool of eligible marriage

partners have had direct consequences for marriage payments. Habakkuk (1950: 24) discussed 'an increase in the price of husbands' among landed families in eighteenth-century Britain. Ernestine Friedl, discussing the attempts of the wealthier Vasilika girls to get urban husbands of higher status, makes similar assumptions. This article has tried to make somewhat more explicit the kinds of factors which need be considered before such assumptions are fully justified, and has shown which ones have shaped the Cypriot solution to a set of conflicting structural principles. These factors are the persistence of preferred early marriage for girls to safeguard family honour; a lack of approved roles for celibate women; economic changes which affect the materials used in building houses, as well as making a house a more important contribution to a new marriage than agricultural land; the local migration of eligible local males and their replacement by less eligible stranger males. To all these must be added a shift in numbers of males available at marriage, from a situation which traditionally favoured women, to one which increasingly favoured men; this last factor is in my view the critical one in the Cypriot case.

This article has been following up an analytic approach developed by Firth and Barth which attempts in the explanation of social change to relate individual decisions to changes in collectively-held values, while taking proper account of economic and cultural constraints. This is *not* to say that individuals have limitless choices, for in a sense they can only choose what their culture allows them to see.[15]

NOTES

Versions of this article have been given in a number of anthropology seminars, and I am grateful to the participants for many useful points. These must include the LSE, UCL, SOAS, and Kent and Cambridge universities. Particularly useful have been comments from Alan Macfarlane, Caroline Ifeka, Michael Attalides, Pamela Constantinides, and George Dalton. Research funds were supplied by the SSRC to whom thanks are due. I would also like to acknowledge the encouragement and stimulation of Professor William Goode, who while I was in the field commented promptly, at length and very much to the point, on problems raised by my data.

[1] Throughout this article the present tense refers to conditions in 1968 and 1969. This is perhaps less excusable than usual, since in August 1974 the inhabitants of Kalo, like many other Cypriots, became refugees following the coup against President Makarios, and the invasion by Turkey. As this article goes to press, these villagers are still refugees, and their circumstances in no way resemble those described in this paper

[2] There is no law or custom limiting transmission of either land or houses exclusively to one sex.

[3] Numbers in brackets on these examples are fieldwork census numbers, retained here for future identification.

[4] Davis (1969: 74) reports a similar pattern in the South Italian town of Pisticci.

[5] For a useful discussion of variations in authority of parents over children, see Stone (1961) and Goode (1961).

[6] When a girl can expect no help from her parents, for reasons of their poverty, she gains an ambiguous freedom to decide for herself. She needs normally to have some special personal qualities to persuade a young man to ask for her hand, knowing he will get her 'with only her skirt'. Some poor parents try to influence a girl by a beating.

[7] After asking the villagers I turned to the 1946 Census whose author offered a similar explanation although based on systematic data:

> Evidently up to 1931 or a little later the incidence of marriage was declining rather sharply among women while remaining fairly steady among men; by 1946 there had come a sharp change towards earlier marriage for both sexes. . . The explanation is perhaps as follows: in the early years of the century there was a shortage of girls and they were sought for and married young. . . . Thereafter there has been a shortage of men, and this, together with the generally easier circumstances, had made for a higher marriage rate.

This statement was helpful and encouraged me to seek comment from a demographer, who was first sceptical and only after much painstaking work was ready to allow that the crucial change

in numbers of men and women at age of marriage had taken place. Oakley (1971) discusses Cypriot migration to Britain, a subject which clearly bears on the general problem discussed below.

[8] I am grateful to Miss Rita T. Hayes for taking the trouble to make this point in writing.

[9] It is, of course, a slack period in farming when men have the leisure to sit in the coffee-shops and their wives are at home rather than in the fields. Also, during most of this period there is no major Church fast, so good food, particularly meat, may be eaten to celebrate an engagement, and close friends invited to the *trapézia*, feasting tables. I suspect the existence of this period is also an unplanned outcome of the sheer intensity of social competition for the best partners: once the first family announces an engagement others follow quickly so as not to be left with the poorest partners of the season. In villages with a higher proportion of marriages to outsiders, one would not expect to find a closed season.

[10] Girls in poor and remote villages must earn enough money to attract a husband to their village, and this will mean many years of saving, and a relatively late marriage. They may try their fortunes in the towns if they have kin to live with. It would be morally unacceptable for them to live alone in the towns.

[11] It is commonly observed that marriage occurs later when economic and social conditions improve and it might be argued that increased education, first for men, and later for girls too, would account for the changes in numbers of men and women available at marriage in specific age groups Such an argument does not manage to explain why the percentages of women never married returns to the direction of the 1881–1911 trend for age groups 15–19, 20–24, in 1946 and 1960 At certain periods extra men may have been taken out of the marriage market by continuing their education, but this, I argue, would always tend to work against women, and is in any case hidden by the general fact of decrease in men available at age of marriage.

[12] W. A. P. Manser, in a letter to *The Times*, 27/5/72, performed an economic analysis of the relative capital values of husband and wife in a middle-class British situation. His conclusion was that marrying for capital investment and returns was an uneconomic proposition. I understand that recent legislation has changed this.

[13] An emergent custom which reflects the new situation is that of the 'groom's bed'. In addition to the bridal bed some new houses also have a small truckle bed in another room, which is sometimes called the groom's bed I was told by several people that should the husband and wife quarrel the husband could sleep in this bed, without the public act of leaving the house to spend the night at his parents The notion was regarded as funny by those who told me about it, but it is perhaps suggestive.

[14] Professor Goody has suggested (personal communication) that if men migrate there is more property left for women to receive, and that this will include land and houses. This is logically true. It does not explain why this 'extra' property should not simply have been given to the remaining men. Also, if men have migrated, then they are not locally available for marriage, and hence their absence *directly* affects the marriage market without being transformed into extra property available to women Laura Nader (1964) has described Mexican Indians who seem to have recently experienced a comparable change in house provision. She explains this in terms of state and church encouragement of later marriage, and pressure from increasing population 'directly threatening the existence of a bilateral inheritance system' (1964: 230). To this she adds a preference for preserving bilateral inheritance. I am grateful to Jean La Fontaine for this reference

[15] Attalides (1976) has interesting material on urban dowry arrangements, and other ideas relevant to this article, but unfortunately his paper reached me too late for consideration here.

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APPENDIX: SOME EVIDENCE FROM THE CENSUSES OF CYPRUS

CHRISTOPHER LANGFORD

Introduction

The first modern census of Cyprus was carried out in 1881. There have been seven censuses since then, those of 1891, 1901, 1911, 1921, 1931, 1946 and 1960. All of them appear to suffer, albeit in varying degrees, from incomplete coverage as well as from errors of reporting.[1] There are, in addition, quite frequent, although mostly small, numerical inconsistencies within and between tables. It will be argued, however, that the data available from these censuses are adequate for present purposes. And all the measures presented here have been computed directly from the census data without any kind of smoothing or adjustment.

Two census tables form the basis of the analysis presented, one showing the population by age-group and sex, and the other, the population by marital status, age-group and sex. From these tables are derived the various 'sex-ratio' data shown below, and the data on 'proportions never married'. Two points should be noted: first, that data on marital status are *not available* (NA) for 1881; second, that, although the age-sex distribution of the population is provided, in every case, in *five-year* age-groups, the age-groups for which marital status is provided (prior to 1946, anyway, when more detailed information does become available) are 15–19, 20–24, 25–34, 35–44—etc.

It will be seen that the analysis presented relates to the whole population of all Cyprus, whereas Loizos's fieldwork covered Greeks in a single village. The question of how far the general situation in all geographical Cyprus may be important for people's behaviour in one village is a matter for him. (Though it is worth noting that, while the analysis presented *cannot* be carried out, in its entirety, for any area smaller than the whole of Cyprus, some parts of it can, and these show up nothing in the vicinity of Loizos's village[2] which is *inconsistent with* the all-Cyprus picture.) So far as the matter of Greeks and Turks is concerned, while the census data shown relate to a population of both together (and a few others), it simply not being possible to carry out this analysis for the two groups separately, they can reasonably

be thought of as relating to Greeks in this present endeavour. At every census date the vast majority of the population was Greek, and the data must reflect this. Moreover, the main point of the analysis is not so much to establish an accurate description of Greeks at any particular point in time as to detect changes in their behaviour and characteristics over time; and it is obviously extremely unlikely given their position of numerical predominance that changes observed in the whole population will not be due substantially to changes among Greeks. (The proportion of the Cyprus population recorded as Greek Orthodox has been fairly stable over time, rising from about 74 to about 80 per cent. between 1881 and 1946, and dropping back to about 77 per cent. in 1960.)

Ages at marriage and proportions ultimately marrying

Census data provide the only means of examining age at marriage in Cyprus before the 1960's. Although there has been a system of marriage registration since early this Century, the first year for which data on age at marriage are available is 1963.

The discussion which follows is based upon the data of table 5. These are undoubtedly of great value in the present connection but it must be noted that they rarely permit precise conclusions. In 1931, to take a specific instance, some 90 per cent. of Cyprus males aged 35–44 had been married. But it is not possible, even by taking into account the situation of this group ten and twenty years before, to be very specific about *when they had married.* Whether their marriages had been heavily concentrated at particular ages or had taken place more evenly over a wider age-interval, for example, is open to reasonable doubt. Every effort will be made to keep in mind the limitations of the data in what follows. It must be recognised at the outset, however, that a speculative element is necessarily involved.

TABLE 5. Men and women who have never married, as percentages of their age-groups (all Cyprus)[3].

Males Age-group	*1881*	*1891*	*1901*	*1911*	*1921*	*1931*	*1946*	*1960*
15–19	NA	98·6	99·2	99·2	99·1	99·2	98·1	98·5
20–24	NA	83·6	83·4	83·5	85·0	82·9	74·4	64·1
25–34	NA	36·5	32·7	32·1	35·5	33·4	28·9	18·5
35–44	NA	10·8	9·3	9·7	10·7	10·2	8·7	5·1
Females Age-group	*1881*	*1891*	*1901*	*1911*	*1921*	*1931*	*1946*	*1960*
15–19	NA	87·6	88·7	89·0	91·0	91·5	88·6	90·6
20–24	NA	43·1	45·1	44·3	54·3	57·4	52·4	43·9
25–34	NA	12·2	11·1	10·9	14·6	17·7	19·8	14·0
35–44	NA	4·7	5·7	5·1	5·4	4·6	6·7	6·1

The data of table 5 indicate that quite a stable pattern of marriage operated for Cyprus males from the end of the nineteenth century until at least the early 1930's. There was almost no marriage before age twenty and little in the early twenties.

Most men married in their middle or late twenties, or in their early thirties. Not more than 11 per cent., and very probably rather fewer, never married at all. By 1960, however, the situation had changed: the progressively smaller proportions of men never married in the age-groups 20–24 and 25–34, in census years after 1931, indicate a pronounced shift in the direction of younger marriage. The drop in the proportions never married in the 35–44 age-group suggests that there may have been, in addition, a movement in the direction of higher proportions ultimately marrying.

Early on, there was a stable period for Cyprus females too, but this came to an end rather sooner than for the men, some time between 1911 and 1921. The pattern operating during this period involved lower ages at marriage for women than those indicated above for men, marriages before age twenty and in the early twenties being of considerably greater significance. After 1911, *two* movements may be discerned: first, as evidenced by the data for 1921 and 1931, there was an *upward* shift in age at marriage for women; second (from the data for 1946 and 1960) there was a *reversal* of this change, until, by 1960, the situation was not unlike that of 1911 again. There is no evidence of a change at any stage in the proportions of women ultimately marrying: the proportions never married in the 35–44 age-group remain at the same, very low, level throughout.

Relative numbers of men and women of marriageable age

Given the data of table 5 there is the strong suggestion that, after 1911, there were relatively fewer men of marriageable age in Cyprus than before: so long as men adhered to their traditional age-proportional marriage pattern, which they did during the 1920's and into the 1930's, the female response was delayed marriage; only when the male pattern altered in the direction of earlier (and possibly more) marriage, more males thereby becoming available, were women able to marry younger again.

There is *direct* evidence in the data of table 6. These indicate that, from 1921 on,

TABLE 6 Relative numbers of men and women of marriageable age in Cyprus.

Ratio		*1881*	*1891*	*1901*	*1911*	*1921*	*1931*	*1946*	*1960*
Females 15–34 / Males 20–44	Actual	0·89	0·96	0·95	0·95	1·02	1·08	0·99	1·02
	1911=100	94	101	100	100	108	114	105	107
Females 15–34 / Males 25–44	Actual	1·10	1·24	1·24	1·21	1·35	1·46	1·33	1·37
	1911=100	91	102	102	100	112	120	110	113

there *were* relatively fewer men of marriageable age in Cyprus than before. The shift is clearly discernible when all men and women of marriageable age are considered; it is more marked still when men in their early twenties are excluded.

It is extremely unlikely that this shift could be due to a change in the pattern of age-misreporting after 1911: for this to be the case there ought to be compensating sex-ratio changes in younger and/or older age-groups; and there is no indication of such changes in table 7.

TABLE 7. Sex-ratios (females/males) for certain age-groups, Cyprus 1901, 1911, 1921, 1931.

	Age-group							
	0–9	*10–14*	*15–19*	*20–29*	*30–39*	*40–49*	*50–59*	*60+*
1901	0·95	0·91	1·01	1·03	0·95	0·94	0·98	0·90
1911	0·96	0·93	1·01	1·08	0·94	0·92	0·97	0·89
1921	0·96	0·96	1·03	1·11	1·00	0·95	0·94	0·96
1931	0·97	0·94	1·06	1·13	1·03	1·03	0·98	0·98

Conclusion

In the late nineteenth century, and early this century, there was in Cyprus a period of stability so far as patterns of marriage were concerned. But a shift in the balance between the sexes, in the female direction, after 1911[4], meant that the old patterns of marriage could no longer be supported. Initially there was a movement in the direction of later female marriage; subsequently a reversal of this movement occurred, along with a shift towards earlier (and possibly more) marriage for males. What must be made absolutely clear, though, is that, although the sex-ratio shift after 1911 was such as to necessitate a change of *some* kind, earlier/more marriage for men and later/less marriage for women were equally suitable *demographic* solutions to the problem. And it is up to Loizos to account for that particular series of changes which actually did occur.

NOTES TO APPENDIX

[1] Even simple inspection suggests, for example, that there is omission of young children and misreporting of ages.

[2] Nicosia District, that one of the six Districts of Cyprus which contains Loizos's village, has been investigated; and Nicosia District less the Nahieh (sub-District division) of Deyirmenlik, which is, effectively, Nicosia District without Nicosia Town itself.

[3] All the measures presented, in this and other tables, have been computed from data provided in the Reports of the various censuses. It should be noted, in connection with table 5, that the 'proportions never married' have been computed from the census table showing the population by marital status, age-group and sex. In the case of 1931, had these 'proportions' been computed on the basis of denominators taken from the census table showing the population by age-group and sex, the results would have been slightly different from those shown. This is because there are inconsistencies between the two census tables.

[4] Overseas migration may well have been the most important factor in this.

EAST AFRICAN FREQUENCIES IN SOME LESSER KNOWN BLOOD GROUP SYSTEMS

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In a sample of blood donors, predominantly Lacustrian Bantu, from Kampala, phenotype and gene frequencies are given for the Rhesus, MNS, Henshaw, P, Duffy, and Sutter systems. The results are compared with the other limited information available on these little-known blood groups in African populations.

I

While there have been several extensive studies of the frequency distribution in east Africa of the ABO blood groups (e.g. Elsdon-Dew 1939; Rife 1952; Foy *et al* 1954; Roberts & Smith 1957; Ssebabi & Nzaro 1974), knowledge of blood groups other than ABO is more limited (Ikin *et al.* 1952; Allison *et al.* 1952, 1953; Roberts *et al.* 1955; Allbrook *et al.* 1965) although some information is available from adjacent regions of Africa (Bat Miriam 1962; Ikin & Mourant 1962; Fraser *et al.* 1966, El Hassan *et al.* 1968; Harrison *et al.* 1969; Mourant *et al.* 1974; Cavalli-Sforza *et al.* 1969). It appeared worthwhile therefore to put on record frequencies of blood groups in several systems in a sample of African blood donors in Kampala. The residents of Kampala include many drawn from populations throughout Uganda, who are attracted from all over the country to the capital for educational, economic or other reasons, so that the sample itself is tribally heterogeneous. The majority of subjects were Lacustrian Bantu (Ganda 55, Kiga 11, Soga 11, Toro 8, Rwandā 8, Ankole 8, others 23), and there were only 14 east Africans of other ethnic affinity. Until such time as tests can be carried out on a more intensive and localised basis, the present results may be regarded as indicative of gene frequencies in this part of Africa, and particularly of Lacustrian Bantu generally.

There were 138 subjects available for examination From the limited amount of antisera available it was not possible to test every individual for all systems, and the numbers examined are given in the tables of results. In the Rhesus system (tables 1 and 2), the tests were carried out with anti-C, c, D, E, e, those that were D negative were tested with anti-D^u, while the majority (112 specimens) were also tested with anti-V and anti-VS. In the MNS system (table 3) specimens were tested with anti-M, anti-N, anti-S and s, and also with anti-He. In the P system specimens were tested with anti-P_1, in the Duffy system with anti-Fy^a and anti-Fy^b, in the Sutter system with anti-Js^a and anti-Js^b (table 4). They were also tested against anti-Le^a and anti-Le^b, but since secretor status was not examined, gene frequencies could not be calculated; in all other systems gene frequencies were calculated by the methods set out in Mourant (1954).

Man (N.S.) 10, 524–529.

TABLE 1. *Rhesus frequencies.*

Phenotype	*Number observed*	*Frequency observed*	*Frequency expected*	*Number expected*
ccDee	113	·8188	·7980	110·1
ccDEe	11	·0797	·1195	16·5
ccDEE	3	·0217	·0043	·6
CcDee	4	·0290	·0332	4·6
CcDEe	1	·0072	·0024	·3
ccddee	4	·0290	·0283	3·9
ccD^{u}ee	2	·0145	·0141	2·0
			χ^2 = ·64	

Gene combinations.

$\overline{cde}$ = ·168
$\overline{cD^ue}$ = ·039
$\overline{cDe}$ = ·711
$\overline{CDe}$ = ·018
$\overline{cDE}$ = ·065

TABLE 2. *The V and VS antigens.*

	Number tested	*Positive*		*Negative*	
		Number	%	*Number*	%
V	112	38	33·9	74	66·1
VS	112	47	42·0	65	58·0

II

Results

The results of the MNS system are set out in table 3. Considering the MN genes alone, the frequencies in the present sample are very similar to others reported from east Africa, with the M gene frequency at 57·6% being perhaps a little higher than the previous Hima and Amba samples, and perhaps a little lower than Iraqw. But it is in the MNS combinations that the present sample is of particular interest. The first series from east Africa tested with anti-S and anti-s (the Karamojong, Allbrook *et al.* 1965) showed the presence of S–s– individuals, allowing the silent S^u gene to be detected. The African affinity of this gene was demonstrated by its discovery in American negroes, to whom almost all more recent samples have related. The present finding of the S–s– phenotype at just over 4% is very similar to the Karamojong 5·5%, higher than those observed in American negroes (e.g. Francis & Hatcher 1966) as would be expected in view of the hybridisation that has occurred in the New World, giving a frequency of the MS^u gene combination similar to that of MS, and of NS^u to NS. Only the frequencies of MS and NS may be compared with earlier samples where anti-S alone was employed; at 11·9 and 8·5% respectively they are very near the overall African frequencies, rather higher than in west Africa and quite similar to others from east Africa.

The Rhesus gene C in the present sample occurs at a frequency (1·8%) that is rather low by comparison with other east Africans, and is nearer the northern

TABLE 3. *The MNSs and Henshaw blood groups*

	Number observed	*Observed frequency*	*Expected frequency*	*Expected number*	*Henshaw + No. observed*
MMSS	4	·029	·041	5·7	–
MMSs	15	·109	·079	10·9	1
MMss	25	·181	·197	27·2	–
MNSS	8	·058	·061	8·4	2
MNSs	17	·123	·116	16·0	4
MNss	39	·283	·290	40·0	1
NNSS	–	–	·022	3·1	–
NNSs	6	·044	·043	5·9	1
NNss	18	·130	·107	14·7	1
MMS–s–	2	·015	·014	2·0	–
MNS–s–	3	·022	·021	2·9	–
NNS–s–	1	·007	·008	1·1	1
				$\chi^2 = 5\ 43$ NS	

Gene frequency

$\overline{MS}$ = ·1161	$\overline{NS}$ = 0854
$\overline{Ms}$ = ·3401	$\overline{Ns}$ = 2501
$\overline{MS^u}$ = ·1200	$\overline{NS^u}$ = ·0883

TABLE 4 *Other blood group systems*

Blood group system	*Phenotype*	*Number*	%	*Gene frequencies*
P	P_1+	122	89·1	P_1 ·669
	P_1-	15	10·9	P_2 ·331
Duffy	Fy^a+Fy^b-	0	0	Fy^a 0
	Fy^a-Fy^b+	9	8·3	Fy^b ·043
	Fy^a+Fy^b+	0	0	Fy ·957
	Fy^a-Fy^b-	99	91 7	
Sutter	Js^a+Js^b-	0	0	
	Js^a-Js^b+	90	83·3	Js^a ·087
	Js^a+Js^b+	18	16 7	Js^b ·913
	Js^a-Js^b-	0	0	
Lewis	Le^a+Le^b-	9	8·3	
	Le^a-Le^b+	45	41·7	
	Le^a+Le^b+	0	0	
	Le^a-Le^b-	54	50·0	

Nilotes, though as with all such low frequency genes, the vagaries of sampling are liable to cause considerable variation in any given sample of restricted size such as the present. The E gene frequency at 6·5% falls in the middle of the range observed in east Africa, and so does the d gene frequency at 16·8%. As regards the Rhesus gene combinations, the high frequency of cDe (71·1%) is similar to that observed in Luo and Kikuyu, and indeed rather higher than in existing samples of Hima and Iraqw, while those of cde, CDe, cD^ue, and cDE are also at the general levels previously reported. The antigen V, or ce^s, again a predominantly African antigen like S^u, has been primarily studied in American negroes, though there are reports

from west, south and north-east Africa (De Natale *et al.* 1955; Blumberg *et al.* 1961; Shapiro 1964; Ikin & Mourant 1962; El Hassan *et al.* 1968). Results for 112 specimens in the present series are given in table 2, the frequency of V+ (34%) being quite similar to the South African Bantu figure (33%) and rather below that in the Karamojong (52%). Similarly, the antigen VS is also associated with Africans rather than Europeans, and in the present sample 42% show a positive VS phenotype.

The findings regarding the Henshaw phenotype, which occurs in 11 subjects (8·0%), help to resolve a longstanding problem of east African gene frequencies. Of the peoples in which this system has been reported, the Tswa, Hima, Karamojong and Nuer showed a phenotype frequency of 3%, the Dinka, Kunama and Beja 1%, but the Iraqw a frequency of 11·8%. The present findings indicate that the Iraqw elevated frequency is not a single aberrant result, but that there exists in east Africa a focus of higher Henshaw frequency, well above the general levels observed elsewhere in west, central and south Africa. The close linkage of the Henshaw and MNS loci is well established, so that the random distribution of He positives with respect to MS, Ms, NS and Ns, in a random cross-sectional series such as the present, is as expected.

The frequency of P_1 positives (89%) though high is nevertheless not so extreme as in other east African samples of Kikuyu (Henningsen 1950; Allison *et al.* 1952) or northern Nilotes (Roberts *et al.* 1955) and it is indeed more similar to the Bantu frequencies from south Africa published by Shapiro (1951; 1953).

There is very little information from Africa against which the Duffy frequencies can be compared. The findings in American negroes indicate the presence of a silent Fy gene, supported by the results from a small sample of west Africans, Amharas, Beja and Babinga (Sanger *et al.* 1955; El Hassan *et al.* 1968; Harrison *et al.* 1969; Cavalli-Sforza *et al.* 1969). The present series of adequate size, tested against both anti-Fy^a and anti-Fy^b, demonstrates clearly the very high frequency of the Fy gene (96%) and the low frequency of Fy^b (4%) in this sample, and the absence of Fy^a. Again, for the Lewis groups there is little African comparative material. The phenotype frequencies are quite similar to those reported from west Africa by Barnicot and Lawler (1953), but rather different from the series of Allbrook *et al.* (1965); Harrison *et al.* (1969) and Cavalli-Sforza *et al.* (1969). Similarly, there are very few comparative data on the Sutter groups of the Kell system in Africans tested against anti-Js^a and anti-Js^b. The discovery and African importance of the Js^a antigen were demonstrated in the United States, and the frequency in African populations has only been examined in the Congo and the Beja, where 16% of Bantu, 5% Pygmy and 7% of Beja were $Js^a(+)$; similarly, no Js^b frequencies have been established in Africans, and almost all studies derive from the American negro. The present Js^b gene frequency of 91·3% and the Js^a of 8·7% are slightly more extreme than the American negro frequencies, the Js^a gene frequency being very close to the Congo Bantu frequency of 8·4% (Fraser *et al.* 1966).

III

Discussion

The present sample is somewhat unsatisfactory, particularly in its small size and the heterogeneity of origin of the donors composing it. Undoubted genetic hetero-

geneity exists in east Africa. In a first comparison of the then available data on ABO frequencies (Roberts *et al.* 1955) in samples from the southern Sudan, Uganda, Kenya and northern Tanganyika, significant differences were demonstrated between statistically homogeneous ethnic clusters. Such clustering of east African peoples has been further demonstrated in the recent extensive study of Ssebabi and Nzaro (1974) of ABO blood group frequencies. Of their four ethnic groupings, the western Lacustrian Bantu (2181 subjects ranging from Nkole southwards as far as Burundi) are statistically homogeneous, so are the eastern Lacustrian Bantu (2902 subjects, including Ganda, Soga and Gishu), and the southern Nilotes (572 individuals from Acholi, Lango, Dama, Alur and Luo). But the 1032 representatives of the Nilo-Hamites (Kakwa, Lugbara, Madi, Teso, and Karamojong) were highly heterogeneous, primarily on account of Teso and Karamojong. Overall, the 19 different samples were highly heterogeneous amongst themselves, and this was primarily due to the heterogeneity among the four ethnic subdivisions, within each of which (Nilo-Hamites excepted) the member peoples tended to be very similar to each other[1].

In view of this heterogeneity, the present results should be regarded only as a general indication of the frequencies in Lacustrian Bantu. The data are certainly too few to reveal any differences between the eastern and western Lacustrian Bantu such as occur in their ABO frequencies. Nonetheless, the frequencies here reported are of great interest. They establish the frequencies in east Africa of some essentially African genetic characters, S^u, Fy, and Js^a, which have previously been inferred, or on which the existing African data were limited. It is a curious reflection that much of our knowledge of African serology derives from American negroes in whom so much intermixture has taken place, and that there has been relatively little impact of this work on studies actually in Africa. It is to be hoped that the present study will be merely the first in a continuing sequence of the peoples here described, not only on a population basis, but also of families to help elucidate some of the outstanding problems of the inheritance of these and other specifically African characteristics.

NOTES

Acknowledgment is gratefully made to Mrs V Lamb for her assistance with the calculations, to Dr A. E. Mourant for his helpful discussions of recent data and the interpretation of our results; and to Dr K. L. G Goldsmith of the Blood Group Reference Laboratory, London, and Dr C. C. Bowley of the Regional Blood Transfusion Service, Sheffield, for providing some of the rare antisera used in the study

[1] χ^2 values were as follows:

Within western Lacustrian Bantu (6 samples)	$\chi^2 = 23{\cdot}4$, d.f. 15	Not significant
,, eastern ,, ,, (3 ,,)	$\chi^2 = 7{\cdot}2$, d.f. 6	,, ,,
,, southern Nilotes (5 ,,)	$\chi^2 = 10{\cdot}7$, d.f. 8	,, ,,
,, Nilo-Hamites (5 ,,)	$\chi^2 = 77{\cdot}6$, d.f. 8	Highly ,,
Between 4 ethnic divisions	$\chi^2 = 112{\cdot}7$, d.f. 6	,, ,,
,, 19 samples	$\chi^2 = 224{\cdot}6$, d.f. 36	,, ,,

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BARGAINING FROM WEAKNESS: SPIRIT POSSESSION ON THE SOUTH KENYA COAST

ROGER GOMM

Spirit possession has often been represented as a device through which the deprived and the lowly claim attention, respect and material favours. This article accepts that this is so among the Digo of Kenya but argues that in adopting such bargaining positions, Digo women reinforce the social control system which keeps them subservient to men. Parallels are drawn between the role of the possessed among the Digo and similar roles in Western societies and it is suggested that wherever people are granted privileges and remissions on the basis of their inferiority they are granted them at a high price.

The meaning of this title is aptly explained by a quotation from Lewis (1971: 32).

> In its primary social function, peripheral possession emerges as an oblique aggressive strategy. The possessed person is ill through no fault of his own. The illness requires treatment which his (or her) master has to provide. In his state of possession the patient is a highly privileged person. He is allowed many liberties with those whom in other circumstances he is required to treat with respect Moreover however costly and inconvenient for those to whom his normal status renders him subservient, his cure is often incomplete Lapses are likely to occur whenever difficulties develop with his superiors Clearly, in this context, *possession works to help the interests of the weak and downtrodden who have otherwise few effective means to press their claims for attention and respect* (my emphasis).

Peripheral spirit possession is thus represented as a strategy (consciously adopted or not) which redresses the disadvantages of occupying a lowly status.

I

The pattern of spirit possession among the peoples of the south Kenya coast[1] is broadly similar to that described for other east and north-east African peoples.[2] The spirits involved are, to use Lewis's words, 'not morally charged with powers held to be responsible for upholding public morality by rewarding meritorious acts and punishing evil' (Lewis 1970 294). Sometimes, however, an exorcism ceremony *is* made an occasion for highlighting the moral delicts of the host and his/her kinsfolk. The biographical details of these spirits are sketchy. Most are regarded as exotic in origin, deriving from other tribes, from Arabia, India and Europe. Some are nature spirits, known to haunt particular localities, and in very rare cases the possessing spirits involved are those of ancestors who have been subverted from their moral functions for the purposes of sorcery. The generic term most commonly used for all these spirits is '*shaitani*'.

The idea of influence by a capricious spirit is a common, often half-joking explanation for a wide range of unusual personality traits; unusual greediness, spitefulness, bad-temper, moodiness, lasciviousness and so on, and is used also to

explain unusually good fortune. Used in such circumstances the spirit possession explanation may be a benign alternative to suspicions of witchcraft, for witches are known by their unpleasantness and sudden good fortune. The explanation also competes with a recognition that people are innately different from one another, and that luck, God and the ancestors play a part in human affairs. Likewise bad fortune may be explained with reference to *shaitani*, witchcraft attack, Allah, luck or the ancestors. Applied in these ways as an explanation, spirit possession does not usually lead to attempts at exorcism.

Spirits are believed to lie in wait for the unwary in lonely places, to creep into bed beside those sleeping alone, or to take possession of the dancers at weddings or funerals. Although people who dance themselves into a trance are said to be possessed by spirits, the onset of possession in this way is rarely followed by attempts at exorcism. Spirits that attack dancers are described as *shaitani ya wirani* and generally go away of their own accord. 'Serious' cases of possession, those which do lead to exorcism, are described as being caused by '*shaitani ya kitchwa*' and are said to be known in terms of a syndrome of aggressive and bizarre behaviour, aggression towards other people, towards the self, towards inanimate objects, running amok, uncontrollable writhing, self-anointing with, or eating, ashes, dirt or dung.

Thus spirit possession occupies an important role in Digo folk psychology but it is only one type of spirit possession, specified in terms of a behaviour pattern which everybody knows, that becomes the subject of therapy.

The main therapeutic apparatus[3] consists of public exorcism ceremonies conducted by *aganga* (doctors) who are experts in divination, counter-witchcraft, herbal and magical medicine as well as exorcism and whose powers are themselves held to be based on an association with a familiar spirit. While different *aganga* claim to specialise in the exorcism of different spirits, the *mganga* contracted to exorcise usually just happens to be the right expert for the job. This is in itself explained by the *mganga's* power to 'call' those spirits over whom he has some influence.

Possession is usually cross-sexual; male spirits possessing women, female spirits possessing men: the spasms, writhings and moanings associated with possession are interpreted as an indication of sexual intercourse taking place between host and spirit. Exorcism is a costly business.[4] Nearly all cases of possession leading to exorcism involve possessed women. Exorcism ceremonies are nearly always provided for women by men; by husbands for wives, by fathers, brothers and mother's brothers for unmarried women.

II

Aetiology

The sort of spirit possession which leads to exorcism is an overwhelmingly female affliction.[5] While women of all ages may be afflicted, first attacks seem to occur especially in the earlier years of marriage, and to a lesser extent during betrothal. In an earlier paper I described at length the position of women in Digo society (Gomm 1972), but this needs to be summarised here briefly.

Digo women are legal minors. Theoretically they remain under the control of men throughout their lives. In practice this means that men (and senior patrilateral kinswomen) make rules for their behaviour which they are constantly breaking. Digo women are excluded from the important businesses of Digo life; politics, religion, and arranging major economic and kinship enterprises. Taking male life as a basis of comparison we might say that in these spheres female activities are trivialised or are problematic in the sense that they breach male rules or threaten male interests. Economically while women bear the brunt of agricultural labour for domestic consumption and for cash crop production, their own ability to make money through farming is severely limited by their obligations to provide food for children, husband and his guests. Too much attention to gardening by women on their own account is inevitably regarded as a dereliction of marital duties. They are excluded also from major trading, being restricted to selling small quantities of garden produce, cooked foods, snuff, perfume sachets, thatch tiles and other low profit commodities. Because they are excluded from major economic enterprises and from the organisation of the kinship occasions which require large cash outlays, they are seen by men to have little serious need for money.

While women can and do inherit land and valuable tree-crops, they rarely inherit a controlling interest; rather they inherit a general right to share in the income from an estate managed by a trustee. Such income as is derived from the estate is apportioned according to the importance of the needs of the various heirs for money. Trustees are males: a woman's demands for cash for consumer spending are unlikely to be taken as seriously as a male coheir's demand for cash to pay brideprice, provide a wedding feast, repay a mortgage or pay tax.

The best opportunity a woman has for making money is through her sexuality. Adultery causes gossip, divorces, compensation suits and ill-feeling between men Given a political system of constant jockeying for the goodwill and support of other men, marital disruption is a threat to hard-won networks of support, and feeds the hostility between factions.

The kinship situation of the Digo woman is a fertile milieu for generating conflicts which lead to marital disruption (Gomm 1972: 109–12) and, it can be argued, for conflicts which are sometimes translated into the symptoms of spirit possession For instance first marriages for girls are usually arranged by fathers seeking to make or consolidate useful alliances. At least 20 per cent. of betrothals are ended by pre-marital pregnancy and/or elopement. Now while the Digo generally accept an elopement which breaks a betrothal as a *fait accompli* (by accepting marriage payments) they do not usually do so gladly. In extreme cases fathers may curse their daughters[6] which is tantamount to denying the relationship between them and places the girl at risk of misfortune. A folk picture of the typical suicide in Digo society is a girl unwillingly betrothed to a man whom she does not want to marry. Such suicides do not seem to be very frequent occurrences but this typification indicates a recognition of the tensions sometimes generated by arranged marriage.

Once married, a young girl who marries a young man may find herself as the most insignificant member of her husband's residential group, under the domination and vigilance of her husband's father's wives. She is given the most arduous

and menial tasks to perform for a period before she is allowed to establish an independent kitchen and she should bear abuse and gossip without rebut. As the sole wife of her husband a woman sooner or later faces the prospect of having to accept a co-wife, and in the households of established polygynists the squabbles of envious co-wives are notorious. Husband and wife relationships offer plenty of opportunity for discord which is frequently phrased in terms of the wife's unbecoming behaviour, or the husband's financial meanness towards his wife.

Digo women are not well socialised for subjection. On the one hand, contemporary Digo society has grown out of a social order in which women had considerably more freedom and autonomy (Kayamba 1932; 1947). On the other hand, young women tune into local radio stations which purvey both a pop-culture stressing romantic love, and propaganda supporting female equality as an ideal for a modernising society.

Conflicts which arise in marriage are frequently resolved by marital dissolution, usually by the wife deserting her husband, rarely by the husband divorcing the wife. While affinal relationships are highly valued as a basis for support in the many moots through which political and legal matters are settled among the Digo and as a basis for favourable trading arrangements, mortgages and so on, marital discord, in so far as it involves both husband and wife lobbying the wife's kin for support, and marital breakdown bring affinal relationships into question. There is a risk of conflict between husband and his wife's father, between husband and his wife's brothers, between wife's father and her brothers and mother's brothers, between wife's father and wife's mother: conflict about what happened, about who was to blame, about how much brideprice is to be returned and in what instalments and by whom (Gomm 1972: 106–8). While a woman's father and especially her male matrilateral relations have a duty to look after her interests in marriage and will intervene to prevent her from being ill-used they are rarely pleased at the prospect of a desertion or divorce. Unless serious ill-treatment has occurred, or unless there is gossip about the way in which they allow their kinswoman to be treated as if she were a slave (Gomm 1972: 105) her relations are likely to counsel the wife to behave herself and obey her husband. Where desertion occurs the idiom in which affines discuss the return of the brideprice (and by implication future relationships between them) invokes male notions of female psychology which characterise women as unpredictable, irresponsible, childish and wilful. This frequently facilitates the maintenance of good relationships between male affines *despite* marital breakdown, blaming the unfortunate turn of events on the wife. Thus women who do not obey their husbands, or cause trouble by deserting them, or girls who abscond from a betrothal run the risk of falling into bad odour with their male kin. As a strategy for resolving marital problems, desertion followed by the doubtful benefits of remarriage, or the difficulties of life as a prostitute, must be set against the ill-feeling of a woman's closest kinsmen.

It can be argued then that the manifestation of spirit possession symptoms by women offers a structural alternative to marital breakdown for coping with the tensions of Digo marriage, and one which is likely to be most attractive when male kinsmen are concerned to perpetuate a marriage.

Some evidence for this interpretation comes from looking at the demands made by the possessing spirit during an exorcism ceremony.

The most important stage of an exorcism ceremony is the interrogation of the possessing spirit by the exorcist through the medium of his spirit familiar. Once he has discovered the name of the spirit he seeks to negotiate a price which the spirit will accept in order to go away. This is generally a present, the cost of which is borne by the woman's husband or a remission from some obligation or responsibility. More rarely a sacrifice is requested. Certainly the most common requests are for *leso*; women's clothes. Perhaps it should be explained that *leso* are printed cottons bearing a design and a motto. New *leso* appear on the market each week and husbands are constantly pestered, and almost as constantly refuse to buy their wives the latest design. Other common requests include money for bus fares, or for buying cloth, petrol stoves, soft furnishings, kitchen utensils and transistor radios, and 'sweet foods'.[7] It is exactly these sorts of items which feature in quarrels between husband and wife which lead wives to characterise husbands as mean, and husbands to characterise women as extravagant. Gerlach (1961: 431) reports that in Lungalunga spirit possession cases reach epidemic proportions during the coconut harvest when men have money to spend, and in Waa where many Digo are workers commuting into Mombasa, there are regular exorcism ceremonies on pay-days.

In an exorcism ceremony the sort of demands made by women in marriage and refused, are made in the voice of a male spirit and granted. Husbands are publicly bound to provide the goods which will be used by the wife in the name of the spirit after 'cure' has been effected.

Other spirit demands are interesting. A betrothed girl is possessed by a spirit who will not go away unless a promise is made that she will have nothing to do with the sea. Her betrothed is a fisherman. A married woman is possessed by a spirit who frequents the place far inland where her husband has opened a new farm. She will certainly be re-possessed if she goes to work there. The clerk's wife who remains behind in Tiwi cannot be cured unless she leaves Tiwi (to join her husband in Mombasa). Gerlach (1961: 430 sqq.) quotes a case where a wife, threatened by the imminent second marriage of her husband, manifested spirit possession symptoms until all the money that he would have used for bride-price was spent in providing exorcism ceremonies and gifts.

To some extent then we can regard spirit possession as an alternative to desertion, and as an alternative which has some advantages. While a wife who deserts her husband risks being the centre of ill-feeling between kinsfolk and affines, the 'patient' possessed by a spirit is the centre of sympathetic concern. Such concern is shown by the way in which relatives and friends come to the exorcism ceremony, contribute to its cost and help by dancing and drumming. It is worth noting that this is a way in which her male kin can demonstrate concern for her wellbeing in marital difficulties without siding with her against her husband.[8]

So far there is nothing which I have said which runs counter to Lewis's contention that 'possession works to help the interests of the weak and downtrodden who otherwise have few effective means to press their claims'. However, so far I have dealt with spirit possession only as a problem of aetiology, asking 'where do the tensions which manifest themselves as spirit possession symptoms come from?' It can also be viewed as a problem of diagnosis; 'how is it that people are known to be possessed by spirits?'.

III

Diagnosis

This is not simply a matter of presenting a patient to a diviner for a diagnosis. People have first to be seen to fall into the category of people about whose behaviour something should be done. Now the 'classic' syndrome of spirit possession—running amok, hitting people and so on—is known by everyone: every Digo child can perform it. However, it does not seem that such symptoms have to be manifested before exorcism is carried out, nor do all people who behave in this way become the central actors in exorcism ceremonies.[9]

For instance Digo men are known to be violent. They will seize a panga and rush from the house to avenge an insult. Usually they are stopped by well-wishers before they can perpetrate a seriously violent act, but their displays of violence and aggression are credited with meaning, even if they resist being restrained, make as if to strike their friends, and slash at trees and huts, it is assumed that they have some good reason for behaving as they do. Digo women on the other hand are not thought to be naturally violent, particularly towards men. Displays of anger by women risk being interpreted as evidence of possession. Consider this case:

> Mwanasha was accused by her husband Rashidi of committing adultery. Her adulterous relationship with Hamisi was common gossip. However Rashidi was in the process of raising a mortgage with Mwanasha's father. Moreover Hamisi was the son of the local sub-chief and Rashidi was himself using tree-crop land belonging to the sub-chief for growing a crop of groundnuts To sue Hamisi for *malo* (compensation) at the level of the local moot was not likely to be successful since the sub-chief was at that time the most powerful voice there and the attempt was likely to jeopardise his crops. To divorce Mwanasha was also unattractive because of the mortgage he hoped to raise from her father. Yet Rashidi was in the intolerable position of being the butt of local gossip and of constant nagging by his own father to do something about his wayward wife.
>
> The situation resolved itself when Mwanasha cheeked Rashidi's father when he rebuked her for her behaviour. He raised his hand to strike her and she flew at him scratching his face and then ran off into the bush. Rashidi and his brothers caught her and delivered her to a *mganga*, who divined spirit possession as the cause of her behaviour. The exorcism ceremony resulted in Rashidi providing his wife with several expensive gifts, but it also altered the situation in his favour. The situation was translated from one involving a husband who could not satisfy his wife sexually and was not man enough to control her behaviour, into one in which a woman was possessed by a lascivious and uncontrollable spirit.

An obvious case of spirit possession, but it hardly needs saying that had the complexities of Rashidi's life been different, divorce and an adultery suit would have been the outcome rather than exorcism.

Again women are known to be naturally lascivious, while men are much more self-controlled, sexually. Thus a woman who refuses to have intercourse with her husband has either just had intercourse with another man or is possessed by a spirit. Such was the case of Mamuna whose symptoms consisted in kicking her husband out of bed.

In considering cases of spirit possession both we and the Digo have to face the problem of authenticity. Do husbands who present their wives for treatment believe them to be possessed, or are they cynically utilising a diagnosis of possession to represent a situation in terms favourable to themselves? Or, is the woman

genuinely possessed, does she believe that she is possessed, or is she feigning? Digo men are certainly very suspicious that they are being taken for a ride.

From our point of view there are obviously two dimensions to the problem of authenticity, because there are at least two parties concerned in defining a person as possessed; the person who is putatively possessed, and others. So far as the deviant is concerned her behaviour might be a cynical feigning of the appropriate symptoms to gain gifts and attention; it might be what a psychiatrist would regard as a 'genuine' hysterical episode brought on by a build up of 'intrapsychic tension' (Freed & Freed 1967: 313–17) and cast into what is a culturally given 'presenting syndrome'; or it might be an acting out of an unshakeable diagnosis made by others. It might also consist of a denial of possession, interpreted during exorcism as evidence of possession by a very difficult spirit.

The behaviour of others towards the deviant might consist in a public refusal to believe in the authenticity of the attack; a private belief that the attack is feigned but a public acceptance of its authenticity; a genuine belief in the reality of the possession; or a cynical use of a spirit possession label to redefine a problem of control as a problem of possession.

I have spelt these alternatives out at some length because it can be argued that the existence of these alternatives does not constitute merely a technical problem of anthropological interpretation. The problem of authenticity is rather, an invariable feature of a class of phenomena which for want of a better term I will call 'mendicity' (Gomm 1974).

IV

Mendicity

It can be argued that peripheral spirit possession belongs to a class of social phenomena in which someone can be represented as bargaining for something, not on the basis of what he or she has to offer in return, but on the basis of what he lacks, is about to lose, suffers from or is deficient in. Within this class we can include the suicide on the parapet, the invalid, the beggar, the victim of witchcraft attack, and certain types of behaviour bracketed as 'mental illness' in advanced societies. Similar are pleas of mitigation at law and in everyday life when someone makes a shortcoming an excuse for otherwise unacceptable behaviour. Recently Wyllie (1973) has drawn attention to close parallels between witchcraft characterised by voluntary and unsolicited confessions and peripheral spirit possession. I have used the term mendicity to draw attention to the fact that these phenomena all involve a particular type of asymmetrical transaction.

Mutatis mutandi they have in common all or most of the following characteristics.

a) that someone who occupies such a mendicant role 'properly' is one who is not deemed fully responsible for his own condition, and that,

b) he is deemed incapable of behaving in any other way (McHugh 1970: 152–179)

c) occupancy of the role entails special privileges which would not be granted to someone of his latent/previous status . . . .

d) but usually entails putting at risk and sometimes losing other values, such as autonomy, credibility, esteem, personal integrity, position, friendship and so on.[a]

e) That because these are situations in which someone is seen to be getting something for nothing, they are usually situations which involve doubt about whether the someone 'deserves what he gets'. Thus it is characteristic of mendicant situations that there are either strict procedures for ascertaining culpability and authenticity (a & b above) and/or that doubt prevails about the moral probity of the mendicant.

Gouldner (1960) in his article on the norm of reciprocity has cogently argued the importance of reciprocity as facilitating stable patterns of interaction.

> Status duties shape behaviour because the status occupant believes them binding in their own right, they possess a kind of prima facie legitimacy for properly socialized group members. The general norm of reciprocity is however a second order defence of stability; it provides a further source of motivation and an additional moral sanction for conforming with specific status obligations (Gouldner 1960: 175–6).

It is Gouldner's contention and one which I believe is widely accepted that the norm of reciprocity is an important social stabiliser in all societies (although not upon all occasions):

> The norm is a kind of plastic filler capable of being poured into the shifting crevices of the social structure, and serving as a kind of all purpose moral cement (Gouldner 1960: 175).

Gouldner directs special attention to relationships where little or no reciprocity seems to occur—relationships such as those characteristic of a mendicant situation. He suggests that:

> If empirical analysis fails to detect the existence of functional reciprocity, or that it has been disrupted, it becomes necessary to search out and analyse the compensatory arrangements that might provide means of controlling the resultant tensions, thereby enabling the problematic pattern to remain stable (Gouldner 1960: 164).

Rewarded mendicant behaviour is behaviour which is problematic in this sense. How is it that people can be persuaded to lay out sometimes large sums of money, to grant privileges, to overlook or suffer insult, abuse and insubordination without hope of reciprocation from the recipient of such largess?

At one level the answer to this question lies within the field of Digo political organisation. Men work hard to ensure the goodwill of other men. The redefinition of a problem of marital relationships as a problem of spirit possession (however the translation is made) places the husband in such a position that he risks alienating his affines and public opinion if he refuses to provide exorcism ceremonies or to accede to spirit demands—in a way that refusing to buy his wife a new dress does not. Spirit possession behaviour ('genuine' or 'faked') thus levers the husband into a publicly visible situation where the correct behaviour for the good husband is clearly defined. Alternatively since a competent man is one who knows how to handle women, be they wives, sisters or daughters, the redefinition of a problem of discipline as a problem of possession (whether genuinely believed in or not) enables men to maintain a stance of competence in the face of conflicting evidence—although at a financial cost. Further, while in cases of marital discord a woman's kin faces problems of deciding whom to side with, the translation of the problem

into one of possession enables all parties to co-operate in effecting a 'cure'. Thus spirit possession can be represented as a set of strategies for redefining situations which is available to all; it can be used by husbands to manipulate their wives, and fathers to manipulate their daughters, as well as by wives to manipulate their husbands.

V

Gouldner, however, asks the question not at the level of motivation, but at the level of 'functional reciprocity'. Applied to the Digo his question would be 'How does spirit possession stand in relation to the *status quo* in Digo society?' This is to address the problem of 'supposing everybody did it, what then?' For in so far as the occupancy of a mendicant status sets aside normal obligations, we must consider what mechanisms exist for limiting recruitment to such roles.

As stated above, in situations in which someone is allowed to get something for nothing there are usually strict procedures for ascertaining whether they deserve what they get. Crucially this means authenticating the affliction as genuine, and as being no fault of the afflicted. Unless these two conditions are seen to be met then rights to the privileges of a mendicant status will not be granted readily.

The authentication of spirit possession as genuine may be the task of a female diviner consulted before an *mganga* is contracted, but female credibility is low and patients may be delivered direct to the male *mganga*. The *mganga* both in divination and in the practice of exorcism authenticates the reality of possession. From this point of view exorcism ceremonies can be seen as an apparatus for confirming a spirit possession diagnosis as well as for attempting a cure. The exorcism ceremony is so structured that the possessed is rewarded for confirming the diagnosis by speaking with the voice of a possessing spirit. Indeed even this is not necessary, for failing the co-operation of the patient the *mganga* may use ventriloquism or claim that the spirit speaks silently in a way that only his familiar can hear. The authentication of the spirit possession attack as genuine is relatively unproblematic once exorcism is involved. Note that it is not necessary to convince everyone that the attack is genuine, but merely to bring opinion to the state where it is publicly said to be. The Digo are not unaware of the interested nature of the *mganga's* involvement, but private doubts on this basis do not hinder co-operation in exorcism.

The personal culpability of the host is another matter. Relationships with spirits are not necessarily seen as involuntary. People are said sometimes to court spirits for the favours they give. For instance it is well known that spirits lead people to buried treasure or cause fish to swim into nets, and of course the *mganga* and the diviner base their powers on the control of familiars. Sometimes such relationships with spirits run out of control.

For spirit possession to be regarded as an affliction worthy of sympathy and attention, demanding of people to spend money on exorcism it must be seen as involuntary. Husbands are unlikely to provide exorcism ceremonies for their wives if they imagine their wives to be actively colluding in their own possession, and this is exactly what is imagined to be happening if women become possessed recurrently. The more frequently a woman has been possessed before, the less

likely it is that her husband will bear the cost of a further exorcism. While it may be the case that refusal to provide exorcism outrages the woman's kin and they may provide a ceremony themselves, it also happens that husband and affines agree that this is a hopeless case, unlikely to be dealt with successfully. An acceptable formula to both parties is an agreement that while possession is genuine and involuntary, nonetheless the spirit involved is so strong that it is unlikely to be driven out by exorcism. The end state of a woman who is recurrently possessed is one in which no one takes upon himself the responsibility for bearing the cost of exorcism ceremonies. Such women tend to hang around the compounds of exorcists, receiving small sums of money for helping them with their work, and for providing gossip essential for their counter-witchcraft activities. They supplement their income with earnings from prostitution.[10] Some operate as diviners. When they manifest the symptoms of spirit possession they are allowed to run through the bush, attack trees, tear their hair, and eat ashes and dirt without hindrance. It can be seen, I think, that as a technique for obtaining favours from men, spirit possession has a time limited utility.

Islam among the Digo gives to women an inferior ritual status. Their ritual activities are regarded by men as dirty and as against the *sharia*. Pertinently here, this includes the traditional propitiation of nature spirits; once it would seem, an important aspect of Digo religion (Prins 1952: 88) involving both men and women, but now almost solely a female concern Such minor cults do involve mediumship (controlled possession) by women which can be used for divination and for communication with the spirits. However such activities as these, performed by women, are not usually taken seriously by men. Involvement in such a cult and possession by such spirits do not grant women any favours from their kinsfolk—how could it, when favours are granted on the basis that possession is an involuntary affliction?

An important difference between the Digo and many other well documented examples of peripheral possession is that the exorcist does not achieve his status through a transition from spirit victim to spirit controller. *Aganga* reach their status through a system of apprenticeship whereby young men are bound for a fee to senior kinsmen. Involuntary possession is thus not a step on an upward career ladder as has been claimed for the Zar cult in Ethiopia (Messing 1958), the BaVenda in Southern Africa (Stayt 1931: 301, sqq.), the Pokomo of Mafia Island (Lewis 1971: 108), or Voodoo adherents in the West Indies (Lewis 1971: 107). While in none of these cases is it clear as to what constraints operate to prevent everyone from becoming an exorcist or medium, it *is* clear that opportunities for improving social status are restricted to those who become 'therapists' and are not available to the majority of their 'patients'.[11] As Lewis has pointed out, the status of the therapist is controlled by suspicions and accusations of witchcraft against those who show the ability to *control* spirits. This is true also among the Digo but here, from start to finish, therapy is controlled by the male establishment.

Among the Digo, involuntary possession may proceed from acute states to chronic ones and the recidivist experiences what might be termed downward mobility in terms of moral standing, although gaining a measure of individual freedom not shared by other women. Some women may be seen to gain control over a spirit and to be able to charge a small fee for divination; scarcely an impressive

upward movement. Moreover a woman who claims to control a spirit loses any capacity to bargain for privileges on the basis of being involuntarily afflicted.

An important feature of any social control system is the conceptual apparatus used to explain deviance. If what passes for normal behaviour is regarded (as it usually is) as 'natural' behaviour, then what are counted as serious deviations have to be explained, and explained in such a way that they do not count as evidence against the taken-for-granted notions of natural behaviour. Most folk-theorising on deviance can be represented as saving a prevailing theory of conformity.

In coastal Digo spirit possession, men regard themselves as the natural superiors of women, with the right and the competence to make rules for their behaviour and to enforce them. From the point of view of the anthropologist it can be argued that the difficulties of maintaining goodwill between men prevents them from enforcing such rules as they make. Women are constantly 'getting away with it'. Moreover the tensions which are manifested in the form of spirit possession attacks are themselves generated by this control system. The concepts of spirit possession mystify the sources of discontent and the failure of control by separating cause from effect and attributing problems of control to causes outside the social and moral order: to the meaningless actions of adventitious and capricious spirits. The reality of possession is authenticated by a male practitioner in the practice of exorcism. Those who suffer from possession are set in a system of rewards and punishments which enables them to be manipulated to confirm a theory. The privileges permitted and the gifts granted to possessed women are part of this matrix.

It can be argued with some truth that spirit possession attacks may redress the disadvantages of a lowly and deprived status, but at the same time the way in which this amelioration is obtained ensures that deviants confirm a mystifying theory which is an essential element in the control system which maintains them as lowly and deprived persons.

The privileges of spirit possession are seen by the Digo as a benign response to affliction, which may be abused by some people. Faced with frequent possession, or controlled possession they invoke the culpability clause. Women who can be seen as courting possession are denied the rights accorded to those who are seen to suffer through no fault of their own.

There are two ways in which younger women[12] can evade male controls. One is through full-time prostitution, in which role women service the sexual desires of Digo men awaiting marriage or deserted by their wives. The other is through chronic spirit possession. Both are conditions of low esteem in which the evasion of male control poses little in the way of a threat to the theory of male dominance. One group is by definition a group of bad women, the other if you like, a group of 'mad' women, in the sense of being seen incapable of sustained rational action. Moreover the badness of one group is frequently explained with reference to their probable possession by spirits, while the madness of the other is frequently attributed to their bad behaviour in courting spirits for personal gain.

VI

If we turn from Digo spirit possession to other types of mendicant situation we

can see that the problem of sorting the deserving from the undeserving is one which is addressed in all cases. They include distinguishing the sick from the malingerers (Daniels 1970: 182–205; Szasz 1972: 37–64; Field 1953; Bagley 1971), the insane murderer from the rational murderer,[13] the genuine suicide from the cry-for-help suicide (Sudnoe 1967), the woman raped from the seductress who changed her mind, the educationally deprived from the lazy, and the unfortunate from the careless. Distinguishing between the deserving and the undeserving poor has dominated income-maintenance policy in England and America throughout recent history.[14] The set of typifications of occupants of mendicant roles always includes a typification of someone who occupies the status illegitimately; the person who is abusing the system.

This is not surprising since mendicity is a setting aside of the normal rules of behaviour to accommodate those deemed incapable of conforming. It constitutes a loop-hole which must be closely guarded.

In dealing with mendicity then we are dealing with bodies of ideas which are used to legitimate the setting aside of normal criteria for behaviour, whether these be status obligations or more generally reciprocal ones. It can be seen that where a group of mendicants is drawn from those of low status, then the body of ideas used to sanction the granting of privileges is usually one which mystifies the causes of their condition.

Thus the poor may be succoured on the grounds of their personal incompetence and depravity (Graham 1965; Matza 1953; Ryan 1971), mystifying the processes which actually distribute income and wealth; the working class child may be given 'compensatory education' on the grounds of his 'cultural deprivation' (Friedman 1967; Bernstein 1968; Keddie 1973; Labov 1973), mystifying the control which elite groups exert on the class-specific criteria for educational success. And Digo women are granted privileges on the grounds that their behaviour derives not from a male dominated culture, but from adventitious spirits. Part of the mystifying apparatus is the low credibility accorded to the mendicant. The poor lack 'insight' or 'intelligence', the educationally deprived are 'inarticulate', the mad are mad and the possessed are after all possessed. Their behaviour is exhausted of the meanings they might attribute to it and given other meanings (Cohen 1971: 19).

Cherished and convenient ideologies are preserved. Many examples of mendicity involve manipulating the deviant to confirm the control theory, or perceiving only those features of his/her behaviour which do (Young 1971; Rosenhan 1973; Gomm 1974; Braginski *et al.* 1969; Schatzman 1969), as here exorcism confirms spirit possession.

To re-address Gouldner's question about the relationship between asymmetrical transactions and the *status quo*; where recipients of apparently unreciprocated largess are the poor and the oppressed, the privileges they receive may frequently be regarded as rewards for colluding in their own oppression.

NOTES

This is an amended version of a paper first read at a staff seminar at Swansea University in May, 1974. The author would like to thank participants for their comments and Dr Margaret Kenna for making the seminar possible. He would also like to thank Professor Ioan Lewis for

his encouragement to publish this article. The contents however remain the views of the author. The fieldwork on which this article is based was carried out in 1967-68 with the aid of a James A. Swan Fellowship from the Pitt Rivers Museum, Oxford and an Emslie Horniman Scholarship from the R.A.I.

[1] This article refers principally to the Digo, who are the largest tribe on the South Kenya coast. However, much of what is said here applies also to minority Bantu groups such as the Rabai and Segeju and to the many families who are the descendants of slaves brought from Tanzania by the Arabs in the 19th century.

[2] See for instance: Harris 1957, Lewis 1966; Messing 1958; Parkin 1972; Shack 1966; Wilson 1967, Onwuejeogwu 1969; Caplan 1968.

[3] An alternative to the procedure described here is a régime of dieting, purging and steam bathing under the direction of a female Arab doctor, during which period the afflicted person lives in the household of the doctor. This therapy is far less frequently used by the Digo than public exorcism.

[4] In 1967 the cost of an exorcism ceremony at Tiwi carried out on two successive nights was about K300/-, the bulk of which was paid by the husband (and his father and brothers), the remainder in small contributions by other attenders Most of the money is kept by the *mganga*, who makes small payments to people who assist in various ways (some of whom will themselves have contributed to the cost).

[5] It is difficult to give a numerical value to the ratio between male and female hosts undergoing exorcism. Concentrating on one *mganga* only the number of exorcisms carried out in 3 months was 25, involving 18 patients, only one of whom was male.

[6] 'Cursing' involves the withdrawal of a blessing which is deemed operative until withdrawn.

[7] It is worth drawing attention to the modesty of spirit demands; women ask for bus fares, not buses.

[8] I say little in this article about group psychotherapeutic aspects of exorcism ceremonies It has sometimes been rather glibly assumed that gathering around the patient a supportive group of kin and friends is 'therapeutic' in the sense of 'curing' an affliction Certainly Digo exorcism ceremonies show nothing in the nature of bringing into the open underlying conflicts and tensions which might be the cause of the problematic behaviour, as convincingly described by Turner (1964). Rather underlying conflicts and tensions are obfuscated by the ceremony (see above). Throughout this article the term 'therapy' is used without a value load after the manner of Berger and Luckman (1971).

[9] Readers will recognise that this is essentially a reformulation of Parsons' description of the sick-role in Western societies (Parsons 1952: 428-79). See also Badley (1971).

[10] It should not be thought that these women are entirely destitute. They continue to garden on their own account It is usually the case, however, that their children born in marriage are removed by their husbands.

[11] So far as cult members gaining in social status is concerned, this is not well demonstrated in the literature. Lewis (1971: 107) points to favourable treatment in trading and jobs being given to cult members of Voodoo groups, but this is set in a situation in which Voodoo groups compete for membership. Upward social mobility claimed by Messing (1958) for Zar cult members seems to consist in being able to rub shoulders with members of higher status groups during Zar ceremonies. If anything his data suggest that the Zar cult has become declassé as it has become more and more associated with those of depressed status. One example however stands out clearly as an exception; the case of 'therapeutic Islam' among the Giriama Here where Islam is a peripheral religion, possession by Islamic spirits who demand adherence to Islamic ways enables upwardly mobile entrepreneurs to evade the financial drag of kinship obligations towards their pagan kin (Parkin 1972: 42-6).

[12] Women with married sons are granted considerably more freedom in Digo society.

[13] For a discussion of the MacNaghten Rule see Mechanic (1968: 82-3) and references cited therein.

[14] From the problem of 'sturdy beggars' in Tudor times to 'scroungers on the dole' today: 'There is nothing legally wrong. It would not be fair to call it scrounging if the person is

genuinely seeking work. But some people are taking advantage of their rights as citizens in an awkward way'—Richard Crossman as Secretary of State for Social Services. Quoted 'Sunday Times' August, 1969.

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WHY DO THE TROBRIANDS HAVE CHIEFS?

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The fact that the Trobriand system of rank and chieftainship is unusual in western Melanesia has either been ignored or explained in terms of high population density and agricultural productivity. However, such explanations are unsatisfactory, as neither the population density nor the productivity is exceptional by Melanesian standards. Following Uberoi, this article argues that the key to the Trobriand political system lies in the *kula* exchanges. Nevertheless, Uberoi has overlooked certain peculiarities of *kula* in the northern Trobriands. These peculiarities, arising from environmental and social conditions, enabled the exchange system to be closed off to a degree not possible elsewhere in the Massim area. This precluded certain economic and political strategies from being undertaken, thereby limiting the range of people who could effectively compete for leadership and facilitating the development of rank and chieftainship. The article concludes with a brief examination of the applicability of the general argument to other Melanesian societies.

I

The ranked descent groups and chieftainship of the Trobriands have long been recognised as atypical for western Melanesia. Although Malinowski overemphasised the power and position of the chiefs[1], there is little doubt that the Trobriand political system differs from the usual Melanesian Big-man type, both in the ascription of differential rank to the sub-clans (*dala*), which operates to limit eligibility for leadership, and the extent to which the status of leader is recognised and given legitimacy. Furthermore, in contrast to the indeterminate number of Big-men that comparable groups in other Melanesian societies contain, a Trobriand sub-clan has only one recognised leader (Powell 1960: 124, 125, 129 and *passim*).

Arguments that attempt to explain the Trobriand political system as resulting from a high population density (e.g. Powell 1969: 601; Uberoi 1971: 96–8), or agricultural productivity (Harris 1971: 392–8; also implicit in Uberoi 1971: 124) are unconvincing. Kiriwina island has a population density of a hundred persons per square mile (Powell 1960: 119), a figure exceeded by areas in western Melanesia which lack systems of rank and chieftainship, including Dobu Island[2] (Brookfield 1971: 24). Although it is extremely difficult to compare productivity, there are no grounds for believing that *per capita* production was significantly greater than in other areas of western Melanesia[3]. Austen's figures indicate that even in a year of competitive food exchanges when excellent conditions prevailed, no more than 0·5 tons *per capita* of yams were produced (1945: 17–18). This does not appear to be outstanding; the average Siö household produces 0·4 tons of yams *per capita* per year (Harding 1967: 97) and the average Enga household produces 0·5 tons of sweet potatoes *per capita* (Meggitt 1958: 311–12).

The thesis of this article is that the social and physical environment of the

Trobriands, or more particularly the north-eastern part of Kiriwina Island, afforded greater potential for individual or group control over crucial political resources than elsewhere in the Massim area, as well as most other parts of western Melanesia. Like Uberoi (1971), I will be concentrating on the *kula* ring. However, I will show that he has overlooked some crucial aspects of its operation, especially in Kiriwina district.

II

The kula *ring*

A number of features need to be stressed in Malinowski's description of the *kula* ring. The *kula* valuables (*vaygu'a*) are necklaces of spondylus shell (*soulava* or *bagi*), exchanged in a clockwise direction, and white conus shell armshells (*mwali*), moving in an anti-clockwise direction. These valuables are 'in a class of their own' (1922: 94) which ideally are exchangeable only against each other and are not permanently owned but need to be passed on. This prevents their destruction or redistribution at death (cf. Kuma, Reay 1959: 96–7; Siuai, Oliver 1955: 209–10). The valuables move in specific channels and each participating community is involved in carrying them on to the next community. Although *kula* communities could trade both with non-*kula* areas and communities beyond the limits within which *kula* partnerships could be contracted (Malinowski 1922: 148, 500–2, 505–8) the *kula* system itself appears as essentially closed. Thus the possibilities for entrepreneurial manipulation by ambitious men were restricted in comparison with other Melanesian exchange systems.

Other research suggests that Malinowski's presentation requires revision (see fig. 1). Belshaw found that the southern section of Malinowski's *kula* ring was in fact made up of two separate ceremonial exchange rings called *kune* (Belshaw 1955: 26–7. See also Lauer 1970: 175). Lauer indicates that the *kula* was not a static system and suggests that 'in the past the *kula* seems to have been subjected to major changes in its geographical dimension and ultimate purpose, adapting to both with great flexibility' (1970: 176). Belshaw's material supports this view (1955: 26). Even Malinowski, noting the absence of any *kula* myths associated with the Northern Trobriands and Sinaketa states that 'this would point, on the surface of it, to the fact that in older days, the island of Boyowa [Kiriwina] except its southern end and the eastern settlement of Wawela, either did not enter at all or did not play an important part in the *kula*' (1922: 307).

Unfortunately, we do not possess any material indicating the process whereby new communities were introduced into the ring in pre-contact times, although it is possible to make a number of relatively informed guesses. *Kula* valuables were used (in varying degrees by different communities) for marriage payments (Fortune 1963: 189; Malinowski 1932: 77; 1966: 291). A marriage between a man from a *kula* community and a woman from a non-*kula* making group might have enabled the woman's male kin to acquire *vaygu'a* and eventually participate in *kula* exchanges. Alternatively, *vaygu'a* might be passed on to trade partners outside the ring in sufficient quantities for them to become incorporated into the *kula* system.

Ambiguity and confusion exist about the nature of *kula* valuables. Although Malinowski states that the principal *vaygu'a* are the armshells and necklaces (1922:

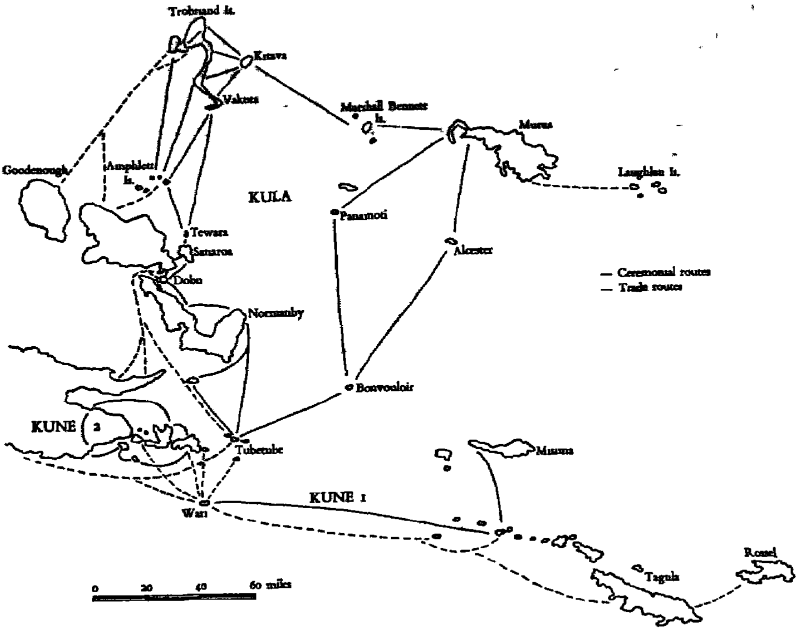


FIGURE 1. Probable trade and ceremonial exchange routes. Based on Belshaw (1955: Map 1), Fortune (1963 203), Lauer (1970), Malinowski (1922: 497–505).

86, 90), in a later section of the same book he mentions that boar's tusks (*doga*) were also involved, moving in the same direction as the *mwali*. Traditionally, these were almost as important as the armshells, but by the time of his fieldwork very few were still circulating (1922: 357). Malinowski suggests that because the *doga* were valued more highly on the mainland than in the *kula* region there was a greater tendency for *doga* to be exchanged out of the ring.

The polished greenstone adze and axe blades (*beku*) also present a problem. The largest and finest of these were not used and did not 'like the armshells and necklaces, go round and round the ring in perpetual exchange' (Fortune 1963: 207) Although Malinowski asserts that the *beku* are not regularly *kulaed* (1922: 354) he later states that the biggest *beku* 'became a specially important class of *vaygu'a*' (1922: 481).

Seligmann states that fine examples of *beku* (called *benam* in Murua, their place of origin) were named (1910: 518, 531). The prices paid for some blades makes it hard to accept that they were minor valuables. A typical example follows:

Kwewos. This stone was sold by Nevan [a famous craftsman] to a Murua man for five pigs, one pair of *masiwaru* [armshells] and one small canoe, apparently a dugout. The Murua man was said to have sold it to a Lachlan boy for ten pigs and five canoes full of coconuts (Seligmann 1910: 531).

This transaction also seems to indicate that the value of the blade was higher in the Lachlans than in Murua. Seligmann's examples (1910: 531–5) illustrate the difficulty of sustaining a characterisation of *beku* as being merely solicitary gifts, at

least in that sector of the *kula* ring to which Seligmann is referring. Finally, in the Dobu area, Fortune writes of magic performed when Tewarran canoes travel south, where solicitary gifts of yams and coconuts are verbally transformed into valuables of the *kula*—necklaces, spondylus shell and greenstone blades (1963: 225–6).

A further interesting point concerns the large coarse necklaces (*katudababile*) produced in Sinaketa and Vakuta. In the caption to Plate XVIII of *Argonauts* Malinowski states that they do 'not play any important part in the *kula*'. But later we are told that the *katudababile* are sometimes given to Dobuans as *kula* gifts by the Sinaketans. Some of these later return to Sinaketa and join the *soulava* in their clockwise movement around the ring (Malinowski 1922: 358).

The following quotation indicates that these *katudababile* were not just minor valuables.

'In the old days the Sinaketans ... used to marry women from Kiriwina. Marriage always entails ... reciprocal gifts. vegetable food from the wife's kinsmen to her husband; that is, the annual *urigubu*, and in return valuables—in this case red shell discs [there is no doubt whatsoever that Malinowski is referring to the *katudababile* here]—from the husband's to the wife's kindred. Now people of rank in Kiriwina were able to supply the Sinaketan husbands of their sisters with a large quantity of food. The Sinaketans in return gave to the paramount chief and to other notables *what to these was of greater importance, necklaces and belts of shell discs*' (Malinowski 1966: 290–1, my emphasis).

Malinowski wrote about the *kula* from the Trobriand viewpoint in general and Kiriwina and Sinaketa districts in particular. But there is no reason to believe that the valuation and type of *kula* items and the means used to obtain them should have been standardised throughout the ring. It is only by accepting that they were not uniform that the differences in the published material can be reconciled. It is hard to believe, for instance, that the value of a *kula* item was the same in the area where it was produced as its value in places a considerable distance away. With the *katudababile* necklaces, for instance, I would suggest that although the Kiriwinans themselves may not have regarded them as true *kula* valuables, it was still important to obtain them as they might have been used as such with communities farther east in order to obtain armshells

If my suggestions in this section are accepted we are faced with the question of why there should have been such specificity of *kula* valuables in the northeastern Trobriands.

III

Kula *in the Trobriands*

A careful examination of *kula* routes through the Trobriands (see fig. 2) undermines Malinowski's assertion (which also forms the main basis for Uberoi's thesis, 1971: 124) that Omarakana in Kiriwina district is 'one of the most important places in the ring' (1922: 468). It is surprising that no-one has drawn attention to the fact that Kiriwina district is in a rather precarious position in the *kula* network. This is apparent from Malinowski's statements about 'the double ramification which

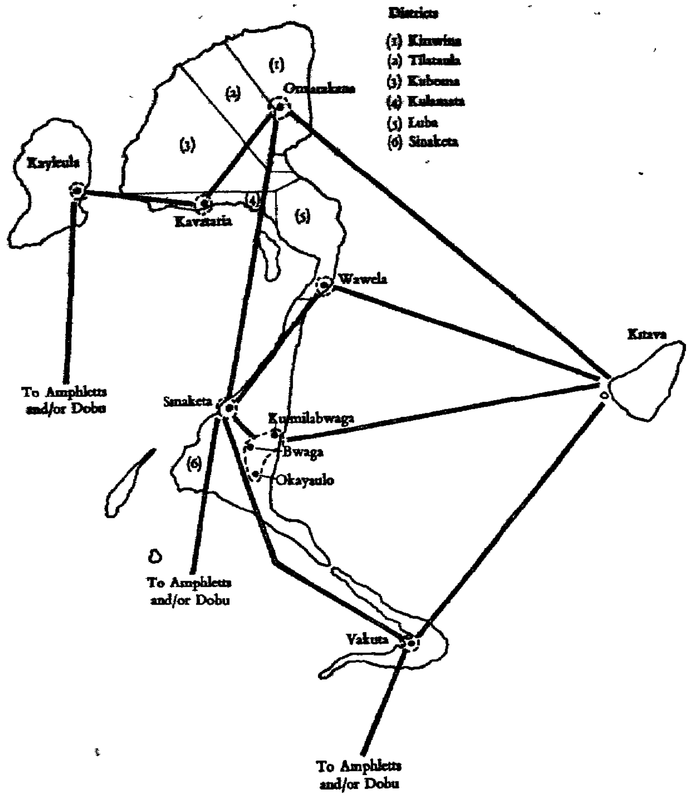


FIGURE 2. Kula routes from Kitava to Dobu.

unites Kitava with Dobu. The short route runs direct from Kitava to Vakuta and from Vakuta to Dobu. Besides this, however, there are several longer ones' (Malinowski 1922: 497).

From Kitava, through which all *kula* valuables must pass, there are four possible routes; through Kiriwina district, through Vakuta island, through the Okayaulo group of villages and through the independent village of Wawela (Malinowski 1922: 476–7). Although only a small number of *kula* valuables passes through these last two routes, the fact remains that Kiriwina district has to compete with three other areas in getting access to the *kula* stream. Some quantitative indication of this is apparent from Malinowski's account of a series of *kula* transactions involving armshells. First the Vakutans went to Kitava bringing back 'a big haul of armshells' (1922: 386) which must have numbered at least 344 pairs as this is the amount that the Vakutans gave to their Dobuan partners a few weeks later (1922: 391). The chief of Kiriwina then visited Kitava and was able to bring back only 213 pairs of armshells. The Sinaketans obtained 154 of these and added 150 which were already in Sinaketa (1922: 386). The total was then passed on to the Dobuans. (The 150

armshells previously held in Sinaketa may have arrived through the other two routes to which the Sinaketans had access—Okayaulo and Wawela—although this is not specifically stated.) Thus out of 707 armshells passing through the Trobriands on this occasion, Kiriwina was able to get less than a third. Even if we make the unlikely assumption that *all* the 150 armshells already present in Sinaketa had actually arrived from Kiriwina at an earlier date rather than through the other two routes, Kiriwina would only have procured 363 armshells or little more than half of the flow. Furthermore, Kiriwina got no necklaces at all from Sinaketa in return for these armshells (1922: 468).

Kiriwina's precarious *kula* position is also illustrated by indirect evidence.

From the Trobriand point of view Malinowski explicitly states that a *kula* partnership constitutes a lifelong relationship and that even if a man was disappointed with his partner the relationship would not be broken (1922: 21, 96, 274; 1926: 25). However, Fortune, writing of Dobu, states that partnerships 'frequently break up and new partnerships are contracted owing to one of the partners failing to meet his just obligations' (1963: 214). Differences thus exist between the two places, even if more ideological than actual. (As the Trobriands and Dobu Island are not in direct *kula* contact the assertions are not contradictory). These differences can be understood in terms of ease of access to the *kula* stream. In the Northern Trobriands—in a precarious position in the ring—partnerships are retained at all costs. In Dobu, which is in a strong position (and where the *kula* may not be quite as important for status building as in the Trobriands[4]), men can presumably afford to be more flexible in this regard.

The utilisation of *kula* items in the Trobriands, particularly Kiriwina district, differs from that in Kitava on the one hand and Dobu on the other. Although the *kula* valuables are involved in affinal payments in the Trobriands their use appears to be limited. Given the pattern of marriage preferences, affinal exchanges probably involve *kula* partners in any case, as there is a tendency for spouses to be of similar rank (Malinowski 1932: 71), and for low ranking persons either not to participate in the *kula* or to have very few partners (Malinowski 1922: 91, 275).

Malinowski does not provide quantitative data on affinal exchanges involving *vaygu'a* so it is only possible to make assertions by comparison with Dobu. Here, Fortune provides a statement which finds no parallel in any of Malinowski's Trobriand material.

'The groom and his kin also go forth on the *kula*. *Regardless of future impediment to their kula* [my emphasis] they must accumulate armshells, many of them, and a spondylus shell necklace or two to give as a present to the bride's relatives as the *kivesi*, or bride price. The bride's relatives need not return the equivalent of these gifts in any haste. In fact the return is often years delayed' (1963: 189).

From the Kitava side, all the way round to Tubetube, *kula* is associated with funerary distributions (Malinowski 1922: 489). After a man dies *kula* valuables are 'dammed up' in order that they may be used in the final mortuary feast. During the interval from the death to the feast no valuables are given away although the dead man's community accumulates as many as possible. This period might vary in length from a few months up to two years in the case of a really important man. At the distribution following the feast all the *kula* partners of the community are invited and the accumulated valuables are released in a sudden flow. As all the

valuables have to pass through Kitava on their way to the Trobriands some important consequences follow, although Malinowski does not deal with them.

First, under these circumstances, the Trobrianders would hold on to the valuables for an even shorter period than usual. They would not be getting any armshells for a period of up to two years, but in order to maintain themselves in good standing with the Kitavans they would still have to pass on the necklaces. Secondly, as all the valuables are given out at once, to all the assembled partners, and as men from Kiriwina would be competing with at least three other *kula* communities for the armshells, it would seem reasonable to expect that the opportunities for manipulation would be more restricted[5]. Furthermore, this competitive situation would entail greater expense in terms of the solicitary gifts required[6].

On the opposite side of the Trobriands there is also a tendency for *kula* valuables to dam up, although it is not as marked as on the Kitavan side. The Dobuans do not have a large scale *kula* distribution at the funerary feast, but after a death they accumulate *vaygu'a* in order to adorn themselves with valuables at the final mortuary distribution—a practice absent from the Trobriands. Therefore, valuables are held up for a few months (Malinowski 1922: 492).

Thus, although the Trobriand utilisation of *kula* valuables in non-*kula* contexts is minimal[7], they are surrounded on both sides by communities which use them extensively in activities not directly connected with the *kula*. Even in Vakuta, *vaygu'a* are used more widely as they have to be given to a wife's brother in return for the latter's harvesting and transport of the *urigubu* payment (Malinowski 1966: 295). The fact that neighbouring areas use *kula* valuables in other contexts tends to increase the precariousness of the Trobrianders' hold over *vaygu'a*[8], particularly in Kiriwina district. (Sinaketa and Vakuta, manufacturing the *katudababile* necklaces, are in a stronger position). It also raises the question of why the Trobrianders do not similarly use *kula* valuables so extensively.

I would suggest that the answer to this question has two aspects. The first relates to the likelihood, touched on earlier, that the northern Trobriands have not been involved in the *kula* for the same length of time as other communities. The second relates to the great importance of the *kula* in defining status (Uberoi 1971: 43, 124–6), and the tenuousness of the Northern Trobriands' hold over the *kula* stream. I am arguing that it is crucial for the men of Kiriwina district to keep the valuables moving; they are both too important politically and too difficult to procure to take the risk of substantially diverting them to other activities, thereby possibly antagonising partners in neighbouring communities.

IV

Uberoi and Powell on Trobriand chieftainship

Although Uberoi has convincingly demonstrated the importance of *kula* in Trobriand politics, two major criticisms need to be made of his work. The first is that he has not examined the *kula* network in sufficient detail and consequently overemphasises the position of Omarakana in the ring. The second is that he shows little concern with applying his argument consistently.

Uberoi suggests that the rank of a local lineage (*dala*) is 'the outcome of a combination of three things; (1) economic advantage, which means a village with

fertile gardens, or one particularly well suited for fishing; (2) the extent to which it is a centre integrating the economic activities of its neighbours; and (3) its position in the network of overseas alliances' (1971: 43).

However, it is necessary to point out that, although a feedback process is no doubt involved, a *dala* is more likely to be a centre for economic exchange as a *consequence* of its high rank than vice versa, as Uberoi implies.

Further to (3), in a later chapter of his book Uberoi states: 'of two villages or districts of otherwise comparable standing, the higher rank would belong to the one which was more proficient in the *kula* and had the more advantageous external affiliations' (1971: 124).

But Uberoi is unclear as to why the Trobriands should have a system of ranking, whereas other islands do not. In so far as he raises the question at all it would seem that he locates the answer in the higher population of the Trobriands, suggesting that this made the *vaygu'a* more scarce (1971: 96–7) However, his suggestion here is simply misleading. The actual population of an *island* is irrelevant. What matters is the population of a *kula* district, of which there are at least seven in the Trobriand group (Malinowski 1922: 475), whereas Dobu Island forms one *kula* district (Malinowski 1922: 82). Uberoi's figures ignore this point.

A comparison of the villages of Omarakana, Sinaketa and Vakuta in terms of Uberoi's three factors yields damaging results. These three villages all contain Tabalu *dala*—the highest ranking *dala*. Omarakana is the most important village in the Trobriands and the one where rank is most exemplified (Malinowski 1922: 66; 1966: 430). Although it does have very good gardens and is the centre of Kiriwina district, where the 'district economy' is most pronounced (Malinowski 1966: 369), its *kula* network and position, as already indicated, are not as strong as that of Vakuta. In the whole of Kiriwina there are only eight canoes and few sailing or canoe-building experts (Malinowski 1922: 120–1). Although Omarakana was an important centre for the distribution of *kula* items in the *inland* network of the Northern Trobriands, it is rather difficult, from the evidence already presented, to find the basis for Uberoi's assertion that the *kula* is 'a sphere wherein Kiriwina is famous throughout the Massim area' (1971: 138).

Even greater problems arise when Uberoi's argument is applied to Sinaketa and Vakuta. Vakuta is the one place in the Trobriands where rank is hardly operative. Although there was a branch of the Tabalu *dala* in Vakuta 'their power here never assumed the proportions even of the small chiefs of Sinaketa' (Malinowski 1922: 69), and the position of Sinaketa chiefs was unimpressive compared with Omarakana. On the basis of Uberoi's analysis, however, one would expect Vakuta to have an elaborated ranking system.

Vakuta Island had four small villages and one large one. Although Malinowski does not give any information about the district economy of Vakuta, it is unlikely, given its size, that it would have been less than that of Sinaketa where 'the district economy is not very pronounced' (1966: 369). More importantly, Vakuta's gardens were much better than those of Sinaketa—Malinowski compares them with those of Kiriwina (1966: 295)—Vakuta was a village of great wealth (1966: 211) and its inhabitants were 'expert and skilled fishermen' (1966: 19)[9]. Vakuta was in a far stronger position in the *kula* than any other Trobriand community. It was the only one with overseas (i.e. outside the Trobriand group) *kula* alliances in both directions.

Some Vakuta men had partners in Sinaketa as well (Malinowski 1922: 276). There were twenty-two canoes in Vakuta, more than in any other district in the Trobriands and nearly three times the number owned by Kiriwinans and Sinaketans. It was also the best centre for canoe building (Malinowski 1922: 121–2).

In this context the evidence from Dobu should be included. For although Uberoi is only attempting to account for differences in rank within the Trobriands it is nevertheless important to test his argument against the Dobuan material in order to point out its inadequacy for dealing with the wider question of the atypicality of the Trobriands.

Malinowski states that the Dobuans inhabit 'fertile and flat foreshores' (1922: 41), and that they are 'very rich in food' (1922: 355)[10]. The Dobuans were very advantageously placed in the *kula* network as all the valuables had to pass through there (Fortune 1963: 202). Their external affiliations went well beyond the *kula*, however, as they engaged in trade with a number of non-*kula* areas (Malinowski 1922: 505). Despite all this there was no ranking or chieftainship on Dobu. Malinowski also thought that some Dobuan communities did not participate in the *kula* or else participated on a very small scale (1922: 275). Yet there does not appear to have been any diminishing of prestige as a result (see also note 4).

Powell hardly recognises that Trobriand rank and chieftainship present a problem, even in terms of differences within the Trobriands. He states that rank is highly advantageous 'in so small and densely populated an island' (1969: 601). But the population density of the Trobriands is less than that of Dobu Island (Powell 1960: 119; Fortune 1963: 18). Powell suggests that 'rank may be defined as an attribute which characterises the members of certain Trobriand sub-clans (*dala*) as the proper leaders in the localities with which they are associated' (1960: 128), and 'which entitles their members rather than others to acquire wealth ... the acquisition of wealth ... does not, on the other hand, in the traditional Kiriwinan view, entitle people to claim rank as *Guyau*, or membership of the Tabalu *dala*' (1965: 98).

However, the traditional Kiriwinan view is not at issue here. Powell has ignored the distinction between native models of society, models which may have an important political function in disguising reality, and an outside observer's analysis of the way in which that society operates.

Furthermore, Powell's exposition does not enable us to understand why there should be such variation in the ability of different local branches of the one *dala* to acquire wealth and power; for example the differences in the acceptance in Omarakana, Sinaketa and Vakuta of the local Tabalu *dala*'s claims to highest rank and the political capital that they can make of this. Neither does it permit us to explain changes in the rank of local *dala*.

The village of Suviyagila, consisting of only one sub-clan (Malinowski 1966: 346) was, in Malinowski's time, one of the despised endogamous group of villages in Kuboma (1932: 421). Yet by the time Powell carried out his fieldwork it seems that the Suviyagila *dala* had acquired *Guyau* rank. (See map, 1960: 123.) There is also the rather strange case of Wilaylima, once a commoner village For a period of two generations it became a Tabalu centre, but it returned to its original owners as the only Tabalu woman died childless. Nevertheless the village 'was distinctly raised in rank by the process' (Malinowski 1966: 365–6). Powell indicates that it

is a *guyau* village (he calls it Ilalima, 1960: 123). This may not, of course, describe actual historical processes, but it does indicate at least some recognition of the possibility of changes in rank.

V

Rank and resources in the Trobriands

Continuing on from points made earlier, I would suggest that the northern Trobriands—or more particularly Kiriwina district—somehow 'plugged in' to the *kula* ring rather late, at a time when the ring was well developed and had considerable fame and prestige. The process whereby Kiriwina became involved could well have been the sending of Kiriwinan women in marriage to Kitava (Malinowski 1932: 70) or Sinaketa and Vakuta (Malinowski 1966: 290–1). This would also have provided the basis for the propagation of the Tabalu *dala*. The extensive gardens of Kiriwina district could be used to provide *urigubu* payments to the husbands of the Kiriwinan women, and in return certain quantities of *vaygu'a* would be sent by the husbands to their wives' brothers (Malinowski 1966: 199, 290; see also 1932: 76–7). Maybe only one or two men were involved initially—but the fewer the better in terms of the development of rank and chieftainship. These men would then be in a position whereby those farther inland wishing to gain access to the *kula* would have to operate through them, most likely by sending their sisters as wives. By sending their sisters to marry Tabalu men others would also get access to the *kula*, but as mentioned earlier there were some restrictions on marriage choices, and the overall dispersion of *kula* valuables could be confined.

I am suggesting that it was the successful exploitation of what Bailey would term a 'new resource' (1969: 35–7, 190)—one which already had considerable prestige through its utilisation in neighbouring communities—which provides the most plausible explanation for the unique development of stratification in the northern Trobriands. Participation in the *kula* became the basis of social differentiation with political success based largely on success in the *kula*. Under different circumstances, although the exploitation of a new resource may have had important consequences for the social position of the men initially involved, over a period of time a gradual dissipation to others in the general area would have occurred[11]. That this dissipation could be averted in the Northern Trobriands was a consequence of a number of very important factors.

One of the most crucial of these was that, apart from its fertile gardens, Kiriwina district, alone amongst all the places involved in the *kula*, did not manufacture anything of value to other Massim societies, had no experts such as canoe builders who could hire out their skills and did not engage in any side trade of significance. It was thus in the unique position of being able to close off the exchange system and erect barriers to the convertibility of *kula* items. In all the other *kula* communities for which we possess information, control by any one individual or group of high valued resources, and the maintenance of barriers against convertibility, were far more difficult. We can see this quite clearly in Seligmann's list of transactions involving axe blades (1910: 531–2, see also 536–7), as well as some of Malinowski's material (1922: 499, 502–3). In Kiriwina district and its surrounds therefore, the entrepreneurial opportunities for other men to exploit through trade etc. were

very limited. Furthermore, as mentioned earlier, the fixed direction in which *kula* valuables moved and the tenuousness of Kiriwina's hold over the *kula* system also operated to facilitate this control.

It might be objected that, although the exchange system in Kiriwina may be more controlled than elsewhere, environmental conditions are irrelevant, and the situation has arisen as a consequence of the development of rank and chieftainship. However, there are two very strong reasons for arguing that the elaboration of a system of hereditary rank and chieftainship in Kiriwina was dependent on the possibilities that the environment offered for control over resources and the exchange system. That is, certain political strategies could be successfully carried out in Kiriwina district but were less effective or impossible elsewhere.

First, the geographical position of Omarakana is such that it is at a considerable disadvantage for the exploitation of resources. Kiriwina district does not have access to the Trobriand lagoon, or to other places where the shells used to manufacture the necklaces and armlets can be found (Malinowski 1922: 367; 1966: 21). Omarakana has only indirect access to the open sea, from which it is separated by a strip of high ground. Furthermore its rather weak position in the overall *kula* network is a result of external conditions, not internal political strategy.

Secondly, although a branch of the Tabalu *dala* is present in Omarakana, Sinaketa and Vakuta, there are great differences in the local effectiveness of their claims to aristocratic status. These differences are in line with what we should expect if we accept that it is environmental conditions, rather than a ranking ideology *per se*, which enables the closing up of an exchange system[12].

Having established themselves, there can be no doubt that the Omarakana Tabalu took steps to consolidate their economic and political position. Pigs, the supreme item of convertible wealth in Melanesia, were concentrated in the hands of men of rank (Austen 1945: 20, 23: Malinowski 1926: 90). The pattern of marriage preferences also operated to limit opportunities, for although food could be converted into valuables through return payments for *urigubu*, there was a tendency for people to select spouses of similar rank (Malinowski 1932: 71, 385). Nevertheless it was the geographical and ecological position of Kiriwina which permitted the control to be established in the first place and which enabled these measures to become effective.

VI

Conclusion

I have suggested that the presence and elaboration of chieftainship and rank in the Northern Trobriands can best be understood in terms of factors which permitted the development of a more controlled exchange system than existed elsewhere. The opportunities available for ambitious men to create a 'fund of power' and strike out on their own were restricted, as the social and physical environment facilitated the existence of what Douglas (1967) calls a 'coupon system'. Once such a system develops, however, the actual nature of the political contest may be masked (see e.g. Balandier 1972: 34); in my criticism of Powell I pointed out the necessity of going beyond native models. Trobriand chieftainship resulted not from an abundance of basic foodstuffs, but from a scarcity of valuables. This

scarcity was not simply a result of population density but was a consequence of the operation of external exchange networks and the distribution of non-agricultural resources. The reason why 'both the principle of rank and the power of the chief break down more and more as we move south' (Malinowski 1922: 69), is that communities in the south were better placed in terms of their access to the *kula* and other important resources. Monopolisation of political tokens by individuals or groups was therefore less possible.

Existing data, which would enable a similar analysis of other societies with rank and/or ascribed leadership, are fragmentary and incomplete. Nevertheless I will briefly review the material on some of these societies as it is at least suggestive of the approach I have outlined.

Seligmann wrote of chieftainship and ranked clans amongst the Roro (1910: 216–22) and Mekeo (1910: 342–8). His observations have been confirmed by the recent research of Davis with the Roro (1973; 1974) and Hau'ofa with the Mekeo (1971; 1973). Wealth items in Mekeo consisted of feathers, armshells and dogs' teeth (Hau'ofa 1973), but the origin of these items is not completely clear. Seligmann states that the people living in the mountains behind Mekeo brought feathers for trade to Mekeo villages (1910: 313–14). The armshells were made by the Hula, Aroma and Mailu peoples to the east of Pt. Moresby and carried by the Motu *hiri* expeditions west to the Papuan Gulf (Groves 1973: 104). However, it appears from both Seligmann's and Barton's statements that the *hiri* bypassed the Roro and Mekeo areas (Seligmann 1910: 204; Barton 1910: 107–10). Seligmann notes that the external trade relations of both the Roro and Mekeo were limited (1910: 204, 313). Davis states 'it is clear that the Roro and Mekeo are relatively recent immigrants to their area' (1973: 13, cf Seligmann 1910: 316); this would help explain the weakly developed trade relations with their immediate neighbours. Thus, although a considerable amount of further information on trade and exchange is required, there are at least some grounds for believing that the Roro and Mekeo may have been in a position comparable with that of the Northern Trobriands, having only marginal access to strategic valuables.

In Wogeo, in the Schouten Island group off the north coast of New Guinea, local units are divided into two, each with its own hereditary leader (*kokwal*). Hogbin regards the *kokwal* as being 'more than a headman, though less than a chief' (1935*a*: 318). Only the *kokwal* wears the boar's tusk ornament. 'Rank is bound up with boar's tusks' (Hogbin 1940: 25). But these tusks are neither made nor available on Wogeo. Like all ornaments, they are traded from the mainland. With the exception of food, Wogeo is dependent on items coming from elsewhere (Hogbin 1935*b*: 376). Trading trips are infrequent, occurring at five- or six-year intervals. These trips are initiated and controlled by the *kokwal* (Hogbin 1935*a*: 310, 320). Although after a 'lapse of two or three years the mainlanders reciprocate and in their turn arrange an expedition' (Hogbin 1970: 17) it is clear that Wogeo, dependent on external trade for wealth items, is not conveniently located in terms of gaining access to them. In a personal communication Ian Hogbin has accepted my suggestion that this facilitated a controlled exchange system and may have been an important factor in the existence of ascribed political office in Wogeo.

The societies of the New Guinea Highlands test my argument from a different angle. Despite high agricultural productivity and efficiency and an extremely high

population density in many areas, Highlands societies have not developed a system of hereditary social stratification and chieftainship. Although Highlanders were dependent on long distance trade for crucial wealth items—shells, salt, axes etc.—the nature of the trade routes was such that nowhere was it possible to monopolise and close up the trade and exchange system. The flow of these items indicates considerable diversity. Hughes, in his work on trade in the interior of Papua New Guinea states that although his maps indicate a linear flow of main wealth items 'these flows are simply the sum effect of myriad transfers of goods taking place in many different directions' (1971: 350). Discussing the opportunities for closing off the exchange system he says 'each personal network of trade was found to be an open system. When trading connections were combined with connections for prestation they covered the entire personal communication net. Each man's personal communication network was rested in a hierarchy of open systems in which trade took place' (1971: 359–60).

The general convertibility of valuables, the multi-directional nature of their combined (if not, in all cases, individual) flow and the general importance and accessibility of pigs ensured an open trade and exchange system throughout the Highlands. This is clearly apparent from Hughes's maps illustrating the flow of the main valuables (1973: 102–3, 106–7) as well as his statements and those of others (Hughes 1971: 137–46, 350–1, 359–65; Brown 1970: 100, 108–11; Langness 1971: 311; Meggitt 1971: 193–5; Reay 1959: 104–10).

Reviewing the material on Melanesian exchange systems Brown states that the egalitarianism of most Melanesian societies should be explained in terms of the 'realm of religion' (1970: 115) rather than the economy. I have argued the contrary position. Cultural prescriptions of rank and ascribed leadership are secondary. Given the fundamental importance of exchange in Melanesian politics, we must focus on the trade and exchange systems and examine whether the main wealth items can be controlled and given limited circulation and convertibility if we are to understand the origins of hereditary social stratification and chieftainship. This control is a cultural act, but the key to its being a successful strategy needs to be located in the physical and social environment—the extent to which a society has direct access to durable resources and the nature of its external exchange relations. Where conditions are such that men can act to limit strategic exchange items and pre-empt others from gaining access to them, then the stage is set for the development of rank and chieftainship.

NOTES

This article is a shortened and revised version of a M.A. thesis presented to the University of Sydney in December 1973. I wish to express my gratitude to my supervisor, Dr Michael Allen, for his invaluable comments, criticisms and encouragement and also to Dr Dawn Ryan for commenting on various aspects of the thesis.

[1] In his most recent article, Powell uses the term *Guyau*, rather than chief, to refer to the leader of a high ranking (*guyau*) subclan (1969: 602), but this term is obviously not suitable for comparative purposes. As Powell fails to provide a more generally applicable term, I have continued, like most other writers, to use 'chief' and 'chieftainship'. The point is to highlight differences between the Trobriands and other Melanesian societies rather than any necessary similarities with say, Polynesia.

[2] The area of Dobu Island is six square miles and its population at the time of Fortune's fieldwork was 1,200 (1963: 18).

[3] In any case, as Sahlins has recently shown (1972), high productivity is more likely to be a consequence of political evolution than a cause.

[4] Fortune states that in Dobu 'it is a mortal insult for one man to say to another "you are always on the sea and without new garden food". Gardening is the supreme occupation.' (1963: 201).

[5] Firth suggests that 'it would be interesting to relate this differential use of valuables to the differential rank structure of the respective communities, the chiefs in the Trobriands presumably retaining greater control than elsewhere through the lack of mortuary display and distributions there' (1957: 224). This seems to imply that in Kitava etc. the distribution of *vaygu'a* at the funerary feast is random. But Malinowski's account makes it clear that valuables are given only to *kula* partners, although people other than partners might be present (1922: 489–91).

[6] Cf. Firth's comment that 'the possibilities of competition in offering ... solicitary gifts ... amount in effect to a *sub rosa* haggling of a polite kind.' (1957: 221).

[7] However, they do cover the body of a dead person with *kula* valuables, but only for a few hours (Malinowski 1932: 130–1).

[8] They are further disadvantaged by the tendency of two neighbouring communities, Kayleula and the Amphletts 'to retain a number of valuables and be slow in passing them on' (Malinowski 1922: 360).

[9] Sinaketa was also an important fishing centre (Malinowski 1966: 17).

[10] Superficially there appears to be disagreement on this point in the literature. Fortune states that 'the Dobuan area is poor in gardens' (1963: 103) although he adds 'the Mission states that bumper native crops, such as are not known now, used to be harvested in the earlier days of the Mission in good seasons' (1963: 103). This discrepancy between Fortune's statements and those of Malinowski may be largely a result of depopulation and contract labour, a point that Fortune seems to have accepted in the second edition of his book (1963: 282–3). In any case, Dobu had sufficient quantities of at least one foodstuff—sago—to enable it to be exported (1963: 207).

[11] As was the case with the introduction of new types of shell valuables in many parts of the New Guinea Highlands (see e.g. Hughes 1971: 25, 113–16, 316–33).

[12] Ideally, a discussion of other Tabalu locales such as Kavataria, Kayleula, Gumilababa, etc., should be included. But the material available on the position of the Tabalu leaders, and the extent of participation in the *kula* is insufficient to do this. Armshells were made in both Kayleula and Kavataria, and both communities made extensive trading voyages to non-*kula* areas. These activities would make the closing up of the exchange system extremely difficult. However, we can get no indication about the extent to which *kula* exchanges related to these non-*kula* voyages and the importance of *kula* for internal prestige. Kavataria and Kayleula were, at most, only marginally involved in the *kula* (Malinowski 1922: 498, 501; 1966: 18), and may have had difficulty in getting access to *bagi*. Neither can we make any statements about the power and prestige of Tabalu 'chiefs' in these communities. However, by Powell's time Kavataria was no longer a Tabalu village (1960: 123).

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AN ENQUIRY INTO CHINESE SETTLEMENT PATTERNS: THE RURAL SQUATTERS OF HONG KONG

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The essay is concerned with how immigrants in rural Hong Kong settle as squatters on rented fields and hill sides. Informants refer their unwillingness to move into the villages to strategic interests. It is shown that these reasons are far from convincing. Rather, the settlement pattern may be seen as a representation of a notion of 'belonging'. Individual strategies may explain superficial variation of social form, but does not provide insights into the imperative forces of the culture. Informants' accounts of their motives are themselves strategically determined. It is argued that all possible propositions have to be tested contextually against all available knowledge pertinent to the situation.

I

In a sense this short article deals with anthropological explanation. The reader is warned, however, that there will be no theoretical development in abstract or philosophical terms. Rather, it is my intention to discuss one particular ethnographic problem in Chinese culture, a discussion which may have some bearing on more general anthropological theory. My subject matter is a settlement pattern, an observable structure and an effect of some sort of social process. It is my argument that we can tell very little about this process from our knowledge about the effect. In analysing observable structures as though they were the result of a strategic 'game' played by individuals seeking to maximise expected utility, anthropologists have often assumed that superficial cultural forms may be seen as phenomena of aggregation, an accumulation of advantages sought and gained. In this essay are discussed two problems connected with maximisation theories.

There is much anthropological concern with the study of how cognitive patterns are translated into social reality. Barth (1966), Bailey (1969) and others have approached the generative social process in terms of incentives and constraints affecting individuals as decision-makers in a social game. Barth stresses the analytical importance of 'transaction'. A transaction is the process which results where parties in the course of their interaction systematically try to assure that the value gained for them is greater or equal to the value lost. It is to be understood that values serve as generalised incentives and constraints on choice. Again, Barth insists that values can be determined only through the study of transactions; indeed, it is claimed that it is meaningless to talk about values outside the field of transaction. Value is maximised value. Here then is a main difficulty. Values are inferred from superficial sequences of social acts, values are what people strive for. But if acts and motives for acts are so fused we have said nothing more than that people strive for something because they strive for it.

Man (N.S.) 10, 559–570.

Obviously we have to untie the bond stipulated between 'value' and social interaction. What we should try to do is to link values to underlying systems of meaning; people strive for things, not because they strive for it, but because it makes sense in terms of a web of semantic relationships. Values, if we must retain this term, may preferably be seen as representations of systems of ranking which, again, are systems of meaning. Thus we can reduce the transactional apparatus from a too powerful explanatory model to a device which helps our understanding of variation of social form. Transactions stripped of maximised value become neutral interactions allowing for a great many motives. The main creative forces may be seen as imperatives founded in systems of meaning; maximisation contributes to the generation of social forms, but mainly as a factor of 'distortion'.

Thus we cannot hope to discover 'values' in terms of repetitive behaviour. Frequent events are often less interesting than the odd cases if the ethnographer is interested in the creative capacity of the cultural system rather than in institutions and customs. In order to establish a generative ethnography we have to take into account all cases regardless of their conformation to a standard of normal behaviour. Such an ethnography must of necessity be a series of postulates, a finite set of models which accounts for an open set of cases. Sometimes it may be possible to formulate the ethnography as a set of syntactical rules as Robbins Burling (1969) did to describe Garo household composition. Possibly it may be more useful to use a semantic approach—this would leave us fewer formal devices but greater hopes of finding out about cultural systems and their integration.

This leads to the second main point which my discussion of a Chinese settlement pattern raises. What do we do with our informants' pronouncements on the matters we study? This question of methodology reflects directly our position with regard to maximisation theories. For instance, Edmund Leach (1962) and others have taken Meyer Fortes to task because of his insistence on the morality of kinship. The critics have refuted the idea that morality is imperative; when informants stress notions of morality, the sceptical anthropologists see how they screen the fundamental forces which are the interests of the parties concerned. In a powerful defence of Fortes's position, Maurice Bloch (1974: 86) has stressed recently that we must not set aside in any way our informants' data:

> If informants stress the morality of kinship then *this* is what we must understand.

But if we turn Bloch's argument upside down, what then? If informants stress that they maximise gains, are we then to believe that they play a social game? Or are maximisation strategies referred to by informants as smoke screens to hide away moral considerations? There are no simple answers to such questions, and, indeed, there cannot be. Instead of trying to solve the insoluble I suggest we should take our informants' explanatory data into account together with other possible explanations and give them careful consideration in the light of all available knowledge we deem pertinent to the situation. What we can do is to hypothesise and argue, and again, counterargue to refute our own and our informants' propositions until we have exhausted all explanatory possibilities. What remains unrefuted we then may leave as a plausible answer to our query until further knowledge, which in turn questions our explanation, is gained.

II

The observations I wish to discuss in this essay were made in the Sha Tin valley of the New Territories, Hong Kong, between 1967 and 1969. The valley is an area of approximately 2,000 acres of low land surrounded by very steep mountains. Of old it is an agricultural area dotted with small compact villages, but today development has left large tracts of arable land abandoned. In contrast to some other areas of the New Territories (cf. Baker 1968; Potter 1968), Sha Tin has no grand-scale lineages inhabiting the villages; most of them harbour smaller lineage groups, either in an exclusive way, forming mono-lineage villages, or in a mixed fashion, as in the bigger multi-lineage settlements. Besides the native population there exists in the New Territories today a great number of immigrants, arrivals from China who have left their home communities, mostly after the establishment of the People's Republic in 1949 (cf. Aijmer in press). Their presence in Sha Tin is strong in numbers as it is in most parts of the New Territories. The appearance of these foreigners on the social scene is one of the more dramatic features of the area. It should be added that the presence of outsiders has been a rather constant feature, even before the Pacific War and the arrival of the Communists in south-east China; but their numbers today make them a majority of the population in most places, with the exception of the inaccessible mountain areas with their sparse settlements.

The predominant traditional pattern of settlement of south-eastern China pertains to the Sha Tin valley, and tight clusters of houses form villages and hamlets. From the point of view of ecology there are mainly two different settings which account for two types of villages. On one hand, the clusters of houses are located in the middle of the fields; like islands they stand surrounded by a veritable sea of green fields which were once irrigated rice land, now devoted largely to the cultivation of vegetables and flowers. Others are situated on the very rim of the fields, where the agricultural land meets the hill slopes.

The compact villages are scattered all over the area. The houses stand close together and form long terraces, separated by narrow alleys. The built-up area of the traditional village is sharply delimited from its surroundings. Sometimes it is marked by defence walls of brick, and moats, of a considerable size. A village may be segmented into a cluster of minor hamlets; these appear also as distinct units and are all built in a regular manner with clearcut rows of houses. Many village houses have been reconstructed recently and new houses have been erected in a local brand of modern style which, however, still reflects the traditional blueprint for a village house. Modern structures are often situated according to the traditionally established settlement pattern which does not suffer from the introduction of new style.

Only Tai Wai and the surroundings of Sha Tin Market have seen recently the coming of urban type multi-storied buildings, which, in the case of Tai Wai, reflect the development of this village, not only into a minor market town, but furthermore into a suburban satellite, annexed to the industrial and commercial areas of Kowloon.

An entirely different type of settlement pattern is formed by the new immigrants who are engaged in farming, gardening, and other rural occupations. They are 'squatters' in the area, living in small sheds and huts, officially labelled 'temporary

structures[1]'. These simple houses are all built of wood, asbestos, asphalt, and tin. Many are located up on the hillsides. Again, many sheds have been erected in the corners of rented fields, outside the traditional villages. Very few outsiders occupied by agricultural tasks will rent a house inside a village. The squatter huts vary in design and size. One may be a tiny place comprising a few square feet only, where one man, without any hope of sleeping comfort, may lock himself up together with his tools, seeds, and insecticides during the night. Others may have the appearance of small farmsteads with a few associated cottages, chicken yards, tool sheds, and pigsties.

III

In my field work in Sha Tin I devoted my main interest to the rural squatters of the area, a very heterogeneous group of people who are newcomers from China. In particular, I studied the Keng Hau area at the bottom of the valley. When in this article I argue about the settlement pattern of squatters, I do so with the rural farming population in mind.

The immigrants convert the old rice fields of the villages into vegetable and flower gardens. This change in land use has a number of implications; suffice here to note that the presence of new tenants implies a departure from traditional ideas concerning land and production.

The recent immigrants of Sha Tin have not settled together in village-like aggregates along traditional lines. Although immigrants may live quite close to each other, the loose 'neighbourhoods' their huts form, are, as I have demonstrated elsewhere (Aijmer n.d.), the result of processes very different from those behind the formation of traditional villages. The immigrants' huts are scattered everywhere on the hillsides and in the fields There are no obvious pragmatic or symbolic foci of interest which can cement these people together. Instead there are vague and flexible ideas of 'neighbourhood' with few organisational consequences [2]

Land does not mean too much to the Sha Tin immigrants in the sense that there is no special attachment to the rented lots. Land is something which is easily exchangeable. It is but a temporary resource to which the immigrant has secondhand rights only, and for a limited span of time. The immigrant family can move away, and be forced to move away, at any time. The latter point is of importance. Insecurity in possession of land is not likely to promote more permanent types of settlement. Once a land owner stops collecting rent the tenancy rights are cancelled. Suffice here to note that there is evidence that the immigrant tenant farmers of Sha Tin have no special ties to their land, and their situation is different from that of tenants in other parts of pre-Communist China.[3]

In the past, villages and village segments have emerged from squatter type settlements. San Tin originated as a small cluster of huts inhabited by tenants to land owners of Kāk Tin. It is a branch of the Liú lineage of Au Pui Wan far up in the mountains, and this fact has promoted, in all probability, the growth of a true village, once the economic circumstances allowed for the construction of proper houses. San Tin is still, in some sense, a segment of Au Pui Wan; they have no ancestor hall of their own and their exposure to *fengshui* 'geomantic' influences (cf. Freedman 1969) is conditioned by the setting of their ancestral home. The Liú

group of Tin Sam is another case. They were landless outsiders also, who settled just outside the defence wall of the fortified host village. Now the wall is gone and the Liú inhabit a terrace of village houses on the border of the village; to some extent they have become incorporated, it seems, but not in all respects.

There are a few more compact clusters of squatter huts in Sha Tin but they do not seem to be inhabited by people living off the land. The immigrant farming population prefers to live in scattered huts. But there is another hypothetical possibility which we must consider here. People could move into rented houses in the old villages. Why is it that the settlement pattern of isolated small farmsteads is the 'choice' generally preferred? Is it only a matter of strategy and maximisation of expected utility? Is the dominating solution based on economic considerations, or are there other social forces at play? When asked about residential preferences my informants in Sha Tin gave a number of suggestions to explain their own particular situations. Also, they gave general formulations referring to social strategies.

Very often the immigrant argues that the vegetable and flower gardener cannot afford to live in a village house because of the rents he will have to pay there. But instead he will have to meet other costs If he lives in a hut on Crown Land or on rented agricultural land, his main cost for housing will be 'tea-money', bribes payed to the police-like squatter patrols if the house is an illegal one, which is often the case. There are some initial expenses for the construction of a residential hut; the costs connected with this vary tremendously owing to the size and the standard chosen by the builder. It is difficult to arrive at good figures for such expenses. Often a dwelling of this kind emerges in stages by way of additions and extensions, and even the farmers themselves are unaware of the total sum of their expenditures. Again, prices have risen constantly during the last decades, and this fact makes a simple summing up of costs for materials and services, widely dispersed in time, less meaningful.

However, we have access to some information on how a man in Keng Hau, Chén Quán, erected his illegal structure in 1967. The building materials, cost him over HK$1,000. In addition to this he had to hire workers and pay off the squatter control inspector as a licence for a temporary structure was not granted by the District Office. He estimates his total input into this project to be some HK$3,000. In 1969, the squatter inspector asked him for additional 'tea money' of HK$800, but Chén was reluctant to pay this sum for different reasons. Altogether, 3,000 dollars is a vast amount of money for anyone who may hope to gain some 500 a month by way of his farming activities. Chén had to borrow money for this purpose, which was possible through relatives of his wife in Hongkong and Kowloon. In 1969, two years later, he still owed them some HK$2,000. Chén's house is a wooden one. Had he preferred a 'stone cottage' of concrete the cost would have amounted to four times as much as he paid for the present dwelling.

Again, there are some other costs connected with a temporary structure. Often such a hut will cover a substantial piece of arable land, which is rented at a price. In fact, the land owner may ask for higher rent for land which will be used for house construction. This happened to Chén Quán. The owner of the plot where he erected his house, raised the rent from HK$100 to HK$250 because of the shift in land use. We must keep in mind also that land occupied by a structure does not give any economic return. This means that a hut is connected with a rent similar

to that for a village house, especially if the land rent is raised because of the hut. In fact, Chén pays some HK$65 a month, besides loss of produce, as rent for his dwelling, which occupies about a quarter of the field.

Although there are violations intermittently, as we have seen above, the general rule is that bribes to the squatter patrols are paid once and for all. For legal structures there is an annual small fee due to the District Office, which should not mean too much for the occupant. At Keng Hau such fees vary from HK$5 to HK$19.

To live outside the villages should be part of a process of maximising gains. Quite often it is stated that the preference is in fact an economic necessity. What then do people have to pay for accommodation in village houses? As it appears, rents are quite low. In the cases I know of there is a variation ranging from HK$30–HK$100 a month for a house. An argument in favour of this proposition is not a convincing one. Again, against the economic strategy interpretation it can be pointed out that many other people in this area, who are not engaged in farming and who do not occupy village land but work in the big cities or in the market towns, or are engaged in industrial enterprises of the Sha Tin valley, rent houses or flats in the villages. Their incomes would hardly be higher than those of the vegetable and flower growers of the area; rather the contrary. Nevertheless they can afford to live in village houses. Furthermore, living inside the residential area of an old village would provide advantages, like the use of common facilities such as bath houses, but above all, the advantage of intensified social interaction, leading to broadening of the immigrant's social network.

Thus we may say that, generally speaking, the immigrant farmer can afford to rent a house or space in a house inside a village. His preference for not doing so, but rather for building a hut in the midst of his rented fields, or on the hill side, cannot be explained in terms of economic strategy.

Another point, which has been raised in the literature also (Baker 1968: 83, Grant 1962: 119; Potter 1968: 66), is that the vegetable gardener has to live beside his valuable crops in order to protect them from theft. From the hut in the corner of the field he would then have a good view over what is going on around. He may own a dog, the barking of which at suspicious signs will wake him up during the night, and also scare those sinister nightly visitors who lurk in the darkness. This reference to a strategy of protection, however, is not a very powerful argument. Many vegetable farmers have their rented fields scattered over a large area and few of them are within immediate reach. Therefore they are not protected by an ever-present farmer. Furthermore, we have seen above that not all the immigrant vegetable and flower farmers live in their fields, but many of them have taken up residence on the hill-sides, often a good distance from their gardens. Some people do not live in the neighbourhood of their fields at all. A great many gardeners who cultivate fields around the village of Siu Lek Yuen, live at Sha Tin Market and travel all the way to and fro by bicycle along narrow paths. This is a ride of more than ten minutes. During the night there will be no one around in the village to protect the crops. So far as I know, no arrangements have been made for a hired guard or the like.[4]

At Keng Hau people may talk about thefts of vegetables generally, but at the same time they recognise that such stealing is very rare. Some five years before, a few outsiders used to raid the vegetable fields; they were opium smokers, and steal-

ing crops seems to have been one expedient for getting funds for visits to the opium divans of Tai Wai.

Indigenous villagers who still occupy their inherited houses and have switched over to vegetable gardening, do not seem to fear for their crops, although they reside inside the villages. Thus, the risks from theft and crop destruction do not seem to give rise to a strategy which would generate the particular settlement pattern pertaining to the immigrant farming population.

A somewhat better argument may be found in the frequent pronouncement that the farming family's work is not directed entirely towards the growing of vegetables and flowers, but also towards the breeding of pigs and chickens. It is suggested that a man who rents a house in a village, will not find it possible to let his wife and children engage in animal rearing; thus, a village residence will cut him and his family off from an important source of income.

Against this proposition may be argued that, in fact, only a few immigrant farming families engage in pig-breeding. At its best, this business is unprofitable owing to heavy competition, accompanied by low prices, caused by the massive import of animals from China. The same conditions are true for chicken breeding. Considerations in connexion with a differentiated economy may well be decisive in the choice between erecting a hut in a corner of a field, or locating it up on a hill side. In a location of the latter type, more space will be available for the construction of pigsties; agricultural land is generally far too expensive to use for animal breeding purposes (cf. Grant 1962: 119).

In the 1950's, when pig rearing was a very profitable trade, all villagers used to keep a few pigs. Then the animals were running around in the village freely. This can be observed in some old-style villages in the New Territories even today. Surely, there can be no serious objection on the part of the villagers to having pigs inside or in the vicinity of the village Although there may be difficulties in locating a more extensive pig breeding business inside the confines of the residential area, there will still be solutions.

However, the main argument against the general validity of this particular reason for the residential segregation of land owners and immigrant tenants lies in the circumstances that although a tenant is completely uninterested in the raising of pigs and chickens, he still lives outside the traditionally established village unit. It seems to me that considerations connected with animal breeding cannot be regarded as a decisive factor.

Another point of view, raised by villagers, is that in a village house there will not be enough space for a gardener, his family, and his tools; a residential landlord will not like to see his house crammed full with men and things. This reasoning, however, could hardly be reckoned a strong argument as the same landlords do not mind city people jamming their houses as residential tenants, nor do they mind the subsidiary cottage industry tasks which are performed inside the houses by the city-workers' wives and children—like the plaiting of plastic handbags and baskets, and the assembling of plastic flowers. The vegetable farmer's few tools would seem fairly tolerable in comparison with these 'home factories'. I believe that this argument has little explanatory power.

Again, it has been proposed that the vegetable farmers much prefer to live in the close vicinity of their fields in order to gain more time for work on their gardens.

It is true that work in the fields is performed from dawn to dusk with breaks for morning tea, the midday meal, and a nap. Time is a most important factor. However, this does not explain too much if we think of the hill-side dwellers who may have a long distance to walk to get to their fields and who would be much closer if they lived in a village. Again, we should consider the indigenous farmers who have taken up the cultivation of vegetables. They do not find their living in the old villages of any inconvenience. As a matter of fact, the distance to the fields from a house in a village is not very different from that from a farmer's hut, widely scattered as they are. Surely this matter of convenience cannot be a decisive factor in a choice between the two forms of settlement.

However, we cannot deny that the time factor is particularly important, and this draws support from comparative data. From the Canton hinterland, for example, it is said that

> Practically all the people outside the city of Honan live in villages, altho a few of them lodge for a time on the fields during the harvest season (Feng & Yung 1931: 160).

Thus, living out in the fields has been an expedient for the saving of time for badly needed work (cf. Potter 1968: 66). In a way, it could be said that vegetable gardeners live in a constant harvest time. On a level of conscious motivation the time factor may play a part, but it cannot be readily accepted as a main formative force.

Another suggestion often made is that the vegetable farmers prefer to live by themselves and not in the villages because they want to be as close as possible to the roads, which is of great advantage in the marketing procedure. Against this proposition it may be argued that what is more important is where the fields are located with reference to the road network; this should be the issue of concern rather than where the residences are. It is the carrying of the heavy vegetable baskets a long distance that matters.

We have found these arguments, presented by informants of Sha Tin, not entirely satisfactory. Of course, these propositions have an interest in their own right. On a level of conscious motivation, the feature of residential segregation between landlord villagers and immigrant tenant farmers is explained in terms of 'economic man' activities and rational strategy. It is not to be denied that such conscious reasoning may form a strategy to be employed in specific social and economic situations. But motivations of this kind have, as we have shown, very limited explanatory power. Indeed, rather they screen off the observers of the society, internal or external, from the more profound realities that pertain to the situation. In order to grasp these realities we must turn to comparative data. We find mention of settlement patterns similar to those observed in Sha Tin in some field reports from China; evidence of similar residential segregation is at hand from Shandong (Stenz 1906: 846), Anhui (Fried 1953: 52), Yúnnán (Fei & Chang 1947: 93), Guǎngxi (Laai 1950, cited by Cohen 1968: 254), and Guǎngdong (Vömel 1914: 598; Chen 1936: 31). It must be understood also that the settlement patterns observed in Sha Tin are to be seen in large areas of the Hong Kong countryside. It is clear that what can be observed today is by no means an innovation in the social landscape (vide *N.T. Report 1935*, J2). There is some sort of regularity here in that outsider tenants, in these provinces of China, tend to reside away from the established villages.

IV

We must assume that one key factor, producing such social effects, is the conceptual apparatus related to land. 'Citizenship' of a village is defined clearly in terms of land, as is obvious by many pronouncements by informants of Sha Tin, as well as the comparative material at hand. The acquisition of rights to land is a prerequisite for the gaining of rights within the sphere of community life. Considering a situation in Yúnnán the ethnographers write:

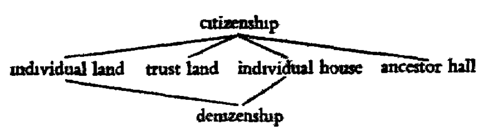
> It soon became evident that the real criterion was the possession of land. Those immigrants who acquire land are then, and not until then, accepted as members of the community, while those who remain landless continue to be regarded as outsiders. In this agricultural village, as in other similar sections of China, ownership to land is a necessary qualification for complete integration in the community (Fei & Chang 1947: 93).

Similarly, an account of village life in Panyú District in Guǎngdong, close to the city of Canton, tells us:

> Aside from members of the clans, Nanching had a few families and individuals of other surnames who were temporary residents of the village or agricultural laborers from outside, none of whom owned land or a house in the village, and all of whom were regarded as 'floating elements,' 'sojourners,' or 'guests' whose roots of life did not belong there. . . . membership in them [the clans] was not open to subscription. Hence, as a general rule, the village as a unit of permanent community life was closed to outsiders (Yang 1951 11).

It seems to me that the further 'in-group' qualification of this last pronouncement is most important—rights to houses was one criterion of membership. My thesis is that citizenship of a village is achieved only when a man is endowed with rights to village land and a village house within the defined residential space of the village. We shall qualify this further below. But we must understand also that an approximation, implying the possession of these rights second-hand, may be enough for obtaining a kind of citizenship. For instance, we may consider the case of the serf families who existed in some of the more powerful and wealthy villages of the New Territories, and Guǎngdong at least. The location of their houses at the rear of the village may be seen as an expression of their status as accepted residents and yet not full members of the residential group (Potter 1968: 20; Baker 1968: 155 sqq).

Both land and houses are crucial elements in the Chinese ideology of agnation. The former category of village property consists of private, individual land on one hand, and of trust land, assigned to patrilineally defined lineage groups, on the other. Similarly, residential buildings stand as an expression of individual rights to houses as contrasted with ancestor halls which stand for shared lineage (or lineage segment) rights. Full citizenship must include rights to trust land and to ancestor halls. A person with private land and a residential dwelling may be counted as belonging to the place, although he had no ancestral roots there—he obtains a 'denizenship'—but such naturalised outsiders in a village, like tenants, shopkeepers, and servants, could not enjoy all in-group privileges.



However, it seems that a denizenship may be a first step toward full membership; in the end it can be transferred into a citizenship by certain ritual acts. My data on this are very vague and far from conclusive. Tentatively it may be suggested that such an acceptance of a man and his progeny will, in a traditional Chinese setting, imply his right to set up an ancestor hall and form a land trust.

My argument is that as an outsider in a village you can rent either a house or a piece of agricultural land, but you cannot have both. If a newcomer should be allowed access to both kinds of property, and thus access to both of the essential spheres of village life, he could soon establish himself as a denizen, and, in the end, claim citizenship and a voice in village affairs. As a general principle, this is something to be avoided. Thus, people who have city jobs or work in the industrial enterprises of the valley may gladly rent houses and 'flats', while those who do not nourish a wish to live inside the village residential area may gladly rent land.

Chén Quán, a gardener in the Keng Hau neighbourhood, may be a test case for this proposition. When he arrived in this area of Sha Tin he began by renting a piece of land belonging to an owner surnamed Cai of the village of Tin Sam. At the same time he took up residence in a rented house in the village of Sheung Keng Hau. He shared this with another family. Later, when the latter moved out, the Chén remained on their own. Suddenly his residential landlord claimed that he 'wanted one room back as he was moving back to the village and he had nowhere else to stay'. 'Since this was inconvenient and unreasonable', Chén's family moved out and left the village His unwillingness to give a room to his landlord seems a bit surprising at first as he had earlier shared the house with another family. But we may find an explanation in the fact that meanwhile Chén had expanded his farm. Zhaò Huŏsheng of the same neighbourhood rented land from Sheung Keng Hau. Zhaò felt that he had too much land and was eager to cut down his farm to a more manageable size. He found that Chén Quán was looking for more land, and it was arranged that the contract was passed on to Chén. However, now Chén rented both a piece of land and a house from Sheung Keng Hau. Surely, this was the reason for his sudden exposure to residential inconveniences. Then, Chén brought his family to Tin Sam. Again he infringed the rules of citizenship. His first land belonged to Tin Sam. This time his fields were sacked: 'Villagers and especially those younger ones stole the vegetables deliberately.' 'At this point', he complains, 'friends in the area advised me to build a hut on the present spot.'

Similarly we may note that when Wéng Guìyou first came to Sha Tin, he was employed as a worker in the area; during the first year he lived in Sheung Keng Hau, from where he moved to the neighbouring village of Ha Keng Hau. Although he was a well-known figure in the area by then, he moved out to a hut when he decided to switch to vegetable farming. The land he rented was then part of Ha Keng Hau.

In this article I have pointed to the fact that there exists among the rural population in this valley of the New Territories a residential segregation. Those immigrants who occupy themselves with horticulture on rented fields of village land, take up residence in small huts or sheds, either located in corners of rented fields, or scattered around on the adjoining hill sides. We have also found that newcomers to the valley who have occupations which are not concerned with land in any respect, move into the natives' villages where they rent houses or rooms. The main

reason for this conspicuous peculiarity is to be found in an underlying concept of citizenship. Certainly, citizenship is a most complex feature; what matters for the present argument is that it may be defined in terms of land and houses. In order to maintain the immigrant's identity as an outsider, his access to rights to land, even of a temporary nature, will never be accompanied by any kind of rights to residence. Similarly, residential tenants will not be allowed to take an interest in village land. In this way, traditional semantics influences the formation of the modern social landscape of the Sha Tin valley.

In conclusion then we may say that the settlement pattern that is observable in the Sha Tin valley cannot be explained by a performance model of rational decision making in a social game of the kind suggested by Barth and Bailey. We cannot infer the underlying motives from what we see and experience, nor can we trust the informants' suggestions and explanations. In fact, they can be refuted as untrue or, at least, as unconvincing. Instead we have suggested that the dispersed settlement of the squatters is produced by an underlying concept of citizenship. Such a notion can be inferred neither from what people say and think about their social situation, nor from the settlement pattern itself. Only by postulating this concept can we form a hypothesis which, when tested against the data, seems to account for the formation of some settlement patterns in the New Territories, and in traditional Chinese society generally.

NOTES

I wish to thank Anthony R. Dicks for comments Names and terms are rendered in Standard Chinese, with the exception of local Hong Kong names which are given in the official Cantonese versions

Fieldwork in the New Territories was conducted from June 1967—February 1968 and June-September 1969. Generous financial support was given by the Swedish Research Council for the Humanities I am most thankful for this help.

[1] This term also embraces more pretentious buildings Actually, it refers to the nature of tenancy.

[2] Shunde is a silk producing area in the Pearl River delta where farmsteads stand isolated in the fields. The same feature is true for the Chéngtu plain in Sichuan. I know too little about these areas to pursue a comparison See Trewartha 1939 9sq, Baber 1882 9, and Richthofen 1903: 181

[3] Permanent tenancy was common in Guǎngxi, Fújiàn, Zhèjiang, and southern Jiangsu, and was not unknown in Guǎngdong. See Freedman 1958: 15sq., who also provides further references.

[4] Crop protection guards were common in traditional Chinese society; see e g Stenz 1906: 857, and Baker 1968: 79 who also provides some further references.

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LABOUR MIGRATION AND THE BASSARI: A CASE OF RETROGRADE DEVELOPMENT?

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The development of seasonal labour migration from the Bassari village of Etyolo is the subject of this article. Village agricultural and social systems are shown to have been instrumental in mitigating the potentially detrimental effects of migration while allowing the village to benefit from the wage economy.

This period of beneficial migration may be ending, however. Analysis of the growth of recent long-term movements to town supports the hypothesis that Etyolo may become dependent on outside wage-labour for cash while simultaneously losing its present ability to feed itself.

As an example of the potentially disastrous long-term effects of the incorporation of small-scale communities into larger economies, a plea is made for recognition of the underlying economic causes of rural-urban movement and for local-level action programmes to enable villages like Etyolo to retain at least some measure of self-sufficiency and choice.

I

Introduction

The abundant literature on labour migration in developing countries has dealt mainly with three interrelated themes: (a) the basic causes of rural-urban movements; (b) the implications of such movements for urbanisation and industrialisation; and (c) the effects of these movements on the rural areas of origin (cf. Grillo 1969: 77). This article, while not neglecting aspects (a) and (b) above, will deal mainly with the effects of labour migration on one rural community in west Africa.

The arguments advanced here can best be seen within the context of theory and empirical findings on the effects of migration on rural units in sub-Saharan Africa, although there will undoubtedly be many points of comparison with other areas of the world. In many of the community studies from Africa, one finds mainly negative effects reported from migration. Certain specific results seem to predominate, many of which appear linked to each other. Thus, a rapidly-developing age/sex imbalance is reported (Schapera 1947: 183; Richards 1939; DeGonzalez 1961: 1270sqq; Gutkind 1962; Finnegan 1965: 134–7), frequently with negative effects on family relationships. In connexion with this other indications of 'breakdown' in rural societies are sometimes noted, including alterations in systems of power, authority and social control. A decline in rural agricultural production is mentioned (Diop 1965: 905; Schapera 1947: 168; Richards 1939: 397), and malnutrition is sometimes observed (Richards 1939). Further negative effects include reports of 'rural shirking' or 'rural urbanism', where returned migrants refuse to work in the village and use it mainly as a place to rest between trips to town

Man (N.S.) 10, 571–588.

(Wallerstein 1965: 154–5; Skinner 1960: 385; Richards 1939: 404; Schapera 1947: 163–6).

In contrast to these negative findings, several studies have appeared which emphasise the positive aspects of labour migration and stress the successful nature of village accommodations to a wage-labour economy (Watson 1958; Van Velsen 1960). Mitchell, analysing variations in local response to migrant labour among the Bemba, Mambwe and Tonga peoples (1958), produced convincing arguments to the effect that differential reactions to migrant labour can be explained by examining local patterns of economic and social organisation. The implication was that some types of societies are more susceptible to the absence of migrants than others. In a later paper, however, he warned of the dangers of over-simplification with regard to the relationship between labour migration and rural 'breakdown', stressing that observed effects were often the result of multiple causality (1959). Others have since mentioned the difficulties in distinguishing between cause and effect when considering migration and change (Schapera 1970: 155–62, 231–6; Skinner 1965: 69; Finnegan 1965: 133).

The search for more subtle and comprehensive models of explanation and analysis cannot, however, change the fact that many rural communities do experience short- or long-term difficulties wholly or partially as a result of labour migration. Since many observers believe that one of the most compelling reasons for rural-urban movement is a lack of cash-earning opportunities in rural areas (cf. Hutton 1970: 14–16), it is generally assumed—or hoped—that local cash-cropping schemes will act to stem the rural exodus (Mitchell 1959, 1961; 1969; Gulliver 1955; 1957, 1960), although several writers have questioned this (Colson 1960: 111–13; Elkan 1960: 33–41).

The present article will address itself to some of the general issues raised above by examining the relationship between labour migration and change in the village of Etyolo, a community of Bassari subsistence farmers in eastern Senegal. Using data from my field investigations, I will argue that at the present time Etyolo appears to have adapted 'harmoniously' to the incursions of a migratory wage-labour system. Village social and economic structures remain fundamentally unaltered and permit people to exploit advantages on both sides of the dual economy in which they now live with little if any apparent dislocation.

If this were all, it would be yet another situation of successful adaptation to change and as such would function as an interesting and optimistic example of the 'development', 'modernisation' and 'integration' of small-scale communities into the wider world. Any such optimism stemming from my first line of argument would be premature and unjustified, however, as I propose to argue further that such accommodations as we see at present in Etyolo village are more apparent than real and are temporary rather than permanent. On the basis of my data and observations, I believe that the village will soon develop a relationship of dependence on migratory wage-labour and that this dependence will generate a downward spiral which can in no sense be called 'development'. I will conclude by arguing that the factors responsible for this future shift in rural-urban relationships will be—and in fact already are—located outside the village, and hence outside its control.

The trends which I will describe—and which are already well in train in this

community—raise profound and disturbing questions at all levels for those concerned with the study, planning, or implementation of social and economic change. This is particularly the case for those who prefer to characterise the evolution of village societies like Etyolo as examples of 'integration' or as a 'transitional phase', for the question which must be answered is, 'integration on what terms?', and 'transition to what?'. For the anthropologist and the development planner, this raises dilemmas of choice and judgment with which we are all familiar and which are, in the end, probably not capable of satisfactory resolution. But the possibility—which I consider a very real one in this case—of the eventual disintegration and dispersal of a traditional village community must surely encourage renewed haste in the search for acceptable alternative solutions.

II

Bassari society[1]

The Bassari inhabit an area lying across the frontier of south-eastern Senegal in the foothills of the Fouta-Djallon massif below the Gambia River. Their territory (known locally as *pays bassari*, or Bassari country) is enclosed by the Niokolo-Koba National Park to the west and the north, and by other ethnic groups (Peul, Malinké and Bedik) to the east. The Bassari number approximately 3,000 in Senegal, with a further 4,000 or so found in the Republic of Guinée on the other side of the frontier. Access to Bassari country in Senegal is by road from the Departmental capital of Kédougou, 90 km distant; this road is closed by the rains from June to December. The immediate area, apart from the small *arrondissement* centre of Salemata, is composed of small villages of Peul and Bassari subsistence cultivators, living in scattered homesteads on the stony hills and ridges of the sparsely wooded savannah.

Etyolo, the largest of these villages (population 484), was the focus of my study. Twelve kilometres from Salemata, it consisted in 1972 of 51 households scattered over an area of more than 20 square kilometres. Everyone in the village (with the exception of the retired canton chief who receives a government pension) is a cultivator, with households being the basic units of production and consumption. Millet and earth-peas are the staple crops, but a variety of other things are grown, including maize, beans, rice, *fonio* (a short local grain), manioc, taro, tobacco and calabashes. There are a small number of secondary occupations in the village—ironsmith, potter, curer and palm-wine tapper—which are practised for gain, either in money, grain, or animals, but only the palm-wine tappers derive a steady or substantial income in this way. Small numbers of chickens, goats, sheep and cattle are kept by each household.

The village—like the rest of Bassari country—is isolated socially, politically and economically from the rest of Senegal by reason of geography and history. Situated in one of the poorest and most remote regions of the country, it was among the last places to be contacted and pacified by the French in the early 1900's. Prior to this time, the Bassari were engaged in bitter skirmishes with the invading Peul, who attempted to convert them to Islam. The arrival of the French put an end to the fighting and established *la paix française*. At the same time came taxes and a form of indirect rule which was to endure until Senegal's accession to independence in

1960. Apart from the construction of a few minor roads and several schools, the area was—and remains still—relatively untouched physically by the outside. Today, although extensive contacts have developed with the major towns of Kédougou and Tambacounda, the two-room government school is the only immediately recognisable external presence in Etyolo; there are no offices, airstrips, industries, trade stores, public wells, markets, missions, or administrative centres in the village.

The Bassari are separated from the rest of Senegal by more than geography or level of infrastructure alone. They are predominantly pagan with a Christian minority, while the rest of Senegal is more than 80 per cent Muslim. They are matrilineal, while many of the larger ethnic groups of Senegal are patrilineal. They are primarily subsistence farmers, whereas most of the north and west of Senegal is characterised by the cash-cropping of groundnuts. The Bassari are basically an egalitarian society within a framework of highly-structured age-classes; the dominant groups in Senegal have a system of endogamous occupationally-based castes. It is perhaps not surprising, therefore, that the Bassari do not really think of themselves as a part of Senegal, and refer to a trip to town as 'going to Senegal'.

The society is essentially a gerontocracy organised along quasi-military lines In Etyolo, there is a highly-structured system of male and female age-classes, with the bulk of power and authority vested in the older groups. It is this institution more than any other which gives form and substance to village activities. Entry into the age-classes occurs at about 6–8 years of age for both males and females, and class progression, along with a complex set of accompanying rights and duties, continues at six-year intervals until death. The most significant aspect of the age-classes in the present context concerns a corpus of ritual duties intimately associated with the agricultural system, called *a-tywin* in Bassari and referred to as *la coutume* (the custom) by those who speak French. From the time young men are initiated (at about 16–18 years of age) until they enter the youngest class of 'elders' eighteen years later, they are expected to carry out a prescribed number of ritual tasks each year, nearly all of which involve extensive cultivation of village fields. Women perform similar tasks at this time as well, although in general their load is lighter.

Age-class labour is thus not only an essential prerequisite for advancement in Bassari society but also makes a major contribution to the agricultural economy, providing massive labour inputs at specified times for the benefit of all. Bassari agriculture is reckoned by many outside observers to be both efficient and productive given local constraints; Dupré, who has studied the system of age-class labour, has spoken of the system in Etyolo as an '*agriculture perfectionnée*' (1965: 152). This is all the more remarkable in view of the fact that inputs from extension workers have been practically nil, and that no agricultural innovations are presently used in the village. Etyolo's villagers do not use ploughs or draught animals (relying instead on locally-made hoes and digging-sticks), practise neither terracing nor irrigation, and make only limited use of natural fertilisers.

Nevertheless, the agricultural system is impressive, and the main constraints appear to be the generally poor soils and periodic dry years. The factors which account for its success appear to be an ample supply of land within and without the village, a careful system of crop rotation, and assured labour inputs at the crucial times of field-clearing, seeding, weeding and harvesting. The Bassari practise

shifting cultivation for their staple crops of millet and earth-peas and alternate plantings of each in a particular field until it is exhausted over a period of 4–6 years, at which time new fields are cleared and the old is returned to fallow. In addition to the inputs from the younger age-classes as part of *la coutume*, a well-organized system of communal labour among older men and women is also practised within bounded areas of the village. Although there is some sex differentiation as regards agricultural tasks, both men and women contribute continuously and more or less equally throughout the agricultural season. The heavier work of field-clearing and stump-cutting is usually performed by men. Most of this work is done during the latter half of the rainy season, in preparation for the next year's planting.

Although harvests can be exchanged and transformed in various ways for other commodities, and are also sold from time to time within the local area, no local cash-cropping exists. Up until the early 1960's a small peanut trade existed in the area which provided some revenue to certain households, but this has since disappeared. There is at present a small weekly market at Salemata, some 12 km from Etyolo, but participation by the village is infrequent and income gained is small. A larger permanent market exists in Kédougou, and demands for foodstuffs there and generally higher prices would seem to make this an attractive outlet for Etyolo's farmers, but the roads are so poor and transport so unreliable that only one village man (who has his own vehicle) exploits this situation at present.

Both age-class activities and the neighbourhood communal labour gatherings are reciprocal in nature. In the case of communal labour, this reciprocity is on a dual basis; there is an immediate 'payment' to workers by their host of millet beer and food, and there is delayed reciprocity as households exchange labour over a growing season. Generally, those households which contribute the most labour to others will average higher attendance at their own communal labour functions. Although as I have argued elsewhere (Nolan 1974) this system is generally 'economic' even for smaller families, it is obviously the larger households which benefit most from this arrangement in terms of the outlay-to-return ratio. The age-class series of agricultural work is also reciprocal, although in the long- rather than in the short-term. Age-class groups receive a measure of millet to keep for each village field they work; this is converted to millet beer and presented to the elders in a series of ceremonies later in each year. Present age-classes will be 'repaid' their labour—and their beer—by their sons and daughters of the next generation. This element of reciprocity is implicitly recognised by younger members of the community, one of whom said with reference to the age-class tasks: 'We do what our fathers before us did; what our sons will do after us.'

Etyolo's agricultural system is thus closely linked to the main aspect of social structure. It also appears to function well in meeting local food needs. The uncertainties of rainfall and the possibility of occasional blights appear to be the major factors which adversely affect production at the present time, but the diversity of crops grown (e.g., maize and manioc) provides some security against crop failure and tides people over the period before the staples ripen. I recorded few instances of grain-buying in the village. People stated that they often sold food to neighbouring Peul farmers or other outsiders in good years in order to pay taxes. Bad years—which come with some frequency—often make it impossible to sell grain for cash,

but do not usually result in prolonged hardship in the village. People have the option at these times of reducing ceremonies and rituals requiring the use of millet beer, on which an undetermined but appreciable amount of grain is expended.[2] There also exists an arrangement (*amb'mb'm*) whereby communal labour may be performed on credit, with beer and food being provided after the next harvest. Although agriculture is strong at the present time, corresponding opportunities for local cash-earning are so limited as to be practically non-existent, and it is in this context that the spread and impact of labour migration must be seen.

III

The development of migration

Labour migration in one form or another has been a feature of Bassari society for many years, but the increase in the rate of movement in recent times has been particularly striking. It is probable that well before the arrival of the French, individuals and groups migrated for purposes of trade. The French introduced two innovations, however, which encouraged the growth of movements out of the village. The imposition of taxes early in the 1900's developed a need for cash; and the introduction of forced labour and conscription in the 1920's removed many male Bassari from their home areas and exposed them to the peoples and goods of the outside. The massive recruitment of men for 3–4 month stints of forced labour on roads and plantations—which had stopped entirely by the mid-1950's—also provided some individuals with small amounts of money. From this period onwards there was a steady and growing seasonal movement out of the village in search of cash and hard goods. The earliest voluntary migrants from Etyolo were traders, adult men who exchanged or sold local foodstuffs for salt and cloth in areas of western Senegal, Guinée, Guinea-Bissau and The Gambia. Beginning in about 1940 men began to leave the village in small numbers during the dry season to work in areas at distances of from 50–250 km from Bassari country. These men usually found employment at a variety of traditional tasks, including bamboo mat weaving and palm-wine tapping.

The numbers of men engaged in seasonal labour jumped sharply around the time of independence in 1960, for reasons which are still unclear but which are almost certainly connected with the closure of the Guinée border, the growth of education in Bassari country, the falling-off of the peanut trade and the emergence of Tambacounda and Kédougou as Senegal-Oriental's largest and most rapidly-growing towns. It is also likely that a 'demonstration effect' operated at this time as well, as young men saw and appreciated the gains to be had from seasonal movement.

The post-independence period led to a number of important shifts in migration patterns. Large numbers of women began to migrate for the first time, at first in connexion with, and later independently of, the men. Migrants shifted from mainly traditional occupations in the smaller villages and rural areas of Senegal-Oriental to unskilled labouring jobs (for men) and domestic employment (for both men and women) in Senegal-Oriental's two large towns, and other migrants went as far away as Dakar to work. Most significantly, this post-1960 period saw the appearance of small but growing numbers of a new kind of migrant, one who did not return to the village after the dry season, but remained behind in town. These

urban-based migrants came to perform useful roles as mediators for their seasonal kinsmen by providing them with lodging in town and helping them find seasonal jobs.

By 1971 when my study began, seasonal migration was an almost universal phenomenon among the younger age-classes of men and women. Although small numbers of these migrants continued to stay behind in town each year, the vast majority were returning regularly to the village in time for cultivation. Because of the fact that such migrants held mainly unskilled and low-paid jobs in town, they did not return with a great deal of money and/or goods. Migrants gave as their main reason for going to town the desire to accumulate cash, clothing, and household items for themselves and their families. That economic motives predominated over any 'bright-lights' attraction of town itself is indicated by the fact that many migrants were eager to work in remote bush areas as highly-paid labourers for a geological survey team at several periods during the late 1960's. Apart from these lucky few—who earned upwards of US$30.00 per month—the more usual wage for a town-based domestic male or female at that time, as in 1972, was between US$4.00–10.00 per month.

My investigation of Etyolo's migrants (Nolan 1974) revealed that there was a clear-cut migrant 'career' for men, whereby a younger man left on seasonal trips before marriage in order to accumulate money and goods for bridewealth and the establishment of a household. After marriage, such trips quickly tapered off in the majority of cases, as the arrival of children and the growth of family responsibilities made absences more difficult. A large proportion of the clothing and hard goods obtained were distributed among family members and kinsmen; some migrants used cash to buy cattle as an investment, or gave cash to their parents to help with expenses. I recorded very few instances of migrants buying food with their earnings. Few migrants gave the necessity to pay tax as a prime reason for their migration, since in many cases, these same migrants said that money could be obtained by the selling of grain locally.

IV

The effects of migration on village life

Seasonal migration is now well-established in Etyolo as a strategy for earning cash, and nearly every younger member of the village will make at least three or four trips away during his or her migrant 'career'. Nevertheless, present-day migration patterns have had very little effect on basic aspects of village life. The two most important village institutions—agriculture and the age-class system—appear virtually unchanged in their basic operation. Returned migrants re-integrate well into village activities and have neither challenged the accepted order nor attempted to create alternative ways of doing things. The reasons for this are numerous and complex.

Etyolo's seasonal migrants are attracted, naturally enough, by aspects of town life and are desirous of the things that wages can buy; but for most of them their security is linked to the village and to local agriculture. Although their participation in the wage economy is intense and frequent, their longitudinal involvement with it—in terms of life-span—is largely temporary. They go to town to obtain specific things; they will return when they have them or when the rains come,

whichever is the earlier. If the low wages which they are able to earn appear to be a major factor in drawing them back to town again and again, it is the demands of agriculture and the age-class system—coupled with the general insecurity of town life—which compel them to return again to the village. The necessity to ensure the success of staple crops coupled with the intense ritual and agricultural labour demands imposed on the younger age-class as part of *la coutume*, and which are a pre-requisite for village acceptance and status, make it very difficult for young migrants to remain away from home for long periods. A variety of sanctions and punishments exists to reinforce this, including beatings, ostracism, and sorcery. The demands of agriculture and the age-classes, combined with a total lack of alternative ways of making a living or of gaining social acceptance and hence security, function in some respects like a total institution, thus ensuring a young migrant's continued participation in village affairs. Interestingly enough, the maintenance of claims to land, so important in many migration studies (cf. Van Velsen 1960; Watson 1958), does not appear here to be a major factor in inducing return to the village, since land rights cannot normally be lost through prolonged absence and are in any case vested in individuals; furthermore, land is not seen as a scarce commodity in Etyolo.

Other factors are important too in explaining migration's lack of impact on village institutions. Except for the period of forced labour and conscription, Etyolo's migrants have never been recruited *en masse* for schemes which required them to be absent for long periods of time. They do not earn high wages, and so their economic impact on the village is less than it might otherwise be. The richest migrants from Etyolo have returned home with no more than US$200.00 each, which is hardly sufficient to begin a business or to make heavy investments in goods. By and large, migrants do not learn trades which will be useful to them in the village, and so they are unable to offer skills which would compete with or alter established village activities in any way. Migrant capital, when not distributed to kin or used to buy non-durable goods like clothing, is invested in such things as bridewealth or cattle. Although the latter appears to be a relatively recent innovation, and both cattle and bridewealth may lend themselves to manipulation for prestige purposes, either form of investment tends to strengthen the village system rather than to weaken it.

Because they fit into a highly structured set of age-class roles at all times, returned migrants have little social impact on the village. It is mainly the younger classes that migrate, leaving the elders behind in continuing positions of authority and control. Although younger migrants are sometimes critical of the elders and of the age-class system generally, the fact that the system is heavily invested with value and that stringent sanctions exist for those who will not conform make outright disobedience extremely rare. Migrants, in short, can make very little in the way of either social or economic capital from their wage-labour experiences.

In 1972 the village appeared to be taking advantage of the best of both worlds. Because the vast majority of migrants returned to cultivate the fields, agriculture had suffered no apparent decline, and had probably benefited somewhat, since absent migrants consume no village food. The linking of agriculture with the age-classes promoted stability and continuity in village traditions, and the low opportunity costs involved in dry-season absences made seasonal wage-labour in remote

towns not only a possibility, but an extremely attractive one for most people. The village had gained the material benefits—albeit on a small scale—of work for cash while continuing to feed itself. The goods resulting from migration have undoubtedly raised the standard of living in many households well above what it would have been twenty or thirty years ago, and the continuation of the bulk of village traditions and ritual activities has continued to give meaning to peoples' lives. Undoubtedly there has been some dropping-off of various arts and handicrafts in the village as a result of the time spent on migration, and the village is almost empty of young people during the dry season, but these are facts lamented far more by missionaries, anthropologists and tourists in the area than by the Bassari themselves.

It would be tempting to conclude from the above that Etyolo has reached a near-perfect accommodation to the simultaneous demands of subsistence agriculture and urban-based wage labour. If this were so, it would stand in clear contrast to many of the studies cited in the introduction to this article. Any such view would in my opinion be the result of too narrow an outlook on the situation I have described, and a fundamental misapprehension of the forces at work in that situation. As several writers have pointed out (Watson 1958: 6; Diop 1965:8; Mitchell 1959: 279; Grillo 1969: 82) any real understanding of labour migration—and by implication, of its effects—must come from looking at not only the rural villages but also at the urban target areas as aspects of the same social field. Various factors originating outside the village, whose significance is as yet only dimly perceived in Etyolo itself, may become crucially important in the near future. It is to an examination of these that I now turn.

V

Future trends

The single most important factor which will affect Etyolo in future years concerns the number of migrants who have not returned to the village. For the sake of simplicity I will refer to these as 'long-term' migrants, for it is difficult to assess their degree of permanency in town, as has been noted by others (Van Velsen 1963:35–6; Mitchell 1956; Diop 1965: 79; Caldwell 1969: 190; Watson 1958: 193n). In 1971 I recorded a total of 154 people in Etyolo (91 men and 63 women) who were or had been migrants, out of a total village population of 484. Of these, I could find only 35 (18 men, 17 women) who had ever been absent working during the rainy season; of these, 11 men—all elderly—had been absent because of forced labour. But my village census of migrants obviously only took account of those migrants actually in the village. By careful questioning and cross-checking in 1972, I discovered 73 individuals then absent from the village for a year or more (40 men, 33 women) and living outside of Bassari country. For 34 of these it was impossible to determine precisely how long they had been away, but in general it appeared that there had been a slow but steady outflow since the early 1960's. This was first noted by Monique Gessain in 1967 when in her examination of general migration patterns among the Coniagui and Bassari she remarked:

> Previously, Coniagui and Bassari migrations never lasted more than one dry season. Today more and more, as migration grows, its duration has a tendency to lengthen. If there are at present hardly a dozen Coniagui and Bassari—and among them only one woman—who have

left home for ten years or more, there are more and more who have been gone, for example, for three (1967: 95).

By 1972, of Etyolo's 73 long-term migrants, all but 3 were in large towns (i.e., those with a population of 5,000 or more). Thirty-two were in Tambacounda, 13 in Kédougou, and 16 in Dakar.[3] Although some of these will perhaps return to the village to live eventually, interviews with migrants in these towns indicated that many of them had no intention of going back.

Although long-term migration from Etyolo has grown relatively slowly and in small increments over the years, absentees now total a sizeable proportion of village men and women, many of whom are located in urban areas. This number increases yearly. The real significance of this can be seen, however, if we examine the present age-structure of this absentee group as set out in table 1 below.

TABLE 1. Long-term migration from Etyolo by age-class as of 1972.

CLASS NAME	AGE RANGE	RESIDENT STRENGTH	ABSENTEES
MALES:			
a-xark	46+	25	1
o-pidor	40–46	11	0
o-kotok	34–40	12	5
o-dyar	28–34	15	10
o-palugu	22–28	16	12
o-dugu	16–22	22	11
odo-muta	9–16	19	0
o-dingita	7–11	25	0
(class unknown)			1
		Male total:	40
FEMALES:			
a-xark	52+	30	1
od-seb'k-epeka	46–52	20	0
odo-peka	40–46	22	0
od-brk-bengat	36–40	16	2
od-seb'k-ebatya	30–36	27	2
odo-batya	24–30	25	7
odo-dyar	18–24	18	5
odo-palugu	12–18	33	5
odo-dugu	7–12	24	0
(class unknown)			11
		Female total:	33

Thirty-one of the 40 male absentees come from the three age-classes—*o-dugu*, *o-palugu* and *o-dyar*—which because of their age and the requirements of *la coutume* contribute most to village agriculture. Seventeen of the 33 women absentees also come from the female age-classes in this category, whose members range from 12–30 years of age. The age-classes of 11 females could not be determined, so this figure is probably low.

The numbers for males assume even more significance when compared with the resident village strengths for their respective age-classes as set out in table 1. For the *o-dugu* class with 22 residents, there are 11 absentees, giving an absentee

rate of 33⅓ per cent. For the *o-palugs* group with 16 residents and 12 absentees, the rate is 43 per cent.; and for the *o-dyars* with 15 residents and 10 absentees, the rate is 40 per cent. For the three classes combined the rate overall is slightly more than 38 per cent.

A rate of loss such as this—and which will probably grow—among age-classes which do a major share of village cultivation can only be construed as alarming. In addition to single individuals there are growing numbers of married couples in town, mainly young people, and in a few cases whole households have moved away. Although the precise reasons for such a high rate of long-term movement are unclear, there appear to be several general factors which are at least partly responsible. Some individuals will move to town for much the same reasons as they would move to another village—divorce, sickness, disputes, sorcery, exhaustion of fields, etc.—and so in this sense town becomes simply another alternative choice. A few have moved away because of scandal or disgrace. For a fairly large number who have managed to obtain reasonably well-paid, steady jobs in town, the added measure of security there appears to be an incentive to stay on. And for a small but significant minority who have been able to obtain secondary or post-secondary schooling (usually in mission schools outside the area), there has been the chance for professional or semi-professional work.

Many of the migrants whom I interviewed in the towns expressed a reluctance to return to the village, saying that they would have no chance there to earn money or to practise their trade. Many of them who are married have invested heavily in the urban areas, and have bought plots of land, built cement-brick houses, cleared fields on the periphery and sent their children to school. In 1972, there were no urban-based migrants' associations, however, and no organised activities for village improvement such as described by Little (1966) for other west African groups. Many of the more significant age-class rituals have not been carried over to the town. In particular, the practice of communal cultivation by age-classes as part of *la coutume* has been abandoned altogether, although households still practise other forms of communal labour. Although most parents are anxious that their sons be properly initiated in the village (as one man said, 'if you're not initiated, you're not a Bassari'), many are unwilling to allow them to continue with the age-class activities there. Their feelings seemed to be summed up by one man who said, 'My boys will do the initiation in the village and that's all. The rest of it [age-class activities] is a waste of time. School is more important than the age-classes—they must all go to school now because it is they who will have to look after me when I get old.'

Although some people are not as committed as this man to urban life, and may therefore return to the village, two points should be stressed. One is that the non-performance of age-class activities by long-term migrants or their children (many of whom maintain only minimal links with the village now) is likely to make them unwelcome there in the future. The second is that with the presence in town of more and more long-term urban dwellers—some of whom are relatively well-off—a chain-migration effect will be created, making it more attractive for others to follow. There were clear indications in 1972 that this was already happening.

One of the main dangers in continued out-migration among the younger classes is its likely effect on agriculture in the village. Should the exodus continue, there

will come a time when the number of available workers is no longer sufficient to maintain production at present levels. It is difficult to say with any degree of certainty at what point this might occur, for we would have to know a great deal more than we do now about quantitative aspects of labour inputs in the area. At some stage, however, if there is a falling-off of agricultural yield, seasonal migrants will have to divert a part of their earnings to the purchase of grain to make up the shortfall. This could happen, as is the case in other areas, when not enough labour is available to perform the heavy work of clearing new fields. At this point, a downward spiral will have begun from which there is little likelihood of escape. My analysis of agricultural data (Nolan 1974) indicates that several of the smaller families of Etyolo are already in a somewhat marginal position and rely heavily on communal labour; a decrease in this could have serious effects on their viability as production units and might in some cases compel them to move away. We would also expect changes in the shape of the migrant 'career', for as food supplies decrease, seasonal migration—for those left in the village—will of necessity become more frequent and extend over more of a person's life.

An instructive and depressing example of this process already exists from another area of Senegal. Diop's 1965 study of Toucouleur migration from the Moyenne Vallée of northern Senegal stresses the role of poor agricultural yields as both a cause of and an effect of the seasonal and long-term migration of large numbers of men. Diop considers migration for cash as an economic necessity, given the low level of development in the region (1965: 41–43). These movements, however, take men away from the village at crucial times in the agricultural cycle, with a resulting drop in production (1965: 37). Twenty-five per cent of the active men have emigrated 'permanently' from the Vallée for a number of reasons (1965: 205), and an 'undetermined number' of rural dwellers are forced to migrate each year for lack of food when the harvest has been consumed (1965: 146) Thus a vicious circle of poverty is created: economic underdevelopment means that men must migrate to earn whatever cash they need; the resulting loss of manpower depresses local agriculture; subsequently, rural poverty is accelerated, provoking more migration, including permanent movement (1965: 205).

There are differences between Bassari agriculture and the Moyenne Vallée which need not be gone into here, but which prevent a straightforward comparison of the two situations. It is also true that Bassari agriculture is probably fairly resistant to the pressure of long-term migration, relying as it does on large communal work parties. But it is clear that continued out-migration will eventually take its toll. It is equally clear that this rate of out-migration to the urban areas, influenced as it is for many by the presence of both relatives already in town and attractive opportunities there, lies substantially outside the control of the village.

VI

There are other factors as well, also lying outside village control, which compound the danger. It must be remembered that although the Bassari system of agriculture is relatively efficient and productive, it is carried on in an area where soils are poor and rainfall is continually uncertain. Current difficulties in the Sahelien belt (cf. Grove 1974; Bryson 1973: Lamb 1974) which borders on the

Bassari area point to the obvious dangers involved, and although there is uncertainty in professional circles as to the true significance of these recent climatic changes, there is little cause for optimism as regards the immediate future (cf. especially Grove 1974: 144–5). While much more fortunate climatically than many areas to the immediate north in Mali and in Senegal, a general worsening of conditions in that area would have effects in Bassari country as well. Erosion has also been a major problem in Senegal-Oriental for some time (cf. Govt. of Senegal 1963: II–8 (16)), and may be expected to continue.

Additional circumstances, also the result of outside influences, contribute to this already unpromising situation. Due to the immediate presence of the Niokolo-Koba National Park to the north and west of the area, hunting has been banned for some time in the entire area occupied by the Bassari. Although clandestine hunting continues on a much smaller scale, penalties are severe. The ban on hunting, as the Government of Senegal's 1963 report made clear, will make it impossible to control those animals (monkeys, birds, etc.) which do a great deal of damage to staple crops, and represents a decision taken in favour of tourism and against the farmer (1963: II–8 (16)). Adverse nutritional effects might also result from the reduced intake of meat.

The Bassari group has for long been split in half by the colonial border between Senegal and Guinée. As long as the entire area remained under French control, free access to adjoining areas posed no problem. Since Guinée's independence in 1958, however, the imposition of border and customs controls and the often uncertain political relations between the two countries have made trans-border movements more difficult. Trade which existed in former times between groups across this boundary is now often defined as contraband by the governments concerned, providing another example—as in the case of hunting—where traditional activities have been rendered difficult or impossible because of administrative fiat.

Following the replacement in 1960 of the Bassari canton chief by a non-Bassari *chef d'arrondissement*, government control over various aspects of local life has tended to increase. Villagers are officially prohibited from engaging in certain occupations or selling certain goods without the purchase of special licences (e.g., *the harvest and sale of palm wine; the cutting of bamboo*); *identity cards are now* required for migrants; the possession of locally-made firearms is forbidden; and disputes are now often settled by the arrondissement chief rather than by local leaders. Concurrent with this situation—and perhaps in part because of it—there exists a considerable degree of mistrust and suspicion by the Bassari of non-Bassari 'Senegalese' with whom local people share little in the way of a common religion, language or background.

There are three government schools in the area; the one at Etyolo was opened in 1947. There are also several smaller mission schools, run by the Catholics, in Bassari country. The language of instruction in each case is French. Pupils, in addition to receiving a year or two of primary education, also come under the indirect influence of a major religion at this time—Islam for the government schools, Catholicism for the mission schools. Recommendations for a national language policy for Senegal put forward in 1972 seemed to indicate that in the future Bassari children may be taught in Peul, the language of their former enemies.

The above points are merely incidental to what appears to be the major threat

to village society—namely, the possibility of dependency on wage-labour to provide money for food in the home areas. In view of certain indications that agriculture in Senegal is declining, and the fact of increasingly harsh droughts in nearby areas, it would seem essential to examine carefully the conditions under which socio-economic relationships in rural areas which are presently self-sufficient will persist or be modified. And one should pay particular attention, as I have tried to indicate, to the underlying causes of rural-urban movement and its effects on village agriculture. With regard to the factors of increased government control and restriction, the effects of education and the splitting of the Bassari group in two by national borders, one can see these as contributing rather than causal in the potential process of village decline and fragmentation that I have outlined earlier.

VII

Throughout this article my central hypothesis has been that a continuing rate of long-term migration from Etyolo similar to that which presently exists will eventually culminate in a downward spiral or food shortages, increasing movement to towns, and general rural impoverishment not unlike the situation described by Diop (1965). Should this process, once started, continue unchecked, we might see villages like Etyolo nearly empty of young men and women—not seasonally as we do now but throughout the year—as more people leave permanently for town. The effect that growing urban unemployment might have on this movement cannot be explored here except to say that a tightening-up of the job situation might have the effect of encouraging seasonal migrants to town to stay with their jobs longer instead of driving them back to the village (cf. Schapera 1947: 58; Grillo 1969: 80). Those who stay behind in the village—or who return there—will be forced to spend their meagre earnings on food At this stage, along with much of its self-sufficiency, the village society will probably have also lost a great deal of its cohesiveness and form.

Is this necessarily what 'integration into the wider world' must mean for such villages as Etyolo? Surely such a situation can only be described as a retrograde step in both cultural and economic terms. However, the solution—if there is a solution—is most certainly not to wish for either the preservation of the *status quo* or for a 'return to the old ways'. This is impossible for obvious reasons. It would also be immoral to wish to create—even if one were able to do so—a sort of living anthropological museum, if for no other reason than that this would deny the Bassari their very real aspirations, and prevent them from meeting their even more real needs of the present time. Furthermore, there is a fundamental error in assuming that stasis is the same as 'no change'. In 1961 Audiger described the static nature of the Wolof villages of the Bas-Férlo in northern Senegal; her comments are particularly apposite, I feel, for Etyolo at the present time:

> But this [village] changelessness which is perhaps only an appearance reveals itself in fact to be a decline. The economic development of other areas of Senegal has had as one of its first consequences the effect of making the living conditions of the Bas-Férlo appear even more mediocre. As the towns grow larger, and as the centres of peanut commerce are further developed, so grow the differences in the standard of living between the Bas-Férlo and these other areas. There is a risk that the problem will become a serious one,

since the distance between the two life-styles will get larger and larger. From that point on, if no development is undertaken in the Bas-Férlo, there will be rapid depopulation (1961: 179)

It would appear that the only possible answer to the general problem posed by Audiger, Diop, and the present article is some form of local-level development which can be made cash-generating. Unfortunately, Bassari country offers few possibilities for projects of this kind, being poor in mineral and water resources, far from major industrial and commercial centres, and hampered by poor roads and seasonal isolation. The 1963 report on development of the Government of Senegal stated with respect to Senegal-Oriental:

> The development of this region presents an aspect which is essentially pioneer.... In fact, the area is remote and underpopulated, and government inputs for development will be inevitably small if they cannot be financed other than from the output of the region's resources as they are known at the present time. (1963: II–8 (17))

Two possibilities were being discussed by outsiders in 1972: the development of tourism in the area; and an attempt to introduce new varieties of rice as a cash crop. Bassari initiation ceremonies and dances are colourful and exciting, and dozens of tourists often manage to travel long distances to attend each year. In 1972 a package tour of the area was organised by a firm in Dakar at initiation time. Apart, however, from the occasional sale of ceremonial masks and ornaments to such tourists—and this mainly by those families living in close proximity to the road—the Bassari receive no benefit at all from organised tours, and there is little likelihood that this situation will change in the near future.

A pilot project in rice-growing was begun in Etyolo in 1972 by the Compagnie Française du Développement des Textiles (CFDT) at Tambacounda. The Bassari already grow rice for domestic consumption and are experienced in the necessary techniques. The improved varieties of dry-land rice which are being tested are expected to produce higher yields, and will be bought for cash by the CFDT if the project goes ahead. The road from Kédougou to Salemata is being slowly improved by the government, which would contribute to the viability of the project if the tests prove successful. Two possible difficulties might arise from cash-cropping, however. The first is that the bottom land suitable for growing rice in the village is in limited supply and unevenly distributed among households, so that not every family will be able to benefit from cash-cropping. One result of the creation of a situation where some households derive cash from rice farming and others do not might be to encourage even more out-migration on the part of the less fortunate.

The second disadvantage—equally problematic—stems from the first. It is possible that in a situation where one section of the community derives cash from cultivation and another section derives merely subsistence, the communal labour system may break down in whole or in part. One of the underpinnings of reciprocal labour arrangements, in Etyolo if not elsewhere, appears to be the fact that no one can reap regular cash rewards from the labour of others.

In short, the dilemma outlined here is a familiar one, by no means peculiar to the Bassari area alone. A situation is developing, if my analysis is correct, which will result in the eventual depopulation and impoverishment of many if not most of the families of the village. The measures presently available for correcting this situa-

tion would appear to imply other changes in the society of a more unpredictable kind which in any case may not have the beneficial results desired. As things stand now the village is at a stage in relation to the outside wage-economy which can only be called 'terminal', in Howlett's sense of the word (1973), in that no further development appears possible under present conditions. As I have tried to argue, there are strong indications that the next step will be a retrograde one. Furthermore, the village itself does not appear to be in a position to influence this process to any great degree.

As I hope I have shown, to take a *laissez-faire* attitude towards this situation will be to permit it to worsen. The resolution of the dilemma posed here can only be found, I feel, in some sort of positive action at the level of the villages concerned to provide alternative ways of earning cash locally. Common sense dictates that any plans drawn up in this regard be worked out on the basis of full and extensive consultation with village people.

No one should believe that this will automatically have the effect of halting the rural exodus, but it may alter both the composition and the rate of flow for the migrant stream, provide an acceptable alternative for many people to living in town, and contribute to the continuing agricultural self-sufficiency of the rural area. This will be difficult, of course, and the working-out of satisfactory schemes will require time and a great deal of thought It is therefore essential that a start be made as soon as possible, and that studies of—and alterations to—proposals for tourism and cash-cropping as well as other possible schemes be carried out without delay. In a country such as Senegal, with limited amounts of money and manpower, it is perhaps presumptuous to ask that some priority be given to an area of so few people at such a great distance from major centres, especially when there are other areas in similar or worse situations. The development of remedies and alternatives, however, need not involve great outlays of money or personnel, if carefully thought out and conscientiously executed.

The pattern of change in Etyolo—as for small-scale communities all over the world—is essentially one of slow but inexorable incorporation into larger political and economic structures at a national level, and the forging of new relationships within this context. These relationships are not necessarily happy ones. Local-level efforts at development are essential to enable the village to continue to influence and accommodate itself to both this incorporation and an increasing dependency on the money economy, and to ensure that its new relationship with the outside world will prove on balance to be a beneficial one.

NOTES

The present article stems from fieldwork carried out in Etyolo on migration and village change from 1970–1972. This work was funded by a grant from the Foreign Area Fellowship Program in New York.

[1] A corpus of published literature on aspects of Bassari society presently exists—nearly all in French—mainly as the result of intensive field projects in Senegal-Oriental carried out by the Centre for Anthropological Research (CRA) of the Musée de l'Homme in Paris, headed by Professor Robert Gessain. A comprehensive bibliography of sources relating to Senegal-Oriental, and the Bassari in particular, can be found in R. Gessain (1972). Two articles of special relevance in the present context are Dupré (1965) on the agricultural system of Etyolo village, and M. Gessain (1971) on age-classes in Etyolo. All translations in the present article from sources originally in French are my own, and I alone must accept responsibility for any errors of translation and/or interpretation.

[2] I was unable to make any systematic study of the actual amount of millet used in making beer for the various ceremonies and communal labour events. The number of occasions where beer is used is very high; I recorded scores of separate ceremonies at which beer must be prepared and drunk. At the height of the growing season, there are often four or five beer-drinks per day in various sections of the village in connexion with communal labour. Charest has estimated that 25 per cent. of the annual millet harvest of the neighbouring Malinké is used for beer, feasts and communal labour (1972: 397); while I have no comparable figures for Etyolo, it is my impression that the figure for the village is at least that, and probably greater. A reduction or postponement of a number of ceremonies requiring beer can therefore free an appreciable amount of grain for eating, in bad years. This happens with both communal labour payments and with initiation ceremonies.

[3] In fact, these figures are low, as I learned during a six-week stay in Tambacounda where I interviewed long-term migrants. Although only 32 people were supposed to be in Tambacounda (according to villagers in Etyolo), I actually found 42, whom I classified as long-term migrants and who could be said to come from Etyolo (Nolan 1974 245, 268–74). In part, this discrepancy is due to the types of problems described by Caldwell (1969: 36–38) in attempting to define the origin of town-based migrants, especially in situations of high inter-village mobility.

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COMPADRAZGO IN CATHOLIC SRI LANKA

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Most discussions of *compadrazgo* are concerned with data from Latin America or the Mediterranean. This article is concerned with the institution as it exists among the Catholic population of Sri Lanka. It is suggested that a satisfactory analysis of this example of *compadrazgo* requires an examination of the wider kinship system in which the institution is set. The crucial contrast is seen as that between the rituals of marriage and baptism which generate the 'earthly' and the 'spiritual' families as well as relations of affinity and spiritual affinity. *Compadrazgo* is seen as a part of a triadic notion of man involving natural, social and spiritual components, a triad which is also apparent in the kinship system as a whole.

I

For the last twenty years there has been a steady flow of writings on the subject of *compadrazgo*, mostly concerned with data from the Mediterranean and Latin America. The majority of analyses have been functionalist in style, concerned with the ways in which actors can use *compadrazgo* ties in an instrumental fashion, or how the institution furthers 'vertical' or 'horizontal' solidarity.[1] In the last few years however, new approaches to the subject have appeared, the most impressive being that of Stephen Gudeman (1972; 1975). He points out that one cannot reduce *compadrazgo* to some epiphenomenon of more 'real' political or economic factors, and that one must begin the analysis by regarding *compadrazgo* as a symbolic system, intimately connected with the rituals of baptism and the concept of the person in Catholic thought. A second approach which moves away from functionalist paradigms is that of Julian Pitt-Rivers (1958; 1968; 1973). For him, the relationship between a man and his godparent is reminiscent of that between a man and his mother's brother in a patrilineal society. Like Gudeman, he stresses the importance of the concept of the person in *compadrazgo*, but his analysis is closer to that of Fortesian anthropology than to the structuralism utilised by Gudeman.

My aim in this article is limited. I am not intending to assess the vast amount of data available on the subject, nor at the moment am I attempting to produce a general theory of *compadrazgo*. Rather I limit myself to examining certain weaknesses in the analysis produced by Gudeman and Pitt-Rivers and show how previous analyses of *compadrazgo* have to be augmented and modified in the context of data from a village in Sri Lanka.

In his 1972 article, Gudeman sees *compadrazgo* as a result of the conceptual distinction in Roman Catholic thought between 'physical birth' and 'spiritual birth'; between 'man as a spiritual and natural, or cultural and biological being' (1972:47). These two aspects of the person are the basis of two sets of relationships, one set up

by birth, the other by baptism. The natural parents of the child, by giving certain rights to the spiritual parents, create a spiritual tie between the two sets of parents which 'results in the *compadrazgo*'. What Gudeman calls the '*compadrazgo* set' then mediates between the 'birth set' and the 'baptismal set'.[2] Gudeman goes on to show how the analogies, baptism : birth :: spiritual : natural are worked out in Roman Catholic theology, particularly in the thought of St Thomas Aquinas; how spiritual relationships stand opposed to those of sex and marriage, and how there is a parallel between the baptismal and birth sets and the concept of the Holy Family. Having examined the theology of baptism and *compadrazgo*, he goes on to consider his own Panamanian data, and a large amount of comparative data, showing how variations in *compadrazgo* are variations on this basic underlying theme.

Gudeman's second article is concerned with developing certain aspects of his earlier work, in particular the relationship between the 'sacred' and the social context in which it is operationalised, and the rules concerning the choice of godparents. He makes the interesting point that the distinction between spiritual and natural does not necessarily imply a distinction between natural parents and spiritual parents, showing how historically the latter distinction was a relatively late development in which social usage preceded the Church rule which prevents parents from acting as godparents to their own children. Gudeman argues that this distinction was realised for extra-religious purposes which he implies were those of creating social relationships between the child and others and the parent and others.

Secondly, Gudeman discusses the relationship between parent and godparent. This is normally asymmetric and irreversible, a parent being unable to act as his child's godparent's child's sponsor. But this is not a rule of the Church. Gudeman argues that whilst one can claim that asymmetric choice produces 'greater social solidarity', such an argument is difficult to sustain. Instead he falls back on an argument presented in his previous paper which I had best reproduce:

> Just as two forms of paternity must be kept separate, . . so natural and spiritual co-parenthood must be distinguished Further, since spiritual relationships are higher than natural ones, the godparent in terms of the child occupies a higher status than the parent. Therefore, if choice were returned from godfather to father, each would be both natural and spiritual co-parent to the other, and each would occupy both a higher and lower status in relation to the other. By prohibiting choice reversal this structurally impossible situation is avoided (1972 63).

In other words, just as the rule banning parents as baptismal sponsors of their own children originated in society and became a ruling of the Church, so the Church ideal that spiritual is superior to natural has resulted in the common folk ruling that bans choice reversal.

There are some problems with Gudeman's analysis. First of all, there is a certain amount of confusion in his conceptual apparatus. The title of his first article refers to the 'natural' and the 'spiritual' person. Yet in the text, 'spiritual' becomes synonymous with 'cultural' and it seems that 'cultural man' is being contrasted with 'natural man'. Secondly, he limits most of his discussion to the contrast between the relationships set up through baptism and those set up through birth. For him, the 'natural' and the 'spiritual' families are central. Yet at times, he introduces 'kinship' as distinct from the 'family' and the connexion between kinship and *compadrazgo* is not made clear.

This last problem is closely related to another which concerns the relationship between the parent and the godparent. Gudeman treats this relationship in a rather cavalier fashion claiming that structurally this relationship is concerned simply with 'mediating' between the 'natural' and the 'spiritual' (or is it 'cultural'?) families. Yet he admits that the relationship between the natural and the spiritual parent is a spiritual relationship, and so of the same order as that of godfather and godchild. Therefore it is difficult to see how it can mediate between the 'spiritual' and the 'natural'. Furthermore, Gudeman fails to deal in a satisfactory fashion with the variation in the relationship between parent and godparent. Gudeman himself mentions a number of cases where choice is reversible, and in this article I shall discuss another example. As he writes in his second article, the 'inherent or incipient asymmetry' between parent and godparent may or may not be socially recognised, yet he fails to specify the conditions under which such recognition will or will not be given. In this article, I hope to outline an argument which denies the logic Gudeman claims to have discovered in the quotation mentioned above, and which accounts for the relationship between parent and godparent within a wider framework.

In summary then, my objections to Gudeman's analysis are that it is not clear whether he is talking about 'spiritual' or 'cultural' man; that he fails to deal in a satisfactory fashion with the relationship between *compadrazgo* and kinship, and that the relationship between the natural parent and the spiritual parent is not explained.[3]

As I have already indicated, Pitt-Rivers's approach to *compadrazgo* is rather different from that of Gudeman. It is also much less easy to summarise. Although admitting the distinction between the natural and the spiritual versions of the family, the stress of his argument lies elsewhere. In his 1958 article, he points out that the role of the godparent in Andalusia 'reminds one' of the mother's brother in patrilineal societies. This idea is developed in his 1973 article where he couches his argument in terms of both 'collectivities' and 'aspects of the person'. He writes that the basic collectivity in Andalusian society is the ambilateral nuclear family within which the individual is a 'social cypher'. In contrast to this, the godfather stands outside the basic collectivity and is a sort of 'spiritual affine' concerned with the 'personal destiny' of the child rather than with the child as a member of a social group (1973: 102–3). The essential conceptual opposition in Pitt-Rivers's analysis is that between 'pseudo kinship' and 'real kinship'.

Here, Pitt-Rivers stresses a point which is going to be important in my own analysis: the similarity between the sponsor/child relationship and complementary filiation. Yet there are certain drawbacks in his formulation of the problem. For instance, he sees the godparent as being concerned with the 'personal destiny' of the child: with the child as an individual rather than as a member of a social group. If this is so, then it is difficult to explain the Serbian case of sponsorship in which relationships are not between individuals but between patrilineal groups called *zadruga*. (Hammel 1968). Furthermore, Pitt-Rivers appears to be reducing the institution of *compadrazgo* to the product of actual groupings on the ground. Here, I would follow Dumont and argue that it is not the form of the groupings which is important, but rather the principles of kinship which lie behind the form the groupings or the collectivities take.[4]

Despite these criticisms, it is upon the insights produced by Gudeman and Pitt-Rivers that I wish to construct my own analysis. From Gudeman I take the point that the *compadrazgo* is an elaboration of Catholic theology, and involves a distinction between the natural or the earthly family on the one hand, and the spiritual family on the other. From Pitt-Rivers I take the point that in some senses, the godfather is similar to the mother's brother; that the godfather is a spiritual affine.

II

Roman Catholicism was introduced into Sri Lanka by the Portuguese in the sixteenth century at about the same time as Catholicism was arriving in the New World. In Sri Lanka it was most enthusiastically taken up by the Karaava caste who occupy a narrow strip of land along the west coast of the island. After the Portuguese were driven out of Sri Lanka, many converts reverted to Buddhism or Hinduism, whilst others became Protestants. But a large number of the Karaava, in particular those living north of Colombo, remained loyal to the Catholic Church despite the persecution by the Dutch and the indifference of the British

The village from which my data come is called 'Wellagoda', and lies on the coast about fifty miles north of Colombo.[5] It consists of approximately 750 people, the majority of whom are of the Karaava caste. Most of the villagers are fishermen, and all are Roman Catholic. Whilst I was in the village there was a resident priest, but this has not always been the case. This priest was sent to the village as a form of retirement and had little to do with the people of the village except to carry out the minimum number of services required.

Like most of the Catholic Karaava, the people of the village are bilingual in Sinhalese and Tamil[6] which accentuates their marginal position in Sri Lanka. Not only are they Catholics in a Buddhist country but they also speak Tamil in a country where the population is predominantly Sinhalese speaking. Not only do they subscribe to an alien religion but they also possess a dubious ethnic identity This marginality is also expressed in their social organisation which displays marked variations on the standard principles in Sri Lanka.

How far these variations are the direct result of Catholicism is difficult to ascertain. Some which I mention below do seem to be directly related, notably the shifts in the kinship terminology and the changes in the marriage regulations. Other variations such as the patrilineal bias in Wellagoda do not seem to be the result of Catholicism, whilst further features of social organisation and custom, such as the 'impurity' of women after childbirth, seem fairly widespread throughout Sri Lanka. In the present context I hope to show that no matter what the historical origins of the elements involved, there is a basic structure apparent in the Wellagoda kinship system.

In Wellagoda, caste ascription is based on patrifiliation.[7] Children take the caste of the father, the mother's status being irrelevant to the caste status of her child. Furthermore, caste status is fixed at birth, and there is no ideology of pollution through contact. Cross-caste unions are common, and a woman does not lose her caste status through marrying a man of lower caste than herself.

This patrilineal slant is also apparent in the formations and concepts of social

groups in the village. These groups are called *kattiya*, affiliation to which is through the father except in the case of in-marrying men who belong to the *kattiya* of their wives. *Kattiya* form a loosely segmentary organisation which orders differences in language, status claims, ritual, economic and political activity in the village.

Furthermore, the kinship system of the village is decidedly odd in the context of Sri Lanka. Unlike the Sinhalese described by Leach, Obeyesekere, Tambiah and Yalman, or the Tamils described by Banks and David[8], the people of this village do not employ a 'Dravidian' kinship terminology. I hesitate to give this variant a name but there are three basic differences between it and the normal terminologies of the island. First, affines are distinguished from cross-cousins, the latter being classed with siblings and parallel cousins. Second, the mother's brother is distinguished from the wife's father, and finally, wife givers are distinguished from wife receivers. I will return to some of these points later in this article.

Spiritual kinship (*gnana naekaema*) is created through two rituals: baptism (*gnana snaanaya*, lit. 'knowing bathing') and confirmation (*adhivrandi alepeya*, lit. 'increase anointment'). Baptism occurs within four or five days of birth, the general feeling being the earlier the better. The child is taken to the church by the two godparents, the godmother carrying the child. Although the child's father may accompany the group to the church, he has no role to play in the ritual, and he does not enter the church. The child's mother does not even accompany the group to the church for she cannot attend church for a few months after giving birth.[9]

The ritual itself follows much the same pattern as baptism in Roman Catholic churches in the West. According to the villagers, the important features of the ceremony are that water should flow over the child and that the child should be anointed with the chrism. During the ritual the child receives its christian name, usually that of a saint although in recent years purely Sinhalese names have begun to gain favour. As a result of the service, the child's soul is saved. Without baptism, the villagers claim that the child is either doomed to hell, or will simply 'die like an animal'—i.e. it has no soul. As a result of the ritual the godparents promise to care for the spiritual welfare of the child and to be answerable to God for its sins.

The ceremony of baptism takes a very short time. After it, the godparents and the child return to the child's parents' house where there is a small party for the godparents and the child's close relations. Ostensibly this party is in honour of the child, but the child is usually hidden away leaving the adults to enjoy the drinking and the eating.

Confirmation takes place during the child's adolescence, and is interpreted by the villagers as marking the child's spiritual maturity. The child is said to receive a new gift of the Spirit, and can now recognise the difference between good and evil; between right and wrong. From this point on, the person confirmed can be a godparent. At the ritual itself, there is only one godparent who must be of the same sex as the child. This godparent may be one of the baptismal sponsors or may be newly selected.

As is normal in other Catholic areas, these two rituals set up relationships not only between the child and its godparents, but also between the parents and the godparents of the child. The terms which are used between the members of what I shall call, following Gudeman, the '*compadrazgo* set', are shown in figure 1. It should be

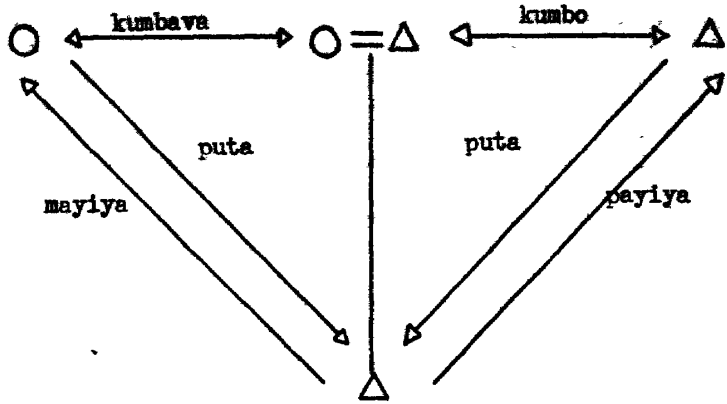


FIGURE 1. The terminology of sponsorship.

noted that as well as the child calling its own godparents *payiya* and *mayiya*, the child's siblings use the same terms to address or refer to these persons. In contrast to Latin America or the Mediterranean, there is no special term for godchild, the godparent simply using the normal terms for 'son' (*puta*) and 'daughter' (*duva*).

Before looking at the relationships involved in the *compadrazgo* set, it might be useful to describe the principles which are supposed to govern the choice of sponsors. The child's parents, who make the actual choice, are supposed to ignore caste, status or wealth in their decision. What is important is that the godparents should be good 'honourable' people, capable of living up to the responsibilities of the role, and of setting the child a good example. There is no preference for close kin, although cousins may sponsor a child. They should either be young and unmarried, or the choice should be reciprocal. Thus A should choose an unmarried man, B, as his child's godfather. Once B is married and has children, then B should invite A to be the godparent of one of his children. When one is asked to be a godparent one cannot refuse, for to do so would be tantamount to denying the child the possibility of salvation. In theory then, the choice of the godparent is divorced from the sphere of secular or materialistic interests.

Between the roles involved in the *compadrazgo* set, there are certain formalised ideas about correct behaviour, rights and duties. Between the child's co-parents (*kumbo*) reciprocal terms are used. Unlike Latin America, the relationship between *kumbo* approximates to a joking relationship. It is one of complete equality with no strictly defined duties between them. Rather it is said that they, 'just eat and drink together'. They are expected to be close friends and drinking partners, but there is no expectation of economic help between them, and *kumbo* do not use each other for political purposes. Rather the opposite, for even to mention a *kumbo* is almost a joke in itself.

The relationship between a godfather and a godchild (*payiya* and *puta*) is much more concerned with the spiritual life. A sponsor is responsible for the spiritual welfare of the child and should always set the child a good example, which rather

clashes with his role as the child's father's drinking partner. If the child's parents die, then one of the sponsors should adopt the child if none of the close relations is willing or able to do so. Baptismal sponsors are supposed to come to the child's first communion, confirmation and wedding, at which times they should give the child small presents. If the child dies before it has been confirmed then the *payiya* should supply the coffin and the *mayiya* the funeral clothes.

Looking now at the relationship from the child's point of view, the child should always treat his sponsors 'like parents'. The sponsor should be treated with respect at all times, and a godchild should not smoke, drink or sit before the godparent. Neither should the child talk about sexual matters or use coarse language within the godparent's hearing. In a sense the godparent has even greater power than the parent, for whilst the latter can only bless the child, the sponsor can also curse the child. At the annual festivals of the Church, particularly at Christmas and the New Year, the godchild should make small presents to the sponsor. At the godparent's funeral, the godchild should light candles at the head and the foot of the grave after the coffin has been interred.

Finally it should be noted that there is no special relationship between the two godparents of the child. They have no obligations to each other and their only link is that they share a common *kumbo* and have a godchild in common.

Between members of the *compadrazgo* set, certain restrictions are placed upon marital and sexual relationships. A parent and a child can, of course, have no sexual relationships. Neither can a godparent and a godchild for they are said to be like 'parent and child'; they are said to be related through *gnana naekaema*, spiritual kinship.

A man can marry his godchild's sister, although such a marriage is rare, but whether one can marry one's godparent's children is rather unclear. Some say that such marriages are perfectly possible although they require Church permission, the relationship being said to be similar to that between third grade collaterals (second cousins). Other informants say that such marriages are completely impossible. Figure 2 might make these restrictions slightly clearer. Thus A can marry B2 but not B1; A1 and A2 can marry B2. Whether B1 can marry A1 and A2 is unclear. Finally, there are no restrictions on co-sponsors of the same child marrying, although such matches are sometimes said to be 'not very nice'.

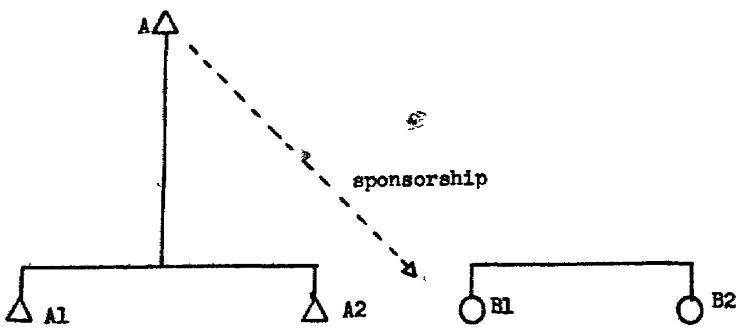


FIGURE 2. Sponsorship and marriage regulations.

What I would like to stress at this point is that these relationships set up through baptism and confirmation are almost entirely devoid of political and economic content. Sponsors are not thought of as patrons either of oneself or of one's child. Neither are economic ties important. In practice and in theory the relationships are without pragmatic instrumental content, and it would be difficult to reduce the principles defining the relationships of the *compadrazgo* set to some sort of epiphenomena of more 'real' or more 'important' economic or political factors. This lack of pragmatic content is shown in the brittle nature of the relationships. Despite the theoretical value the villagers place upon the institution, many sponsorship relationships are allowed to fall into abeyance. Thus only 8 per cent of a sample of 230 sponsorship choices were reciprocated, and at sixteen weddings, the sponsors of the bride and groom made appearances at only six.

III

As I have already indicated, Gudeman begins his analysis with the distinction between natural and spiritual birth, whilst Pitt-Rivers starts by stressing the importance of the godparent as an 'outsider'. The point at which I want to begin my own analysis is with the actual ritual of baptism which creates the relationships of the *compadrazgo* set. Furthermore, I shall ignore official Catholic theology using instead the villagers' own ideas, a sort of 'ethno-theology'.

The villagers make a direct contrast between marriage and baptism, claiming that these two rituals are the most important in any person's life. Both always take place in church, and almost everyone in the village goes through both rituals. Marriage is said to be 'for man'; baptism 'for God'. These two rituals, along with confirmation, are the only ones which create interpersonal relations.

At birth, a child has both body (*kaya*) and soul (*aatmaya*). But the soul of the child remains in a sense inactive until baptism has taken place.[10] As I have already indicated, there is some uncertainty as to what happens to the soul of an unbaptised person. Whilst some people claim that it goes to hell, others imply that as the soul has not been activated, then a person simply dies once and for all. Villagers link the importance of baptism to the necessity of saving the child from original sin or, as they put it, 'the sin of Adam'. Adam, by eating the fruit of the tree of the knowledge of good and evil discovered sexual difference, and baptism saves the soul from the sin of the sexual activity which produced the child. Even so, villagers say that man must pay for Adam's sin, and that therefore it is men's doom to work and women's doom to bear the pain of childbirth.

Whilst baptism saves a person from the sexual activity which produced him, there is still the problem of dealing with a person's own sexuality. Villagers agree that the supreme virtue is the complete rejection of sex: a life of chastity. But they say that chastity is only for the religious experts, the priests, nuns and monks. It is wrong for ordinary laymen to remain unmarried just as it is wrong to have sex without or outwith marriage. Thus there are three classes of people: the religious experts whose religious state allows them to reject sex entirely; those in the secular world who control sex through marriage, and those who have sex without marriage.

The dividing line between those who control sex through marriage and those

who enjoy uncontrolled sex is in effect the dividing line between society (*samaja*) and nature (*jaataya*). What marriage does is to mark off man from the animals. Couples who engage in premarital or extra-marital sex are said to be 'beasts' (*muruge*); prostitutes and promiscuous women are known as 'bitches' (*veesi*), the most promiscuous animals the villagers know. Furthermore, the people of Wellagoda state quite explicitly, like Lévi-Strauss, that marriage is the basis of society and that without marriage there is no society.

Thus baptism and marriage both demarcate man from his basic animality. Baptism activates the soul, the spiritual aspect of man which is lacking in animals. Marriage controls the sexual aspect of man, setting up the society and social order which animals lack. These two aspects of man, the spiritual and the social, are contrasted with the sexual, animal aspect of man, the product of an act of copulation, through the rituals of baptism, confirmation and marriage.

If we now turn to the actual product of an act of copulation, we find that in this Catholic village there is no stress on filiation in a ritual sense, unlike other parts of Sri Lanka where there is a ceremonial recognition of the child by the father.[11] Furthermore, filiation is independent of marriage in the sense that no distinction is made between the *pater* and the *genitor* unless there is a formal act of adoption. Thus in theory, an illegitimate child should take the caste of his *genitor*, not of his mother's husband. Filiation is seen as a purely natural link, part of nature rather than of culture. In this context, the relationship of father to child is particularly stressed. From the father a child takes its caste status and its inherited name. Caste status is formulated in terms of blood (*le*): high caste people have 'good blood'; low caste people have 'bad blood'. Castes themselves are seen as natural phenomena rather than as social categories and caste status is something one is born with. One's caste status is part of one's bodily substance.[12]

So I am arguing that there are three aspects of man implicit in the Wellagoda concepts of kinship. There are first the bonds of natural kinship in which individuals share a common bodily substance. Secondly, there is the spiritual aspect of man, associated with baptism and confirmation and the relationships which these rituals generate. Finally there is the social aspect of man concerned with marriage without which there could be no society. The rituals of baptism and confirmation are concerned with the spiritual growth of the child. Marriage is concerned with the child's social maturation.

We can now turn to marriage and the contrast between marriage and baptism, in particular to the contrast between the relationships generated through these two rituals. In sociological terms, marriage does two things. it sets up relationships of affinity between men, and it creates (eventually) the nuclear family. Similarily, I shall argue that baptism sets up both relationships of spiritual affinity between men, and also the spiritual family.[13]

Unlike other parts of Sri Lanka, marriage in Wellagoda is conceptualised as being a relationship between two unequal groups of people: the wife givers and the wife receivers. As in the Indian situation, wife receivers rank higher than wife givers. This asymmetry is displayed in a number of ways. In the terminology, the normal Sinhalese Dravidian system is modified. Normally, the term *massina* is used in a reciprocal fashion between wife's brother and sister's husband, but in Wellagoda,

modifiers are introduced so that the sister's husband becomes the *loku massina* (big *massina*) and the wife's brother becomes the *podi massina* (small *massina*).

This terminological asymmetry has its parallels in ritual and in rules of behaviour. In Kandyan Sri Lanka, the relationship between *massinas* is one of easy-going friendship and familiarity and stands in contrast to the markedly asymmetrical relationships between siblings. In Wellagoda, the relationship between wife's brother and sister's husband is markedly asymmetric and very similar to that between brothers, the *loku massina* being equivalent to the *ayiya*, or elder brother. When wife's brother and sister's husband meet, the former treats the latter with deference and respect.

In ritual too, this asymmetry is marked, particularly at marriage. Here, the groom's feet are washed by the bride's brother. The groom has to be given a dowry or else he will not marry the girl: in village thought, the groom has to be 'bought'. Throughout the marriage ceremonies, the groom is treated with almost exaggerated respect and veneration by the bride's family.

As well as this marked asymmetry in affinal relations, an affinal relationship cannot be reversed or repeated. Between households, and in particular between *podi* and *loku massinas*, there can only be one affinal link. Marriages of two brothers to two sisters are frowned upon because they repeat an affinal tie. Sister exchange is more serious since it repeats an affinal tie and reverses the affinal relationship. Both of these types of marriage are said to break the bonds of *vivaha naekaema* (marriage relationship). Such marriages are said to lead to disasters: houses are burnt down, fishing gear lost and so on.[14]

The final point about affinity is that the affinal relationship between wife's brother and sister's husband is inherited between generations and is transformed into the relationship of affinity between mother's brother and sister's son. Admittedly this is a debatable point but the evidence would seem to support it.

Consider the case of Anton. He is the child of his father's second wife, but Anton still refers to his father's first wife's brother as *maama* (mother's brother). Leon is Anton's *maama* because he is Anton's father's *podi massina*: the relationship is purely one of affinity.

More explicitly, in the context of marriage, there is a close link between brothers and sisters. A brother is responsible for his sister's marriage. He should pay for it and only once she is married can he think of marrying. This association is continued in the next generation: the *maama* who is important in a child's life is the one who paid for the child's mother's wedding, and who went on his knees to wash the child's father's feet.

Finally, on the level of terminology, one must note that *maama* does not simply refer to the genealogically identified mother's brother but rather to a category of kin which includes the mother's brother as well as certain unambiguous affines such as the father's sister's husband. The mother's brother is simply the *maama* par excellence; the representative affine of the senior generation who is related to the sister's son through marriage. If there is no true mother's brother, or if one is not available, the father's sister's husband can perform his duties.

The most important role of the *maama* is at marriage. Here his role is secondary only to those of the bride and groom, for rather than the parents, he is in charge of the rituals. The *maama* is said to 'have the power' at weddings. It is the two *maamas*

who give away the bride and groom at marriage. Before the wedding, the groom should be ceremonially shaved by his *maama*. On the wedding night, the groom's *maama* pushes the groom into the bride's room and on the following morning inspects the sheets for evidence of the bride's virginity.

The other two rituals at which the *maama* is prominent are not so important. The first is concerned with a boy's first fishing trip. Villagers do not allow him back to shore until his *maama* has brought a few bottles of arrack to the beach for all to drink. The other ritual is that of a girl's first menstruation, a ritual which is little elaborated in comparison with other parts of Sri Lanka. When a girl menstruates for the first time, she is secluded for three days. Afterwards she is given a bath and a new set of clothes and jewelry. Throughout this time, it is said that her *maama* is in charge of the girl, although in practice it is usually his wife who stays with the girl.

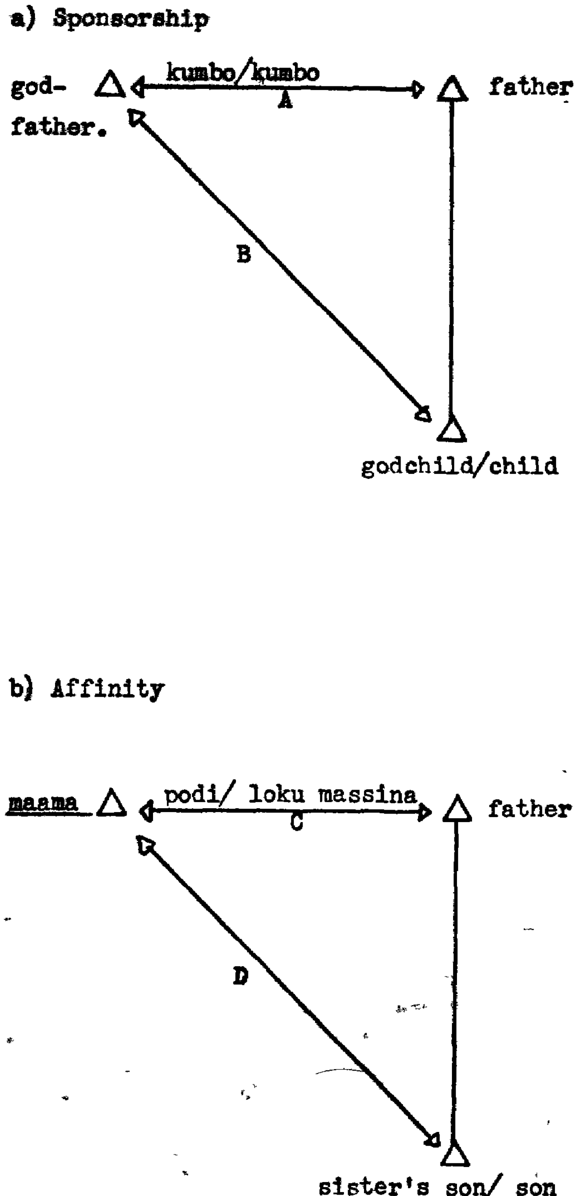
These three rituals are all concerned with the growth of the child both as a natural and a social person. The boy becomes a man through fishing; the girl becomes a woman through reaching puberty; both become full adults through marriage. Furthermore, all three rituals are related to original sin, or rather to the results of it: work and sex. The mother's brother is here involved in the socialisation of natural growth. He is carrying on the relationship he began when he gave his sister to the child's father in marriage and as an affine himself he officiates at those rituals concerned with mediating between the natural growth of the child and its social maturation.

We are now in a position to examine the relationships generated by baptism and marriage. Baptism results in the *kumbo/kumbo* and the godparent/godchild relationships; marriage produces the sister's husband/wife's brother and the mother's brother/sister's son relationships. Baptism ritualises the godfather/godchild relationship, what might be called the vertical axis of the set, whilst implying the *kumbo/kumbo* relationship. Marriage ritualises the horizontal relationship of wife giver and wife receiver, whilst implying the vertical relationship of mother's brother and sister's son.

First of all, let us compare the *kumbo/kumbo* and the wife's brother/sister's husband relationship. The most striking contrast is that whilst the *kumbo/kumbo* relationship is one of easy-going friendship of drinking and joking, that between wife's brother and sister's husband is one of asymmetry and reserve. Other contrasts come to mind. The *kumbo/kumbo* relationship can and should be reversed; the affinal relationship should not be reversed. As if to mark the opposition of spiritual and social, unless the choice of *kumbo* is being reversed, the preferred choice is an unmarried man, a man not involved in socialised sex. Finally, one does not invite one's *kumbo* to one's wedding. Social and spiritual are kept apart.

The opposition is again well marked if one contrasts the sponsor/sponsored and the mother's brother/sister's son relationships. Sponsors are responsible for the spiritual growth of the child and their presence is required on such formal occasions as first communion and confirmation. They may be present at their godchild's wedding and they should attend, but they have no formal duties to perform. Finally, the sponsorship relationship is activated at funerals, both that of the child and of the sponsor.

The mother's brother on the other hand, is responsible for social growth of the child. He performs at a boy's first fishing trip; at a girl's puberty rite and most im-



GURE 3. Sponsorship and affinity.

portant of all, at marriage. The contrast between the two relationships is most clearly seen at a girl's puberty rites. Whilst the mother's brother has to attend, the sponsor must not attend.

In structuralist style, the situation can be diagrammed as in figure 3. It can be read as follows: relationship B is to relationship D as spiritual is to social. This opposition is marked at the senior level by the opposition of relationship A to relationship C. Relations of spiritual affinity stand in contrast with relations of affinity.

This, however, is only one aspect of the analysis. If we return to the ethnography of sponsorship I outlined earlier, it is clear that the people do not make an explicit comparison between sponsorship and affinity. Rather they contrast the rituals of baptism and marriage and the relationships of parent and child with that between godparent and godchild. This links back to the two-fold significance of marriage. Marriage not only creates relationships of affinity; it also creates the nuclear family.

The 'sanctity of the family' is a common Catholic theme in Wellagoda no less than elsewhere. Church sermons continually reiterate the ideology of the importance of the family, how it is the building block of society, how everyone needs the support of the family, and so on. The conscious model for emulation in family life is the Holy Family, the *suddha pavula* In every house in the village, there is at least one small altar in the main room dedicated to the Holy Family and before which prayers should be said every night. Upon the altar are three small statues: Joseph, Mary and Christ. These three are the models for all, Joseph being the perfect father, Mary the perfect mother and Christ the perfect child.

Yet the Holy Family is a peculiarly ambiguous concept. On the one hand there is what might be called 'earthly family', the model for emulation, consisting of Joseph, Mary and Christ. On the other hand, there is what can be called the 'sacred family' of God, the Virgin Mary and Christ the Son of God. As Leach has pointed out, this ambivalence is essential if Christ is to be legitimated both as the Son of God, through his father God, and as 'of the line of David' through his earthly father Joseph (Leach 1967). Like most Catholics, however, the people of Wellagoda do not make such a distinction between the two versions of the Holy Family, but collapse them into one.

It is within this contrast of the 'sacred' and the 'earthly' family that the second aspect of sponsorship, that stressed by Gudeman, has to be seen. Baptism is spiritual birth, or at least, spiritual regeneration. The relation of the child to its godparents is of a purely spiritual nature, analogous to the relations between Christ and His parents in the spiritual version of the Holy Family. This stands in contrast to the earthly family, to the relations between a child and his natural parents, which in turn is analogous to the earthly version of the Holy Family. The contrast forms a second sphere in which the relationships set up by sponsorship can be viewed.

The opposition and transformation of the sponsor/sponsored relationship and the parent/child relationship become clear when one looks at the duties involved. A child should obey and respect its godparents for they are like one's natural parents and should be treated 'as if they are parents'. A godparent should treat the child 'like an own child'. If the parents die, then the godparents have the ultimate responsibility for the child's welfare. If the child dies before confirmation, then the godparents are responsible for the funeral. Only afterwards when the child has

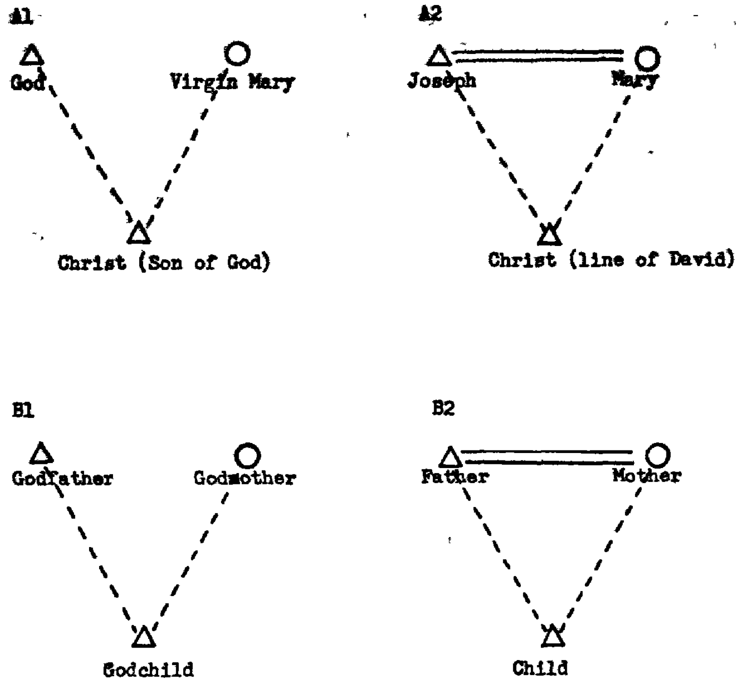


FIGURE 4. Models of the family.

reached spiritual maturity does the duty become the parents'. The godparents are responsible for the child's spiritual welfare; the parents for its secular welfare.

The logic of the aspect of sponsorship can be illustrated as in figure 4. In structuralist terms, it can be written as A1:A2 : : B1:B2. A1 and B1 both involve spiritual relations between parent and child. A2 and B2 involve natural relations between parent and child.[15]

In this context, although the idiom is the family, what is of greater importance is filiation and the contrast between natural and spiritual filiation. Here, marriage is of secondary importance, and occurs only in versions A2 and B2 Marriage in this context stands solely in relation to sex; to socialising the animal nature of man. On the other hand, spiritual filiation by its very nature does not involve sex and thus in A1 and B1 there are no marriage relationships. So one finds that a married couple are never chosen as sponsors for a child, although a couple of sponsors may marry later. If a married couple were chosen as sponsors, this would threaten the purely spiritual relationship of filiation with their godchild.

This stress on natural filiation leads back to the problem of the mother's brother, the *maama*. Earlier in this article I defined the *maama* as an affine, and the mother's brother as the *maama* par excellence. But things are more complicated and involve a further twist to the natural/social/spiritual opposition.

In the context of the marriage regulations, the mother's brother is banned as a

husband for his sister's daughter. But the marriage is banned not because of an affinal relationship (*vivaha naekaema*) but because of consanguinity (*le naekaema*, lit. 'blood relationship'). Wellagoda people say that one cannot marry one's close blood relations because the blood is the same. So one cannot marry one's father's brother's mistress's daughter because the blood is the same. They are all natural relations, related by a common natural substance. Any sexual union between close consanguneal kin is dangerous and a marriage which breaks the bonds of *le naekaema* is said to result in the birth of deformed, monstrous or imbecilic children.

I would suggest that in this case, the mother's brother is both an affine and a consanguine; that a child is related to his mother's brother both by affinity via the father, and by filiation through the mother. Thus in the rituals I have mentioned earlier, the mother's brother is an affine, but in terms of marriage restrictions, the child is related to the mother's brother through consanguinity. Affinity and complementary filiation are not mutually exclusive categories but are rather different ways of looking at the same phenomenon, and thus the nature of the relationship depends upon the particular context.[16]

To complete this analysis of *compadrazgo* in Wellagoda, we must finally look at the marriage restrictions associated with baptism; those restrictions couched in terms of *gnana naekaema*. Referring back to figure 2, it is obvious that A cannot marry B1 as this would replace a spiritual bond with an affinal bond, and as I have shown these stand opposed and contrasted. Secondly, A and B1 are 'like parent and child', so again sexual contact or marriage would be out of place. B2 is, however, marriageable for all the As as it is only her sister who is a spiritual relation of the As. For B1, however, the situation vis-à-vis A1 and A2 is ambiguous. A is only her spiritual father whilst he is the natural father of A1 and A2. Therefore there should be ban on marriage. But because A is a sort of father to A1, A2 and B1, then they are all a sort of sibling, and so marriage would be out of place. Just as the two versions of the Holy Family are collapsed into one, so are the two versions of paternity, the spiritual and the natural, and the result in both cases is ambiguous.[17]

If we take the results of marriages which break the bonds of *gnana naekaema* into consideration, we find that once again we are back to the distinctions of spiritual, social and natural. The offspring of unions which break the *gnana naekaema* restrictions are said to be spiritually doomed, for they have no chance of reaching heaven, i.e. punishment is of a spiritual nature. Those unions which break the bonds of *le naekaema* lead to the birth of monsters, i.e. un-natural offspring. Breaking the *vivaha naekaema* restrictions leads to disasters of a social nature: houses burn down, fishing gear is lost, and so on.

IV

I have tried in this article to produce an analysis which encompasses the theories produced by both Gudeman and Pitt-Rivers. Each in itself is insufficient to explain and analyse the institution of *compadrazgo* as it exists in Sri Lanka. Elements of both approaches have to be reformulated and refashioned to produce a more all-embracing frame of analysis.

In his work, Gudeman brings out only one aspect of the problem. By focusing on the relationship between birth and baptism he reduces the *compadrazgo* set to the

contrast between two types of family, the earthly and the spiritual, and this leads to a confusion between the spiritual and the cultural. Furthermore, he misses the contrast between marriage and baptism which is crucial for Sri Lanka. The 'earthly' family is in itself a mediator between nature and culture, socialising man's inherent animal nature. Because he misses this set of contrasts, Gudeman is unable to deal with the relationship between the natural and the spiritual parents in a persuasive manner.

Pitt-Rivers's analysis is insufficient for different reasons. Whilst he recognises that *compadrazgo* must not be viewed solely in terms of the family but rather in the wider context of kinship, his emphasis upon 'collectivities' and the individuating role of the godparent leads him astray from the central point. He, like Gudeman, fails to recognise the relationship between marriage and baptism.

The basic contrast in this Sri Lankan case of *compadrazgo* is that between baptism (and confirmation) on the one hand and marriage on the other. Both are concerned with denying the 'natural' or the 'animal-ness' of man. Marriage creates both the nuclear family, a socialised version of natural reproduction, and relationships of affinity. Baptism creates the spiritual family, modelled after the nuclear family but stressing the spiritual nature of man. It also sets up relationships of spiritual affinity. *Compadrazgo* in Wellagoda depends upon a triadic notion of man as a natural, a social, and a spiritual being, a notion which is worked out in the concepts of kinship.

In a sense, what we have here is similar to Lévi-Strauss's 'unit' or 'atom' of kinship (Lévi-Strauss 1963: 46, 48). For him, only four terms are required: brother, sister, father and child. In Wellagoda, however, we have to add a fifth term: the godparent. Just as, according to Lévi-Strauss, a man cannot marry his sister and thus has to give her to another man, so because the father of a child is not allowed to be the spiritual keeper of the child, the spiritual welfare of the child must be settled upon the godparent.

The analysis I have proposed here is concerned with one variant on the Great Tradition of Catholicism: with one Little Tradition. Thus it is only to be expected that this analysis will not hold good in every case of *compadrazgo*. Yet even a cursory reading of the literature indicates that similar structures are apparent in other ethnographic instances, for instance, in the Serbian data discussed by Hammel. As one of his informants remarked,

> Now young fellow, I think I see what you want to know, and I will tell you. We have kinship, alliance and godparenthood (Hammel 1968: 4).

NOTES

The fieldwork on which this article is based was carried out in Sri Lanka between 1969 and 1971, supported by the Social Science Research Council, the Smuts Memorial Fund and the Esperanza Trust. Some additional data were obtained during fieldwork in 1974 also financed by the Social Science Research Council. Earlier versions of this article have been read to various seminar groups all of whom I wish to thank for their advice and interest. I should also like to acknowledge the help and critical comments received at various times from Professors Leach and Tambiah, and Drs J. P. Parry and M. C. Jedrej.

[1] The 'instrumental' approach is stressed by such writers as Osborn (1968); the 'solidarity' approach is seen in the work of Mintz and Wolf (1950) and Foster (1953; 1961). Frequently

there is a confusion between the uses that an individual can make of *compadrazgo* ties (a function for the individual) and the role of *compadrazgo* in society as a whole (a function for society).

[2] By '*compadrazgo* set', Gudeman means the set of parent, child and godparent, a set which is based upon the 'sharing of the baptised' (Gudeman 1972:47).

[3] Gudeman can also be criticised for his use of Christian texts. The writings of St Thomas Aquinas are crucial to his analysis for it is here that he isolates the spiritual/natural opposition, a theological opposition which is used as the basis for a sociological analysis. Gudeman claims that baptism is concerned with spiritual birth and is contrasted with natural birth. But Aquinas's thought on the matter is slightly different, for he does not compare parturition with baptism. Rather, baptism is contrasted with conception, with the 'mixing of the seeds', whilst birth is contrasted with confirmation (Pt. III Suppl. 56 2 Obj.1).

[4] By this I mean that the structural principles of *compadrazgo* cannot be approached in terms of corporations. See Dumont (1957).

[5] 'Wellagoda' is a pseudonym. I should stress that the data I am discussing here are limited to this one village, and that the form of *compadrazgo* appears to vary over very short distances along the west coast of Sri Lanka.

[6] Throughout this article I shall use Sinhalese terms rather than their Tamil equivalents.

[7] Indeed, it is a moot point whether one can talk about 'caste' in this context, but rather should use the indigenous term, *jaataya*. Nevertheless, the villagers think of their *jaataya* as being comparable with the *jaataya* which occur elsewhere in Sri Lanka and which are commonly called 'castes'.

[8] See Banks 1960; David 1973; Leach 1961; Obeyesekere 1967; Tambiah 1965; Yalman 1962; 1967.

[9] Recently (1974), the Church has begun to insist that mothers should accompany their child to baptism. The result has been to delay the date of baptism, and children are not being baptised until they are two or three months old.

[10] In fact the actual relationship between the soul and baptism is rather unclear. Although the child has a soul before it is baptised, it is only by being baptised that the child has a hope of salvation. Otherwise it might as well have no soul.

[11] See Yalman 1967. 306.

[12] This is not to say that caste status is a *physical* substance, but rather that it is an attribute of the person with which an individual is born.

[13] Here I use 'affinity' to describe the relationships set up through marriage 'Spiritual affinity' will be used interchangeably with 'spiritual kinship' to denote the relationships set up through baptism and confirmation.

[14] This distaste for the repetition of marriage links appears to be related to the stress in Catholicism upon the status of marriage. In an area such as south Asia, marriage is debased. In Dumontian terms it is subsumed by the institution of alliance (Dumont 1957: 25); for Yalman it is predicated by the brother-sister tie (Yalman 1967; 1969). Here we find exactly the opposite tendency. Each marriage is unique in itself, repeating no previous affinal tie and being outside the range of second cousins.

[15] I should perhaps stress here that I am working in terms of the Wellagoda people's concepts of Christ's paternity. As they normally fail to distinguish *pater* from *genitor*, the relationship between Joseph and Christ is seen as a natural relationship of blood rather than as the relationship between a cuckolded father and his wife's child.

[16] I would suggest that this is true elsewhere, and that in most ethnographic cases, the argument as to whether the mother's brother is a consanguine or an affine may well be misplaced (for a similar view, see Yalman 1969: 626). Rather, he is both, and his role as an affine or a consanguine will be stressed depending upon the particular context.

[17] This ambiguity is similar to the ambiguity of the relationships which Gudeman calls 'political' (Gudeman 1972 58). In Gudeman's case, *politico* is used to describe the relationships between the previous children of a married couple, between a child and an in-marrying aunt or uncle, and between a godchild and his godfather's children. Gudeman seems to imply here that conjugal and *compadre* ties are equivalent. Rather, all that is equivalent is the ambiguity in each case: brothers but not quite; uncles but not quite.

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DEVELOPMENT
Review Article

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'Development' is a weasel-word. In general discussions, not ideologically committed, of social change it may be equivalent to 'evolution' without the unhappy social Darwinist connotations of that word. Indeed Krishnan Kumar, in one of the books under review,[2] asserts that social theorists, after neglecting such questions for several decades, when they took them up again 'reverted almost without modification to the basic form of nineteenth-century evolutionism', and in another of them[9] 'development' in the English title is represented by 'evolution' in the French Kumar is referring to Talcott Parsons, and is not interested in the many social anthropologists who have traced the changes that the expansion of Europe has brought to the peoples whose lives were previously organised around production for their own subsistence. In broad lines there is a one-way process: techniques *are developed* as knowledge increases, and to take advantage of them calls for new forms of social organisation. Marxists and functionalists alike recognise this; their difference lies in the evaluation of results, and here, whatever Parsons may argue about the world as a whole, social anthropologists have never tried to maintain that they were everywhere wholly beneficial. The word is used in its neutral sense in the record of the last International African Institute seminar organised by Daryll Forde, *The development of indigenous trade and markets in west Africa*, edited by Claude Meillassoux.[9] Here there is a striking difference in the approach of the French and British participants. The former follow a strictly evolutionary line, assigning the institutions and processes they have observed to their place in the marxist schema. The latter's contributions are more open-ended; thus Abner Cohen discusses the organisation of trading diasporas with his own study of the Hausa cattle trade as a base, and Polly Hill the trade in grain carried on by secluded Muslim women from their own houses.

'Development' began to have a narrower connotation for British readers when, at the end of the second world war, grants began to be made under the Colonial Development and Welfare Act, and, later, 'aid' to 'underdeveloped countries' became the responsibility of a Ministry of Overseas Development. It has been remarked that 'underdeveloped' is a euphemism for 'poor'. In its turn this adjective came to be considered opprobrious and was replaced by 'developing'; London University's Institute of Education created a department concerned with developing countries. Officialdom has settled for 'less developed', and 'ldcs' is now a term of art. For economists 'development' means an increase in gross national product, and there was a phase in which they looked to psychologists and anthropologists to tell them what characteristics, temperamental or social, were favourable or unfavourable to development so conceived.

Man (N.S.) 10, 607-612.

Development, then, has come to mean change in the direction that the speaker thinks desirable; and the word has taken on a new meaning for those who are concerned more with the equitable distribution of wealth than with its total production. Although many of these adopt the neo-marxist interpretation of the present situation advanced by the 'academic Ché Guevara', as de Kadt[2] calls Andre Gunder Frank, it is not only marxists who are concerned to find that current development policies have done much to create unemployment and not enough to alleviate poverty. Several collections of essays by British writers published in the last year or two examine the present situation in the light of Frank's theory, and one book, that by Colin Leys on *Underdevelopment in Kenya*, applies it in detail to a single state. The essence of the argument is that the world must be looked at as a whole and interpreted by class analysis; this demonstrates that the exploiting classes are everywhere interlinked, the governments of the poor countries being dependent on the capitalists in the rich, and leads to the conclusion that only a peasant revolution—world-wide, or in country by country?—can effect any amelioration. There is enough truth in the description of the present state of affairs to make the theory attractive; it has been stimulating to enquiring minds while providing the less critical with a pre-fabricated framework into which to fit their data.

Emanuel de Kadt, who edits *Sociology and development*,[2] is one of the former. 'Not all interests', he writes, 'are class interests, nor is all "authentic" consciousness class consciousness'. If 'the system' can only be changed by world revolution, this is something beyond the power of sociologists to influence. And he is not sympathetic to 'the group that gleefully watches things getting worse in the belief that only then will they get better'. They are represented in his collection by Wertheim, who admits that the first moves for the emancipation of peasants have commonly been crushed by right-wing revolutions, but takes comfort from the fact that this increases class solidarity. World revolution, he concludes, 'need not be scrapped off the agenda. What needs to be scrapped is modernization from above on a world-wide scale'. The same approach is adopted by Rayah Feldman in the Hull School's *Beyond the sociology of development*,[10] who doubts whether Nyerere's policy of checking the process of differentiation can lead to 'a true social revolution'.

Colin Leys[6] regards the imputation of such views to dependency theorists as calumnious. He sees the present condition of Kenya as the outcome of Kenyatta's fear that the withdrawal of foreign capital would lead to economic collapse, along with the ambition of the new African bourgeoisie to become large farmers on the European pattern. Kenya attained independence without the radical economic change that could have enabled it to break away from the constraints of an 'imitation bourgeois state'. Such a change would have followed very much the lines recommended by an ILO mission in 1972, but whereas their report assumed that it could be made by the exercise of wise leadership within the existing political and economic framework, Leys observes that there is within this no interest strong enough to challenge the vested interests of the status quo. Although he mentions the possibility of revolution he does not treat revolution, or even the situation that might produce it, as inevitable; on the other hand, he is not alone in foreseeing a struggle of some kind after Kenyatta's death. But the outcome, as he sees it,

must depend on the vicissitudes of international capitalism as a system. Modestly, he claims for academic studies that all they can do is 'try not to obscure the structures of exploitation and oppression which underdevelopment produces, and which in turn sustain it'.

David Lehmann's introduction to *Agrarian reform and agrarian reformism*[5] strikes a note that will be echoed by 'applied anthropologists', if there are any left, when he criticises the visiting experts who offer programmes without any awareness of the circumstances in which they are expected to operate. Not all of us may share his assumption that land reform is important only as a stage on the way to socialism, but all would agree that the conflicts of interest which surround it are crucial to its success, and that the existing situation of landlords may affect its possibility. The schemes discussed in this book—in Peru, Chile, China and India—are all in the context of a capitalist regime, even China having been so in the early days of Mao Tse-Tung's power.

One respect in which many anthropologists differ from the neo-marxists is in their attitude to class analysis. Orthodox opinion on this subject is that class divisions exist whether actors and observers are aware of them or not; only mystification obscures them. But much recent fieldwork has shown the importance of other forms of alignment in the non-industrialised world. Ethnicity, a word which is not merely a euphemism for 'tribalism', is coming to be a theoretical topic in its own right, as urbanisation and national politics create new relationships between people whose fathers and grandfathers were isolated from all but their nearest neighbours. Certainly Kenyatta and other politicians may find it convenient to accuse their opponents of 'tribalism', this does not prove that there is no such thing. Cohen's account in the Meillassoux volume[9] of the Hausa diaspora shows how it is a moral community, and how it re-formed in face of a national policy that purported not to recognise 'tribal' divisions. Both Fallers and Peter Lloyd in recent writings find the concept of class stratification inadequate to explain their data. Fallers[3] introduces a collection of reprinted essays, published shortly before his death, with the statement that after twenty-five years spent in the study of this phenomenon he has come to the conclusion that there is no such thing. In the view that he had then reached, 'inequality' was a term 'both more straightforward and less loaded with cultural bias'. He found that the strikingly hierarchical Ganda had no word which he could use when he wanted to talk to them about social class, only words for dyadic master–servant or patron–client relations.

Peter Lloyd[7] describes the result of a study of urban Yoruba in very much the same vein. He defines class-consciousness not simply as the recognition of common interests but as 'the articulation of basically symmetrical relationships in pursuit of these'. Its obverse 'is not no consciousness, a kind of blank, but the dominance of asymmetrical patron–client relationships'. And a class-conscious man may be aware of a class dichotomy and yet prefer to seek his individual betterment through a relation to a patron. The Yoruba men whom he actually questioned took inequality for granted. In part this attitude stems from their traditional belief in destiny, but destiny to them is a limiting, not a determining, influence. Hard work can outwit destiny, laziness prevents its accomplishment. But to find a 'helper' is at least as important as working hard oneself; 'if one does not know an influential somebody one can get nowhere'. Such a patron is loudly praised

for his generosity, though, as Lloyd points out, it is by no means wholly disinterested; a personal following brings prestige and can be a means to greater power. Those who have no patron go on hoping that their luck will turn. Nearly all see wage-labour as a first step towards self-employment and so have not the obvious class interest of lifelong wage-earners. In contrast to the stereotypes of peasant and working-class attitudes offered by such writers as Gans and Banfield, Lloyd sees all sections of Yoruba as committed to 'modern and middle-class values of achievement'. Caroline Upton and Robin Cohen in their contribution to *Beyond the sociology of development*[10] also discuss stereotyped explanations of 'peasant conservatism' and argue, in effect, that although it may play its part it is not the sole reason for resistance to change; they seem to believe that 'those whose sociological careers are dependent on the maintenance of academic boundaries' are thwarting the kind of enquiry they would consider fruitful.

If one were to ask, in the light of the books examined here, what *is* the difference between sociologists and anthropologists, it would seem to be that the former work on the widest possible canvas, sometimes the whole world, and some of them consider it morally wrong to take a narrower focus, while the latter turn their microscopes on the interactions of smaller groups (not that the ten million Yoruba are such a very small population), and sometimes find that their stories differ in detail—the sociologists might say not in significant detail—from what grand theory predicts. They sometimes examine the social context of a single innovation or limited area of policy, as do Frankenberg and Joyce Leeson[2] when they discuss the unnecessary cost of medical services in Zambia, and conclude that the country would be better served by those Chinese-style 'barefoot doctors'—not unlike the New Guinea 'medical tultuls' except that they practise acupuncture and are not regarded as members of a distinctive profession—through whom, it is argued, the masses' desire for health is effectively mobilised. Perhaps it is true that Western doctors practising in the tropics have been too thoroughly socialised in one tradition to think they have anything to learn about, or from, their patients, but to call this 'imperialism' is to make the word an imprecation rather than an explanation. Western doctors have rarely taken up work in the tropics from self-interested motives, and have never, as far as I know, been made an instrument of colonial policy.

When Henri Dunant saw the horrors of Solferino, he did not wait for the revolution that would abolish war, but created an organisation to mitigate the sufferings, and sometimes save the lives, of men whose lives were not in any case likely to last until the revolution. Those of us who called ourselves 'applied anthropologists' used to think, in the same way, that our knowledge could be of value to specialists who were trying to introduce small-scale improvements in hygiene, food production and the like; we thought it was worth something for people who might not live to see the revolution—which we had not then been told was the only solution to the problem of poverty—to have a little better health and a little better nutrition in the meantime, and that we knew something about the difficulties that the specialists might face from immediate local pressures. Such small-scale 'palliatives' are still sought, but sadly, to judge from recent publishers' lists, it is no longer professional anthropologists who write about them.

Guy Hunter's *Modernizing peasant societies*[4] is published under the auspices of

the Overseas Development Institute, who, although they are well aware of the difficulties in the way of making development aid effective, naturally have not given up hope of doing so under the present world political system. He is at one with the marxists in holding that agriculture rather than industry should be the key, but not in accepting the inevitability of the kind of neo-colonial system that they describe. His aim is a technical one, first to increase food production and then to enable farmers to earn an income that will provide an internal market for the goods to be produced by the increasing numbers who can no longer draw their subsistence directly from the land. Indeed he would have subsistence farming disappear altogether, and all agricultural produce be marketed. The measures he advocates to this end are technical—notably irrigation—and political, land redistribution and the provision of credit. In the latter field he is aware of all the possibilities of evasion by landlords, but does not discuss what Leys and Lehmann would put in the forefront, their influence over the reform legislation itself. However he confronts squarely a problem that can hardly be left to revolutionary enthusiasts alone—the absolute necessity for the enforcement of minimal standards of cultivation. He finds his answer in the improvement of extension services, but cannot tell us how they can avoid being identified with a meddling, unpopular 'Them'.

Of course he knows that the answer lies in a real appreciation of farmers' problems. The same advice is put more succinctly in Robert Chambers' newest book on the management of settlement schemes.[7] Without sharing their assumptions he agrees with the marxist writers that too many such schemes have benefited only those who were already better off, but he is more concerned with the necessity of securing the acceptance of plans which must, he insists, be made from above, though with greater awareness of farmers' circumstances than has been common up to now. His central interest is in administration, and he is as much interested in the problems of the lower-level agent of development as in those of the farmer. It is these agents, he argues, who must be 'brought into the picture', allowed to discuss the feasibility and difficulties of the programmes they are required to implement, and above all not burdened with endless meetings and the superfluous collection of data. His observations were made in connexion with a promising scheme launched in Kenya in 1971, which was abandoned after only a year—a point for Colin Leys? His book is published in Uppsala, and appears to have escaped the notice of librarians in Britain.

Finally Peter Marris[8] interprets conservatism in general as an essential protective mechanism, that which imposes continuity and meaning on experience, and not only for the fortunate. He too is concerned with resettlement, in this case slum clearance, but also with wider phenomena such as 'tribalism', which he is far from regarding as a mystification intended to check the development of class-consciousness. His general theme is that a significant part of adjustment to changed circumstances is a process of 'mourning for the irretrievable past', and—here as far from Leys as from Cohen—sees attachment to tribal identity as a manifestation of this. 'Tribal institutions are concerned, above all, with the confusion of identity, the loss of stable meaning in relationships, which arise when people who speak different languages must live together'. His most interesting contribution to the problems of development is his discussion of the disillusionment of American city

authorities with their first experience of community participation in planning. The co-opted representatives of the poorer citizens did not co-operate with the city fathers but treated them as enemies. But, says Marris, the lines of conflict must be drawn before the process of bargaining for accommodation can begin. Perhaps there is a lesson here for those who offer participation in many other fields.

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OBITUARY

MAURICE FREEDMAN 1920–1975

Maurice Freedman was born in the East End of London to a Jewish working-class family. He was proud of this background and he seems to have returned to his childhood experiences constantly when later in life he turned to the academic study of the human condition. However, he first pursued studies in English but this career was interrupted by the War, a period which he partly spent as a soldier in India. The experiences he gained in that country seem to have had a catalytic effect on his mind, and when he returned to academia it was as a student of social anthropology at the London School of Economics. It was now he developed his interest in interethnic relations and minority problems. He edited a book on the Jewish community in Britain and did fieldwork among overseas Chinese in south-east Asia. One of his best known books, *Family and marriage in Singapore* (1957), is the result of about two years' research in Singapore. Freedman's mind was always open, he was constantly looking for new problems and toyed continuously with new ideas. He never wanted to become a 'specialist' in a narrow field like minority studies, he soon turned to the intricacies of peasant problems and explored personally two different rural social landscapes He made field investigations for WHO in Indonesia and conducted further field work in the New Territories of Hong Kong.

His experiences of urban Chinese society in Singapore and elsewhere brought him to reflect upon the background of the Chinese settlers in south-east Asia, reflexions which, in their turn, led him to write two magnificent monographs, corner stones in the anthropological study of China—*Lineage organization in south-eastern China* (1958) and *Chinese lineage and society: Fukien and Kwangtung* (1966). Political events had made China a field closed to foreign scholars, so Freedman turned to libraries and archives, scrutinised every piece of writing on south-eastern China, and fed his creative imagination with rediscovered data to produce an explanatory model for traditional Chinese kinship organisation. His two lineage volumes must be deemed classics not only in sinological anthropology but also in the broader fields of kinship studies and historical anthropology. In many ways he opened up the field of China to modern anthropology. He was not the first one to be interested in that country—there were many talented people who had worked in and on Chinese society before him—but he was the first one to give a truly systematic account of a great number of puzzling and superficially contradictory pieces of information, the rich yield provided by his investigating eye.

Freedman's work in the ethnohistorical field convinced him that something was wrong with anthropological conventions. These new considerations concerned the study of complex societies, such as that of China. He pleaded with passion for cross-disciplinary studies, urged a break with the village study tradition, and envisaged a new sort of anthropology which aimed at a synthetical understanding of the large and complex phenomena that we intuitively call civilisations.

Phrasing Freedman's career differently one might say that he pioneered urban anthropology (then not yet fashionable), turned to the historical reconstruction of social forms, and then tested his ideas about the past through rural studies in the field (the New Territories). His experiences from 'residual China' led him to entirely new problems, this time concerned with symbolism. His thinking became deeply involved with the use of symbolic expression. From a start with ancestor worship he developed particular interests in such phenomena as geomancy and divination. The latter was to have been the topic for a lecture series he planned with much enthusiasm for that future which never came.

Maurice Freedman taught in many places—London, Oxford, Kuala Lumpur, Yale. He was to leave the chair which was once Malinowski's to take up that which was originally created for Radcliffe-Brown. I do not think he cared all that much for predecessors, nor was he particularly concerned with any particular 'British school'. He was a true internationalist and as such an undisputed leader (he would not have liked the word) for a multinational congregation of anthropologists and sociologists concerned with Chinese society. His seminal influence cannot be overestimated. He was a true focal point, a 'transformer' who worked by stimulating others, absorbing their results and creating new hypotheses. In this way he steadily worked on that synthesis which was his vision of China. Recently two of his colleagues wrote: 'We wrote our books and papers for him.' In a sense I think we will continue to do so.

University of Gothenburg

GÖRAN AIJMER

CORRESPONDENCE

Anthropology and gossip

SIR,

I am sorry such a long time must elapse between a criticism and a reply, but I did not see Mr Machin's comment on my paper until June.

Mr Machin's point is well taken, but all too obvious and crude. Of course Makah, other American Indian tribes, and former colonial peoples resent the whites for what they have done during the course of conquest and exploitation. Consequently it is natural and expectable that they will show whatever form of resistance is available to them. But no one is going to say that a nineteen-year-old female graduate student was responsible for taking away lands, despoiling fishing grounds etc., nor can we assume she condones such acts. Yet such an anthropologist was clearly resented, as I am suggesting many other anthropologists have been mistrusted. What is it, then, that the anthropologist represents to the subjects of his enquiries that they find particularly disquieting? Granted he or she comes in as part of a long line of missionaries, officials, entrepreneurs and other 'benefactors' with whom the anthropologist is identified in some way or other, there still remains the question of pinning down what, if any, is the particular offence committed by the anthropologist. And there is the question as to whether that offence is committed by anthropologists against people who are *not* victims of colonial exploitation. To take a well known example, Ronald Frankenberg studying a Welsh village reported his own situation in similar terms to Colson.

The answer I proposed in my article was that the anthropologist invades the privacy of a community and of individual members of the community. He does so by seeking information concerning kinship, genealogy and interpersonal status, all of which make up what I called the 'good name' of the community and of individuals. Now Mr Machin would like to think that emphasising this as the reason for informants' resistance to the anthropologist and the consequent vitiation of the anthropological report is nugatory. Let me in as few words as possible explain why I believe my argument is not only sound, but far more basic than his hackneyed and bludgeoning accusations.

It was very clear from Colson's account of the Makah that genealogy was the key to power within the Makah community—who held dominance and status over whom. It is equally clear that the social autonomy of the community itself vis-à-vis the outside world depended almost entirely on the ability of the Makah to keep themselves and their way of making decisions and settling affairs to themselves. So long as, and in so far as, individuals and the community could preserve autonomy they maintained a measure of power, and thence self-respect. In spite of all the batterings and destruction meted out by whites in previous years, the Makah (a rather composite group anyway) still were able to retain a sense of common identity and self esteem by organising their social lives according to standards and criteria of their own. Later on, when they became more sophisticated in dealing with state and federal governments, they would be able to win back some of their former economic and political rights. But at all times the most vital and basic fact is that they retain control over themselves and their decisions concerning themselves in those parts of their lives where they confront each other as persons who love and hate and as persons who inherit a common history and tradition unique to themselves.

This is the area of living that the anthropologist concentrates his study on: and this is the area of living that a community most needs to keep to itself. Individuals and a community seek to preserve their integrity by various ways and means which I subsume under the term 'privacy'. One of these means is gossip, the relaying of information, true or false, about a good name. When an anthropologist pursues his studies among a foreign community he invariably seeks information which from his point of view seems of no political significance at all, but which, to him, has scientific value. But in pursuing these studies the anthropologist enters into a relationship with that community, and when he publishes his findings he pushes that community into a political relationship with the outside world. By the part he plays in procuring information within the community and using it to further his own inquiries he invades the privacy of individual members and threatens their autonomy vis-à-vis their fellows. When the sum total of the anthropologist's findings are communicated to the outside world he runs the risk of revealing the secrets of community life and organisation to outsiders, thereby compromising the privacy and autonomy of the community. This it seems to me is the particular offence that the anthropologist runs the risk of com-

mitting towards the subjects of his investigation.

What we have not fully realised is that our 'tribes' are, to state the obvious, people just like we are, and when we enter their lives we do so as another human being who becomes a part of the ongoing life-concerns of the community—as they become a critical part of our own concern. Let's face it, upon the 'success' of our fieldwork depends our own future as individual anthropologists—whether, when and where we will get a job, what others will think of us 'professionally' and what sort of reputation we will gain amongst our peers. Every fieldworker is in it, in part, for his own advancement and his own path to power, reputation, and good name.

In the matter of privacy lies the very heart of the sanctity and integrity of social life and power. The matter is far from trivial, far more crucial and subtle than the crude materialism espoused by Mr Machin. I regret that I cannot give fuller arguments in the space of a letter, and that I cannot attend to one or two other points raised in the correspondence. I hope to publish fuller statements on the numerous facets of the issue at some time or other.

Peter J. Wilson

University of Otago

SIR,

I read Peter Wilson's recent article 'Filcher of good names' (*Man* N.S. 9, 93–102) and Barrie Machin's subsequent criticism of it (9, 625–6) with great interest. I concur with two points raised by Machin:

1 Wilson's article *is* extremely competent and
2. his article nevertheless gives the impression of having missed the point—

although I would not see this point as having anything much to do with Machin's paradoxical 'anti-naivity' campaign.

Wilson is castigated by Machin because his treatment of the problem of privacy in the context of Makah-Anthropologist relationship rests on a misunderstanding of the real reasons behind the situation. The Makah, according to Machin, espoused the defensive reaction of a colonially oppressed minority using what weapons were still left to them against a member of the oppressing majority.

'Privacy' in this context would be a *Scheinproblem*. I am inclined to think that there is something true about this criticism; but I take exception to the crudeness and superficiality of the criticism, which is the more surprising as Machin accuses Wilson of proffering an argument unworthy—in the sense of too trivial?—for publication in 'so prestigious a journal as *Man*'. Attempts such as Machin's at fighting bourgeois preoccupations with vulgarised Marxism and a naive belief in class-struggle merely try to exorcise the devil with Beelzebub.

Wilson, equipped with his *termini operandi* 'gossip and privacy', though desirous of achieving a synthesis between what he calls psychological and sociological points of view, soon lapses back into a modified version of the functionalism which he claims to fight. It is admittedly a more elegant brand than Gluckman's, but nevertheless the ugly dragon is not slain but presented now in the acceptable disguise of a dinosaur. Some coup-de-grâce! A socioculturally tangible and isolable phenomenon (gossip) is presented as performing a useful *function* ('privacy protection') in relation to another such phenomenon (the privacy-hungry individual). This scenario of social rationality, to my mind, is both a failure as a synthesis and a ready target for materialist diatribes A true synthesis should strike at the very roots.

In Wilson's version of the Makah situation although at first sight the reaction is seemingly that of the group, it is implicitly posited that the self-interest of the individual, the status-concerned, power-seeking individual is at the root of it all. Wilson's concern with 'privacy' seems to me to spring from a long philosophical preoccupation with the individual It is firmly grounded in Kant's postulate of an unbridgeable opposition between individual interest and public interest, selfishness and duty, of which no unification would ever be possible The inevitable conflict between the ambitions of the individual and the duty to the collectivity commanded by the categorical imperative inspired a long line of bourgeois thought, and in particular the so-called *Lebensphilosophien* of Dilthey and Nietzsche. Nietzsche spent a lifetime finding the existential rules that would free man from the shackles of duty and collectivity There are other respects also in which I would see a parallel between Nietzsche's and Wilson's thought.

Privacy, so Wilson argues, leads to power; or rather, the assertion of power is contingent upon privacy. The thrust of Wilson's argument is that power arises from separation—this is what privacy ultimately seems all about—rather than, as one would expect, from interaction. Extreme privacy, logically implies total separation, i.e., isolation. How this could lead to the assertion and exercise of power remains mysterious. There is either a *non-sequitur* here or some kind of cryptic psychologism. Privacy, in his opinion, is achieved and maintained by the concealment and obscuration of one's curriculum vitae and genealogy. The power, based on 'privacy' then, is the result of not-knowing on the one

side, and of successful strategies of concealment and mystification on the other. What results is 'the power of the mysterious'. (Perhaps sometimes the mysterious does have power over man, but this remains at best a special case.) This argument sounds familiar to those acquainted with Nietzsche's writings. The unfolding of the *Individuum* takes place through one's arbitrary assertion of separateness from the collectivity. The assertion of singularity and separateness is what life is, or should be all about. And life is *Wille zur Macht*. Again, with Nietzsche as with Wilson, separation mysteriously leads to power (there is only a difference in emphasis and valuation between the two writers).

Wilson's model, though fascinating, is somewhat too simplistic and lopsided as it operates with variables which are both too few and too exclusive. Above all he neglects the dialectical relations between idea, context and history. Wilson's ideological home ground is Kantian-based preoccupation with individuality carried into the sociological field of discourse. By uncovering the roots of this concern one can see that it is intrinsically an extension of the identity problem At its most basic level this problem is a derivative of the contrasts of oneness—separateness, or selfness—otherness This basic dichotomy is capable of resolution and application in many diverse ways and there is an unceasing dialectical dialogue between the antithetical sides. Gossip and power are part of this dialogue and therefore they are not positivist social facts that may gainfully be studied outside of context The dialectical dialogue moves in the *force-field* (Adorno) that extends between idea (contrast) and reflected object, momentary situation, changeable context, etc Wilson falls short of examining the subtle relationships prevailing between idea and context.

From a philosophical tradition which has already been subjected to a heavy barrage of criticism, he derives a model of a type which is no less questionable in his hands than in Nietzsche's, and which he then goes on to apply in a clearly inadequate way.

E Kolig

University of Otago

SIR,

Machin (*Man* N.S. 9, 625–6) commenting on Wilson's recent article on gossip (*Man* N.S. 9, 93–102) makes the sweeping statement that '*ad hominem* or *ad feminam* attacks are less fearsome than weak arguments, suspect methodology, theoretical monism, ahistoricity, participant insensitivity and academic etiquette' (626). Why Machin should bother discriminating the bad from the worse defies the imagination as he succeeds in committing most of the errors he so roundly castigates. Perhaps his own preference for *ad hominem* argument accounts for this. He cites Wilson as dismissing what he considers to be Gluckman's 'outmoded functionalism'. But the latter half of the quotation refers to gossip as 'information promulgated in self-interest' which is surely Paine's argument (1967) rather than that of Gluckman. Colson's account (1953) of the Makah might not be to Machin's liking but his statement that Colson gives a 'benign' description of the Makah experience is his own interpretation presumably designed to fit his own argument rather than an interpretation likely to be widely adhered to by other anthropologists. Both Colson and Gluckman (1963) in his re-analysis make considerable point of the oppressive experience of the Makah.

Machin falls into the trap of attaching pejorative and non-discriminatory labels, e.g. functionalism, to a variety of different analytical perspectives applied by anthropologists. His argument implies that the functionalism of Colson, Gluckman and Malinowski are all one and the same It denies, for example, Gluckman's radical departure from Malinowski's approach (*see* Gluckman 1948). Perhaps he might benefit from reading *Malinowski's sociological theories* (1948), if only to increase his own understanding of Gluckman's position.

In common with many anthropologists I am in agreement with the tenor of what I gather to be Machin's position. This is that anthropologists should be concerned to describe the frequently oppressive contexts they encounter as they are and must be concerned with the use to which their accounts are put. Anthropologists must confront critically the societies they study and the wider systems in which they are located. My point is that Gluckman with his 'outmoded functionalism' did precisely this. It is well demonstrated by his stress on locating points of contradiction and conflict in social and political systems, with his concern to examine change as a dialectical process, to isolate and map the sources and dynamics of rebellion and revolution, and with his aim to place local events, situations and communities within their wider national, international and historical contexts (e.g. 1943, 1958, 1959, 1960, 1969, 1971) Major contributions of Gluckman include his examination of the oppressive structures of colonially dominated societies. It was Gluckman, more than any other anthropologist of the time, who drew the attention of his colleagues and students away from a 'tribal orientation' to a concern for the examination of the structure of colonial administrations, rural and urban economic

and political systems. But more than this, it was Gluckman, and many of his colleagues and students with their more radical views, who personally confronted colonial administrations and opposed their policies (see Brown 1973). What is significant about this is that in the late 'forties through to the early 'sixties, Gluckman and many of his anthropological colleagues were almost unique in their position. At a time when it was far from fashionable to hold the views they did, they were often penalised. Gluckman himself because of his views, was prohibited entry into two colonial countries, the Federation of Rhodesia and Nyasaland and New Guinea. At a time in anthropology when it is more acceptable and more worthy of approbation to hold such views I sincerely hope that Machin will, at the very least, be able to record such a career as that of Gluckman.

Ad hominem, your correspondent's use of such a terrible name as Treblinka in the context of the Makah, identifies himself as a competent user of 'newspeak and double think'. The death and destruction which occurred in both situations were social products of radically differing social formations and their conflation in the hands of a trained anthropologist is incomprehensible otherwise. One would need only to have read Howard Fast to grasp that mass murder is common to the history of American Indians and Polish Jews and enters the consciousness of the living of both groups Since your correspondent finds Orwell's ideas useful he might find it worth reflecting on his notion of the 'smelly orthodoxy'. In this context the term usefully identifies those who, disregarding the historical and social locations of earlier anthropologists, rip out aspects of their work for tendentious and moralistic condemnation. 'Rip out' and rip off.

It is right and proper that anthropologists who assist knowingly in the subjugation of the people among whom they work should be condemned. But to associate the names of Gluckman and Colson with the names of such anthropologists is an insult. The question of 'academic etiquette' is irrelevant in the context of such a snide and vicious attack.

Bruce Kapferer

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Reindeer and men

SIR,

Tim Ingold's 'On reindeer and men' (*Man* N.S. 9, 523–38) contained much of interest. the circumstances of changes in herd management in north-east Finland, for instance; but his supposedly 'ethological' portrayal of the relationship between men and reindeer (with its sociological implications) cannot pass unremarked.

He speaks insistently of a 'social contract between man and deer' (p. 537). I suppose this to be an anthropomorphic fantasy which even as a metaphor for heuristic purposes is (a) unhelpful: his good points about changes in herd management and economic conversions can be grasped—and accepted—without the metaphor; and (b) ultimately misleading.

However, it seems as though Ingold is determined to be taken literally: the deer are involved in 'political/economic decision-making' (p. 523); their posited relationship with herders is one of value maximisation:

the value of security for the deer (p. 525); and (in conditions of intensive herd management) 'the deer recognises certain obligations towards its master. . . . Other men cannot command the same deer's obedience in this way: the relationship is exclusive' (p. 530); there follows a straight-faced explication of the reindeer:man relationship in terms of Radcliffe-Brown's well-known *in rem* and *in personam* distinction (pp. 530–1).

Nor do these statements purport to represent the herdsman's perception of his relationship with his animals but are made explicitly by the ethnographer (*cum* ethologist) on behalf of the reindeer. What first appeared to be an undisciplined parody of the sociological usage of words like 'contract' and 'transaction' (p. 525) now also looks like a parody of the claims of Pavlovian research.

Of course the ease with which animals may be manipulated by herdsmen is in proportion to the extent to which an environment of security and congenial routine has been established for the animals; but is there entitlement to posit a sense of obligation on the part of the animals? Of course herdsmen deploy their accumulated knowledge about the habits and responses of their animals, but where is the evidence for supposing that the animals have anything at all approaching a reciprocal body of 'knowledge', deduced from experience, about the herdsmen? Of course frightened animals will (in many circumstances) attempt to evade the herdsmen, but it is highfalutin' to begin to explain this in terms of a broken contract.

As far as I know, reindeer herdsmen *always* draw upon a combination of 'physical force and technological superiority' (p. 531), and not—as Ingold would have it—just when intensive management is abandoned. Wherever the close man animal relationship of intensive herd management was maintained it was (with local variations) through the use of dogs, lassoes, bells, harnesses, corrals, fences, smudge fires, salt licks—and when necessary the knife, for surgery (testicles, antlers) or for butchery. Indeed, one difficulty with Ingold's thesis is lodged in his characterisation of the change from intensive to extensive herd management as one in which '*symbiotic* pastoralism is replaced by *predatory* pastoralism' (p. 537. my emphases).

For instance, what kind of persuasion (metaphorical, sociological, biological, ecological, ethological?) can make sense of the idea of 'a long-term contract of mutual advantage' (p. 525) or of a symbiotic relationship as opposed to a predatory one, between a reindeer owner and the large number of his bull animals that within a few days of their birth (under intensive herd management) will be castrated and/or slaughtered?

Anyway, do reindeer 'think'—and think 'transactionally'? Be that as it may, Tim Ingold misses the chance, as it seems to me, to ask 'Do *herdsmen* suppose their reindeer think?' But then, Ingold may say I have missed *his* point, Perhaps others have too? I would really appreciate it were he or someone else to clarify, better still to demonstrate, the ethological metaphysics of his interesting essay.

Robert Paine
Memorial University of Newfoundland

Sir,

Robert Paine is, of course, correct in seeing in my article a parody of a certain brand of transactionalist theory. I cannot deny that this intention was at the back of my mind when I wrote it. He is perfectly entitled to view this as undisciplined and unhelpful. In hindsight, I would admit as much myself. Paine does, however, question some of the more substantial points of my argument. Although I have rethought much of it since writing the paper two years ago, I would still stand by my main contentions, and therefore welcome the opportunity to clarify some of the issues that Paine raises in his letter.

There are really three themes, perhaps sometimes submerged or clumsily expressed, which I see as crucial to my argument, none of which Paine appears to have entirely appreciated.

The first is the necessity to separate *ideological* variables, concerning the articulation within human society of rights over animals, from *ecological* (or ethological) variables, concerning the nature of the *interspecific* relationship between human and animal populations. I take the distinction between hunting and pastoralism to be an ideological one. briefly, pastoralists recognise rights over live animals, hunters over dead ones. Now, I admit that the simple opposition between symbiosis and predation is hardly adequate to describe the wide range of forms that the man–animal relationship can take, whether the ideological context be hunting or pastoralism. Nevertheless, it is surely legitimate to contrast a situation in which animals 'in the terrain' *seek*, or at least admit, the presence of man with one in which they *avoid* him. Technically, if this presence is protective, leading to a *higher* animal population than would otherwise be sustained, then the relationship is a symbiotic rather than a predatory one, regardless of the fact that the majority of animals may, in the end, suffer death or mutilation by the herder's knife. After all, every animal ultimately dies, one way or another. The essence of the symbiotic relationship lies in the provision of security for the animals, and strictly selective husbandry which eliminates

only those animals incapable of further increase. Under such conditions, slaughter could be regarded as anticipatory rather than predatory; whereas in a predatory relationship, no provision is made for security, and a random factor is introduced into husbandry. In any case, I find Paine's assertion that herdsmen have always used force rather puzzling, in view of the fact that many elements in his subsequent list (para. 6) do not involve the use of force at all: for example bells, smudge-fires and salt-licks. Although corrals and fences have been generally, though not universally used, even in intensive management systems, the reliance on temporary or permanent barrier fences, strung out for many miles across the landscape, has become a prominent feature of herding campaigns only with the breakdown of traditional controls. Herding with dogs, unlike snowmobile driving, takes advantage of the natural response of deer to threat without using direct force. The lasso is useless until the deer is within roping distance, whilst only deer that are already broken in can be harnessed. Finally, I have never heard of a man controlling his animals' behaviour by threatening them with a knife! Here, Paine is confusing his own distinction between herding and husbandry.

The second theme of the article is to take up the differences between domestication and pastoral man–animal relationships. I stressed that the bulk of the pastoralist's expanding herd is domesticated neither in the sense of tameness nor that of genetic modification. The pastoral relationship, whether symbiotic or predatory, is one between a population of men and a population of animals: hence the transactional analogy. On the other hand, through domestication, as I have defined it, the animal is partially *incorporated* into a human group, establishing a particularistic relationship with its master, and performing certain expected duties for him. The assortment of quotations in the third paragraph, some dealing with domestication, others with the pastoral relationship, and the assertion that I am positing a sense of obligation on the part of undomesticated animals under symbiotic pastoralism (para. 5), suggests that this point has not been driven home. The confusion may arise from my misleading use of 'the contract', but I suspect it is also due to Paine's lumping together of both domestication and symbiosis under the same rubric—'intensive management'.

The third theme of my article is the contradiction between the ideals of pastoralism and the ecological foundations of pastoral controls. Pastoralists aim to achieve ever greater concentrations of animals on the range, but the natural response of the animals to increasing population density on inadequate pasture is to break down into ever smaller and more widely dispersed social units, making it impossible for the pastoralist to provide the security for his animals and exercise the strict selection that underly the symbiotic relationship. Control over reproductive increase therefore breaks down before population densities become so high as to bring about a Malthusian catastrophe. The point is simply that reindeer are not just passive objects but social animals; and their responses to changing environmental conditions are not altogether under human control, and may indeed *undermine* this control. Given the fact that man is such a ubiquitous feature of the reindeer's environment, it would be extraordinary if they did not react in some way, be it positively or negatively, to his presence.

Finally, Paine's preoccupation with the herder's perception of his relationship with his animals must stem from his current ethnomethodological bent. I can assure him that the Lapps amongst whom I worked certainly viewed deer as strategic agents whose tactics, sometimes stupid, sometimes cunning, by all accounts had to be reckoned with (see section VI of my article). But this is really a rather anthropocentric position. An eminent ethologist has aptly remarked that 'the animal's tendency to zoomorphize corresponds to man's tendency to anthropomorphize' (Hediger 1965: 29). It is unfortunate that reindeer are not in a position to contribute to academic debate.

Tim Ingold

University of Manchester

Hediger, H. 1965. Man as a social partner of animals and vice-versa. *Zool. Soc. Lond. Symp.* **14**, 291–300.

Palpable nonsense in the conflict of life and death

Sir,

Edmund Leach, in his comment on Peter Munz's letter, accuses Munz of substituting 'conflict' for 'binary opposition' and of then attributing to Lévi-Strauss and to himself opinions which are 'palpably nonsensical'. (*Man*, N.S. **10**, 316) Munz seems to make a habit of demolishing arguments which have not been proposed. He states on page 26 of *When the golden bough breaks*, 'Any attempt to interpret myths in terms of their original intentions, as Percy S. Cohen in his 1969 Malinowski Memorial Lecture has done, is therefore misconceived....' I happen to think that any such attempt is not only, in almost every instance, misconceived but inconceivable.

There *is* a great deal of muddle in the use of the terms 'contrary', 'contradiction',

'binary opposition', 'social contradiction', 'conflict', 'semantic opposition', 'system contradiction', and so on. I agree that Professor Munz is muddled. But I also suspect that Lévi-Strauss has helped to confuse anthropologists, that Edmund Leach, Burridge and Friedman have not succeeded altogether in clearing up the confusions and that other French 'structuralists' like Godelier, and especially Althusser, have done their bit in making matters worse. Much of the trouble stems from Hegel and from those who insist on perpetuating the confusions (and, in some respects, the obscurantism associated with such confusions) for which he was responsible. But is it really all so difficult? It is, if we insist on taking terms which have fairly clear meanings in one discipline and then applying them elsewhere, thinking that we have illuminated some new problem, whereas what we have done is to slide from one set of meanings into another, while thinking or pretending that we have not.

In formal logic terms like 'contrary', 'contradiction' and 'subcontrary' have quite clear meanings, and refer to *propositions*. thus p and q are contradictory, if they can be neither true nor false together, they are contrary if they can not both be true even though both can be false, and they are subcontrary if they cannot both be false even though both can be true Some social scientists think that they are stating a contradiction if they say that Britain is both capitalist and non-capitalist and they therefore believe that to state such a 'contradiction' is either impermissible (if they are conventional logicians) or both permissible and substantively *significant* if they are dialecticians.

Taken at its face value the statement is contradictory, because if Britain is capitalist it can not, logically, be non-capitalist, while if it is non-capitalist it can not, logically, be capitalist; and, it must be one or the other, because if it is not non-capitalist it must be capitalist. This is because the statements also contain terms which are binary discriminations. X and non-X. But if one said 'Britain is now capitalist' and 'Britain is now socialist' one would not be stating a contradiction though one might, at most, be stating contraries: for while the statements, at their face value, may not both be true, both might be false; Britain might be something else.

Now, in fact, the way such statements are made does not exactly reflect what they usually mean. In short, they should not be taken at their face value. Those who say 'Britain is both capitalist and non-capitalist' are wrong in thinking that it is really a contradiction: hence, if they are conventional logicians they are wrong in thinking the statement impermissible as a description of Britain today; and, if they are dialecticians they are wrong in thinking it permissible to speak of the 'real contradictions' of British society. For when we use statements like this we really mean: Britain is in *some* respects capitalist and in *other* respects non-capitalist. To state that A is both X and non-X is to state a contradiction: to state that A, with respect to property p (i) is X but with respect to property p (ii) is non-X is not contradictory. To say that a man is both sick and non-sick is contradictory: to say that he is sick in his head but not sick in his foot is not.

Binary discriminations are simply discriminations between things or attributes which are either X or non-X. 'Living' and 'non-living' or 'life' and 'non-life' are binary discriminations. They become binary oppositions when the category of non-X can be stated as a positive one, such as Y. If all non-X is Y, then the classes X and Y exhaust that particular universe. We think of 'life' and 'death' as binary opposites· but they are not. For something can be neither an instance of life nor an instance of death· e.g a stone; only once-living things can be dead. However, within the specified context of animate beings which have been born, it might be permissible to state that the terms 'living' and 'dead', 'life' and 'death' exhaust the whole universe of attributes. a chap out of the womb is either alive or he is dead, but not both, and at least one (And never mind for the moment whether he is alive if his heart stops beating and then starts again: he is alive if it starts again and not if it doesn't).

While there are difficulties in saying that 'white' and 'black' are binary oppositions, since the word 'black' does not equal 'non-white' (except in the minds of some racists), there is no difficulty in saying that 'black' and 'white' are opposites When we say two things are opposites we mean that they are at extreme points on a continuum. If X is at one end of a continuum then everything else on it is non-X; but that sub-class of all things which are non-X, which are Y, and which stand at the other end of the continuum, is the opposite of X. Binary discriminations can be binary oppositions only when there are no more than two points on a continuum. For all practical purposes, among humans all non-males are females and all non-females are males. here we have a case where a binary discrimination is also a binary opposition.

To refer to cultural, social or even physical contradictions brings trouble and is asking for it. One can only resolve a contradiction by believing one thing or another, but not both of them, though at least one of them. It may not be easy to make up one's mind between them but that is a different matter.

Of course, words are not necessarily sacred;

and even where they are, they are not necessarily the sacred property of logicians. So, if people want to talk about cultural and social 'contradictions' they are free to do so: after all, we talk about political opposition without equating it with semantic opposition. But if we take note of what people say about such matters we should bear in mind that we are free to interpret what they mean as less than they think they mean.

One should also bear in mind that other terms, like 'resolve', get over-loaded with meanings. No one can resolve the conflict between life and death (using the term 'resolve' in its non-logical sense) any more than he can resolve the binary opposition between them. Perhaps, however, people may get the feeling that they have, by a mental sleight of hand and aided by cultural devices, 'dealt' with the problem of life and death. One wonders how long the feeling actually lasts.

Percy S Cohen
London School of Economics & Political Science

The state of ethnomusicology in Great Britain

SIR,

May I comment on an error of fact in the review (*Man* (N.S.) 9, 4) of Frank Harrison's *Time, place & music. an anthology of ethnomusicological observation c 1550–1800*, and, arising from it, on the situation of what is generally called ethnomusicology as seen in *Man* and academically in Great Britain? Though the contents of that anthology were said by the reviewer to be not very useful to ethnomusicologists, it was in fact partly due to the interest of professional anthropologists and ethnomusicologists that these written comments from a variety of Europeans, variously motivated and variously equipped, were collected. Anthropologists, ethnomusicologists, historians and literary scholars in Europe and north and south America have found the collection of immediate practical use and have so informed the author

One comment in the preface to *Time, place & music* is perhaps particularly relevant.

> The kinds of bias that may be exercised, particularly in current academic-centred observations of music-practices, are no less in need of a cool, hard look than those of the writers of the extracts printed here. Bias may be involved not only through social, intellectual and technical presuppositions . . . but also in project stereotypes geared to current academic routines and career-structures and in the selecting and ordering of the information acquired. Such bias, where it exists, is quickly apparent if present-day academic motivations are frankly compared with those of earlier, non-academic observers.

If a cool, hard look is directed towards Great Britain in respect of 'the social and historical circumstances in which human beings, including scholars, devise meaningful symbolic structures' (S. Blum, 'Towards a social history of musicological technique', *Ethnomusicology* 19, 207–31) a curious situation may be seen where ethnomusicology and anthropology are concerned. For example, Mr Montagu has in the past stated that he is an ethno-organologist and that 'Sachs is the father of us all'. The work of Curt Sachs (1881–1959) has been described by James Blades, the distinguished player and historian of percussion instruments, as armchair work. Whether concerned with the history and classification of musical instruments (with which Sachs was involved in a curatorial capacity until forced to leave Germany) or with theories about music and dance in many societies of his own time and earlier, it was the result of assembling and organising, according to his own ideas, materials provided largely by other people In particular, his acquaintance with non-European music and dance, apart from two visits to Egypt where he advised on methods of reforming music education, was at second hand, through writings, notations, photographs and sound recordings. This is not to belittle his great achievements in some fields, merely to set his work in its time and place

The ordering and classification of sound-making instruments is as valid an academic exercise as any other classifying operation, and given the raw materials in sufficient quantity, it is indeed possible from an armchair, wherever situated. It is, however, no longer the case that pontifical comment on *all* aspects of the musical concepts, behaviour and tangible products of *all* peoples is acceptable from a single person who has virtually no experience outside the kind of society in which he/she was raised.

In what are considered to be anthropological subjects, the work of many specialists appears in *Man*, in both articles and reviews. As far as 'ethnomusicological' subjects are concerned, articles are few, and books and discs, of whatever nature and from whatever source, are almost all dealt with by one reviewer—an ethno-organologist who has declared 'Our business is music, sounds and noises which we describe by means of dots on paper. The why's, the wherefore's, the how's, when's and where's are important, of course, but they are only peripheral to the music itself' (Galpin Soc. J. 27, 155). Is it therefore to be supposed that there are

no ethnomusicological specialists in Great Britain? Or that there are and that British anthropologists are unaware of them? Or that where musical matters are concerned, these are considered not to need specialism of the kind demanded in other aspects of anthropological activity? Seen from outside, the situation of ethnomusicological study in Great Britain seems, with a few exceptions, to resemble that in Sachs's heyday in Germany in the 1920's and early 1930's. Organology, with or without prefix, has become a part of the academic curriculum in a number of institutions; ethnomusicology in the anthropological sense has not, to any extent. Given the conservative nature of British academic structure, it is understandable that an organological approach has been almost the only means by which ethnomusicological (or whatever one cares to call it) material could be inserted into several Departments and Faculties of Music. Will this, by the British process of instant tradition, set the pattern in other departments, whether of music, anthropology, language, medicine or regional studies, where such matters may eventually be studied?

A clear identification of some ways in which the study of musics may be approached has been propounded by John Blacking; it is doubtless followed into practice by himself in Northern Ireland and certainly is by some anthropologists and ethnomusicologists elsewhere. Will it ever be noticed in Great Britain? Or will the image in this, as in so many aspects of British commercial, cultural and academic activity as seen from abroad, be no longer John Bull but Toad of Toad Hall?

Joan Harrison

Amsterdam

The causes and motives of the Jihād in northern Nigeria

SIR,

Usman dan Fodio (about 1750–1817) was the leader of the holy war which started in 1804 against the rulers of the Ḥauṣā states of what is now northern Nigeria. Shaykh Usman grew up in Gobir and was sent to the town of Agades for further education Muḥammad Fodio, the father of Usman dan Fodio, was called Fodio because both his parents had learned the Qur'ān by heart. Islam in fact spread amongst the Fulānīs along the coast of Guinea as early as 1076 A.D. (see 'The spread of Islam in Africa'—Arabic Text, Ḥ. I. Ḥasan, al-Nahḍa Bookshop, Cairo, 1964, p. 22).

Usman dan Fodio became a learned jurist whose teachings attracted various sectors of society. The Sarkin of Gobir made him tutor to his sons, but in due course this ruler, who was a pagan, feared the growing influence of Islam over his people and he started to persecute Muslims, in particular the Fulānī tribes to which dan Fodio belonged. In 1801 Sarkin Gobir Bāu Nafata decreed that no-one should preach except Shehu Usman dan Fodio, and that no-one could be a Muslim unless his father had been a Muslim also. In 1802 the Shehu quarrelled with the Sarkin Gobir over the maltreatment of a Muslim subject and in 1804 holy war was declared against the successor of Nafata, his son Yunif, who was even more tyrannical.

It was not proposed here to give a detailed descriptive account of the Jihād, but to concentrate on analysing its causes in the light of the latest research contributions at the seminar in Sokoto in January 1975.

Many writers, for example Barth, Temple, Lugard, M. G Smith, Johnston, Muffett, have presented the Jihād in what may be described as racial terms. Thus the Jihād of dan Fodio was described as Fulānī and the Muslim empire which emerged after the success of the Jihād, the Sokoto Caliphate, is often referred to as the Fulānī empire.

In his second address at Sokoto on 21st March 1903 Frederick Lugard tried to justify the British conquest of northern Nigeria as follows:

> The Fulānī in old times under dan Fodio conquered this country. They took the right to rule over it, to levy taxes, to depose kings and to create kings. They in turn have by defeat lost their rule, which has come into the hands of the British. All these things which I have said the Fulānī by conquest took the right to do now pass to the British.

H. A. S. Johnston in his book *The Fulani Empire of Sokoto* (London, 1967, pp. 256–7) concedes, however, that the empire brought about a unity between Fulānī art of government, scholarship and religion and Ḥauṣā agriculture, industry and trade.

Murray Last, although conceding that the authority of the caliphal government was derived from respect for the law of Islam and not from tribal allegiance, nevertheless compares the Jihād to the British colonial conquest. He says in 'Aspects of administration and dissent in Hausaland 1800–1968' (*Africa*, 40, 345):

> The northern states of Nigeria have in the past two hundred years experienced two great reform movements. Both were accompanied by conquest, by a distinct minority and both sought to impose an external universal culture with its set of written laws and distinct administration. The ideals and their application by the two groups have been strikingly similar.

However, some of the factors mentioned by Murray Last in *The Sokoto Caliphate* (London, 1967) as factors that favoured the Jihād are (a) oppression which affected both peasant and nomad in the sphere of justice in which Islam is particularly precise, for example rights of women and taxation; (b) the fact that many of the town Mālams were in fact Ḥauṣā (and not Fulānī); (c) that psychologically the Fulānī Mālam was well prepared for the Jihād. His only ideal was Islam: preaching and teaching was his career, and he was under pressure to put his teachings into practice. The Islamic community became wide—Tuāreg, Ḥauṣā and Fulānī; and (d) that the ideal of Islam appealed to all. Knowledge of Islam was widespread enough, and the Hauṣā states corrupt enough, to make Islamic reform welcome.

Perhaps the clearest paper on the supra-ethnic ethos of the Jihād, presented at the Sokoto seminar in January 1975, is Dr Yusufu Bala Usman's 'The transformation of political communities'. In it he makes it clear that the Jihād was an Islamic reformist movement which produced unity and order. He rejects the view that the Jihād was Fulānī.

> What seems to really indicate that the Fulānī are not 'largely biologically self-perpetuating' even if it is shown that some of the clans had a biological ancestor and genetical cohesion, is the way in which new Fulānī clans emerged from ancestors who are well known not to be Fulānī. The Dallazawa and Rumawa of Katsina provide examples of this. The Dallazawa, from whom the emirs of Katsina from the jihād to the British conquest came, are descendants of an Arab from Wadai, Muhammadu Goshi, who came to live at Makau, near Giremawa, on the plains east of the Birnin Katsina ... The Rumawa in Katsina, also regarded as a Fulānī group, are made up of a number of lineages ... Such processes as these, which produced new Fulānī clans in Katsina in the nineteenth century from non-Fulānī ancestors show that the existence of cohesive lineages and extended kinship groups among the Fulānī does not necessarily mean that the Fulānī are 'largely biologically self-perpetuating' ... Externally commerce, migration and the spread of Islam was integrating the kingdom of Katsina into the Islamic community of the *bilād al Sudan*. These processes of integration involved considerable descent and occupational heterogeneity within the kingdom. One of the major consequences of this was that Islam came to the major bond of social cohesion and the source of the basic code of social conduct, especially in the towns and the densely settled rural areas with a high level of economic activity and considerable migration.

In the nature of things human beings are not perfect and an element of Fulānī-ism may have been unavoidable, but this does not mean that the Jihād was not basically and fundamentally an Islamic reformist movement. After all, Usman dan Fodio was a jurist and not a warrior and indeed he delegated a great deal of the leadership of the fighting campaign to his brother, Abdullahi, and his son, Bello. As Thomas Hodgkin has written in 'Nigeria Magazine' (Independence Issue, 1960, pp. 129–36). 'His ideas and achievements have to be understood in the context of the succession of reforming movements which profoundly affected the Muslim world during the latter part of the eighteenth and nineteenth centuries—beginning with Muḥammad Ibn 'Abd al-Wahhāb in Saudi Arabia'.

Further, as Ādam 'Abd Allāh al-Alūrī wrote in *Islam in Nigeria* (Arabic Text, 1971) at page 25, the kings of Songhai are originally from Yemen, and at page 31, the ancestor of the seven Hauṣā kings was a Muslim Arab called Abū Zayd of Baghdad, who killed late in the 8th century A D. the Sarkin of Daura (the snake which dominated the water-well in Daura) and then married the Queen of Daura (and is today known as Bayajida). Their grandchildren became the seven Hauṣā kings. Thus the history of migration in this part of the world makes it impossible to distinguish the categories of ethnicity This militates further against the writings which claim that the tribal allegiance provides the basis for political transformation in northern Nigeria.

Isam Ghanem

Ahmadu Bello University

Alliance or descent: some Amazonian contrasts

SIR,

I would like to make a brief response to the letter by Taylor and Ramos (*Man* (N.S.) **10**, 128–30), written in reaction to my own letter (*Man* (N.S.) **9**, 305–6).

First of all, let me say that my statements about the Yanomama were not based solely or even primarily on my own field research, which, as Taylor and Ramos point out, was relatively brief. They are rather based on my understanding of the general literature on the Yanomama, to which Taylor and Ramos have both made significant contributions. I would like to apologise for not having made explicit reference to their work, especially since it was Ramos's thesis (Ramos 1972) that was uppermost in my mind when I mentioned the considerable ideological significance of agnation

among some Yanomama. My failure to cite it was perhaps due to the fact that it was not generally available, but this is no excuse and I was indeed remiss.

Taylor and Ramos are also correct in noting that I should have given more attention to the possible significance of regional variation among different Yanomama subgroups.

I believe, however, that the main point lies elesewhere, namely, in the problems involved in the use of terms like 'patriliny', 'descent', 'lineage' and 'local descent group' in the South American context. My differences with others who have interested themselves in the Yanomama do not in any way involve questioning the reliability or quality of their field data, on which much of my own work has been based and which I regard with admiration, the issue is the way in which certain traditional social anthropological concepts have been used in analysis

Finally, a comment on the tone of Taylor and Ramos's letter: I deeply regret that they found it necessary to express their disagreements and objections in the sort of acrimonious and *ad feminam* manner that is, unfortunately, all too familiar in anthropological debates.

Bryn Mawr College Judith Shapiro

Sir,

I am writing to express my opinion of Dr Shapiro's letter 'Alliance or descent some Amazonian contrasts' (1974)

Shapiro states in her thesis (1972) that what I said about lineages in my article 'Remarques sur le vocabulaire de parenté yanõmami' (1971*a*) was not entirely convincing. Upon reading these comments I wonder if she had clearly read the title of that study or had understood that my intention was to write only on kinship terms, and briefly at that, since it was only a question of 'remarques'. On the other hand, she, like Taylor and Ramos, ignores another publication (1971*b*) in which, without exclusively focusing my analysis on the problem, I expand on the point bringing forward arguments that neither Shapiro, Taylor or Ramos have ever advanced.

Let us turn to some fundamental problems in Shapiro's letter. She writes: 'Yanomamo preferences for village endogamous unions . . . have to do, in the case of a man, with the advantage of a more relaxed in-law relationship and, in the case of women, with the even greater advantage of being able to remain near her relatives after marriage.' This is generally and approximately true, but it is a dreadful simplification of a rather more complex reality. In order to remain near their parents, young Yanomami prefer to marry within the group in which they reside, but they have no reticence about going to live in a local group in which an important faction of their sibling group, or their own lineage, exists. I made that point clearly in my 1971*b* article.

It is not true that 'The smallest Yanomamo villages are generally composed of two blocs of kinsmen involved in affinal exchange'. I know that she cites Dr Chagnon, but I also know that Chagnon's views are much more discriminating now. Here again it is a case of a generalisation which does not correspond exactly with the facts. If Shapiro had read my 1971*b* publication, she would have been more careful I conducted my research among the central Yanomami—where I have spent six years—and where I have studied some thirty communities. Compared with the '*shamathari*' of the south, the local groups are small, fifty-four inhabitants on average. And indeed there is no absolute rule· some places are composed of only a single important group of kinsmen, others of three or four. In addition, the largest number of marriage exchanges is made with groups residing outside the locality.

Finally, before deciding whether the ties between brothers-in-law or between siblings is most important, we have to examine concrete situations in which these ties come into conflict or contradiction. Dr Shapiro will then see that all is not as clear as she perceives it. For example, postmarital residence has an influence on these ties. Take, for instance, the service that every son-in-law owes his father-in-law, what happens in the case where the woman is taken from outside the community? Is the residence uxorilocal (with the brothers-in-law) or virilocal (with the sibling)? What become of the ties in the event of conflict or war? Who helps a man in the case of violence? It can readily be seen that these relationships are complex and that it is necessary to perceive the nuances of the situation.

Jacques Lizot

Chagnon, N. A 1968. *Yanomamö. the fierce people* New York Holt, Rinehart & Winston.

——— 1974. *Studying the Yanomamö*. New York: Holt, Rinehart & Winston.

Lizot, J. 1971*a*. Remarques sur le vocabulaire de parenté Yanomami. *Homme* **11**, 25–38.

——— 1971*b*. Société ou économie? quelques thèmes à propos d'une communauté d'Amérindiens. *J. Soc. Am.* **60**, 136–75.

Shapiro, J. 1972. Sex roles and social structure among the Yanomama Indians in North Brazil. Thesis, Columbia University.

——— 1974. Alliance or descent: some Amazonian contrasts. *Man* (N.S) **9**, 305–6.

Correction

SIR,

I realise that the majority of British anthropologists do not wish to be concerned with such sublunary matters as art, languages, or mathematics. However, I trust you will not find me pedantic if I point out that in my letter replying to Mr William Fagg (*Man* (N.S.) 10, 478–9) the mathematical equation as printed is that of the spiral of Archimedes or equable spiral, and makes a nonsense of what I was saying. The equation for the logarithmic spiral, as carefully shown in my typescript, is of course:

$$r = a \text{ to power of } \theta$$

Furthermore, by printing 'south-west' where I had written 'south-east' you have consigned the Kalabari Ijo to the bottom of the deep blue sea.

J. B. Donne

London

Physical Anthropology

HOWELLS, W. W. *Cranial variation in man: a study by multivariate analysis of patterns of difference among recent human populations* (Pap. Peabody Mus. 67). xvi, 259 pp., illus., tables, bibliogr. Cambridge, Mass.: Harvard University, 1973

Professor Howells has written a most important book. This is a landmark in twentieth century palaeoanthropological inquiry, meticulous as regards osteometrical detail, rigorous as regards application of multivariate analysis to classification of skulls Today physical anthropologists are becoming ever more self-important, obfuscating their endeavours with jargon. Not so Professor Howells. Since the death of Sir Wilfred Le Gros Clark, Howells is the sole bastion of the writing of clear English in physical anthropology As two other books of his published in 1973 show, he is always clear and to the point, with disarming modesty which belies the difficulties and tedium of cranial multivariate analysis.

Howells writes, 'For a century and a quarter the work of craniometry has supposedly been the description of, and differentiation of, *populations* in terms of *measurements*' He defines his aims as, '1 the differentiation of populations in terms of multivariate variables which are generated (objectively) by the population differences themselves, and 2 the description and evaluation of *measurements* in terms of *populations*—that is, the relative importance of aspects of shape in differentiation of human ethnic types.' To those ends he reduces 70 craniometrical measurements on 20 different, sexed groups, totalling almost two thousand skulls, to 16 functions capable of describing each group. These multiple discriminant functions have the important property of orthogonality; each gives zero correlation with the rest. The 20 populations are then investigated as regards intergroup differences of the functions using clustering programmes.

Both male and female series show two major world groups: one comprehends black Africans, Andamanese, Melanesians and Australian Aboriginals, the other the peoples of the Eurasian land-mass, the Americas and Polynesia. Howells carefully points out how this differs from dendrograms of world population variation derived from blood group studies, and poses the question of which may be the better indicator of evolutionary divergence. Palaeoanthropologists will find the lengthy section on interpretation of the discriminant functions repays scrutiny. There follows a shorter section on principal factor analysis. Multiple factorial analysis allows craniometrical measurement of each population separately to be reduced to orthogonal factors. Observation of those measurements which load heavily on particular factors gives an idea of osteogenetical variation when the factors of one group are inspected and compared with those of another. Thus two groups which might seem similar as regards shape on the basis of multiple discriminant functions, could conceivably vary as regards precisely which osteogenetical processes contributed to the shape (and *vice versa, mutatis mutandis*), hence highlighting genetical divergence apparent similarity of shape notwithstanding. (Bones have several ossification centres, and there is no *a priori* reason why each centre should not be under separate genetical control.)

Howells, however, is more concerned to view factorial scores in terms of shape characteristics and to that end treats only of a pooled (male) series. That, I feel, is unfortunate. My own as yet unpublished principal component factorial analyses of several skull groups from one small region, suggests disturbing possibilities of far from clear correspondence between inter-group craniometrical measurements (checked by F and t statistics) and loadings of those measurements on particular factors when groups are compared by factorial inspection. Such effects might produce intra-group correlations between discriminant functions believed to be orthogonal when larger series are used as the basis for their definition. That probably does not matter much when a world overview is attempted, as here. It might matter very much if the technique were applied to one continent or one country. It is indeed possible to make good sense of Howells's worldwide study, for instance by postulating a Rhodesian Man/Solo Man ancestry for the sub-Saharan African–SW Pacific peoples, diverging early in the Upper Pleistocene from a Eurasian ancestry which spilled over into the Americas and Polynesia. Howells has written elsewhere of such matters and keeps speculations out of this book which is presented solely in terms of

statistically analytical criteria. It is an important contribution towards attaining objectivity in palaeoanthropology.

M. J. WALKER

University of Sydney

MOLNAR, STEPHEN. *Races, types and ethnic groups: the problem of human variation.* xiv, 193 pp., illus., tables. Englewood Cliffs, N.J.: Prentice-Hall, 1975

Do not be put off by the vagueness of the preface. 'This is a book about human biological diversity' it states, 'an attempt to take a balanced approach to the study of human variation', but with no indication of the object, and you have to read the book right through before discovering what it has been doing. Do not be put off by the lack of organisation, so important in a simple book: the fourth paragraph asks 'why is man such an extremely polymorphic, polytypic species?', but you have to read a further twenty-seven pages before finding the definition of polymorphism, and there is no definition at all of polytypic; much of chapter 1 is repeated in chapter 4. Nor should you be too distracted by the errors of fact which abound: it is just not true that 'anthropometry reached its maximum application during the last quarter of the 19th century'; the Hardy-Weinberg law (page 43) is completely erroneous ('p+q=1, simply stated it means that if one allele increases the other decreases'!!)—these respected savants would turn in their graves; Haemoglobin E does not reach its highest frequency in India (page 79). You should also close your eyes to some of the figures: figure 2.2 fails to show the important initial pairing procedure in meiosis, figure 2.6 is a nonsense diagram, figure 3.5 assigns the uterus to the male side of the diagram, the curves are mislabelled on page 160, and the error of scale in figure 7.1 is conspicuous. Nor perhaps should you notice the errors in spelling (e.g. 'disbursed', page 38, 'sabaecous' for sebaceous, page 65).

For once you shut your eyes to such criticisms, you can see that the book bristles with modern ideas. It reviews some of the biological differences that are to be found among human groups, shows how these are related to environmental and genetic differences, and how they themselves in turn have affected our social development and attitudes. The whole leads to discussion of the future of man in the concluding chapter, probably the best chapter in the book. The book attempts to take a modern view of mankind evolving, asking how he has come to be what he is and where he is going. It might succeed were it not for the types of defect criticised above. Unfortunately these prevent its recommendation to readers at introductory levels, for whom it was obviously intended.

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WALLACE, B. *Génétique des populations;* adapté de l'anglais par G. Lucotte. x, 226 pp., illus., tables, bibliogr. Paris. Masson, 1974

DAJOZ, ROGER. *Dynamique des populations* (Coll. Écol. 6). viii, 301 pp., illus., tables bibliogr. Paris: Masson, 1974. F98

Neither of these two volumes is specifically devoted to man, yet their relevance for human studies is considerable. The first is a French translation and adaptation of those parts of Bruce Wallace's *Topics in population genetics* (1968) that are regarded by the translator as most characteristic of his work. Almost twelve chapters out of the original twenty-five are included. These concern the detection of variability, how it is maintained through selection, especially via heterozygote advantage and hybrid vigour, the concept of genetic load, adaptive surfaces, co-adaptation, non-repeatable changes in populations, and the principal properties of the genetic structure of populations. Much of the work is in general genetic terms, most of the examples as one would expect concern Drosophila. As a text to introduce French readers to anglophone genetic concepts, it is to be welcomed, for just as English genetics received considerable impetus from the introduction of the independent work of the French genetic theoreticians, particularly Malecot, so many French readers still today only know the older work of the English-speaking genetic theoreticians. Most readers in Britain will undoubtedly prefer Wallace's original text.

But the present work is of interest in what is left out. Discussions of gene dispersal, random/nonrandom mating, migration, chance and inbreeding, mutation, the ecological aspects of population genetics—all these are omitted, yet amongst these lie some of Wallace's most valuable contributions These omissions may be because the topics are regarded as of less relevance to the present climate of French work. Perhaps they are already well covered in established French texts, though this is doubtful. But whatever the reason, their omission detracts considerably from the book, for Wallace's original was particularly well-coordinated and structured, with the thread of argument running through the chapters leading in the penultimate chapter on ecological aspects to the visualisation of population genetics in relation to the population's environment. The whole is like a ladder in which half the rungs have been omitted, including the topmost.

Dajoz's book concerns demography—not so far removed from the former as might at first appear, for Wallace's discussion led up to the demonstration of the relationship between gene frequencies, fitness and population size. But Dajoz makes bare mention of genetics. He is concerned with other biological applications of demography. The book starts with first principles and definition of terms, pointing out that the dynamics of population examines quantitatively the numerical variations of populations and the factors which are responsible for them.

Chapter 1 introduces the quantitative characteristics of populations; their spatial structure, density, Lexis diagrams for longitudinal analysis, tables of mortality and survival curves, age pyramids, sex ratio and intrinsic rate of natural increase. In Chapter 2 these are compared in different groups of living creatures, including briefly man. Chapter 3 considers variations in number in natural populations, introducing the notions of natural equilibrium and relative stability of populations, seasonal variation, and oscillations and fluctuations, together with the effects of invasion and colonisation.

The next two chapters concern population growth, starting with interactions between individuals of the same species, the effects of social groupings as well as of number, and of intraspecific competition, supported with some excellent experimental illustrations. This leads to a discussion of the growth of two populations of the same species, and of an isolated population, and this in turn to interspecific competition, the notion of the ecological niche, the consequences of interspecific competition, again supported by experimental evidence, and mathematical models. Chapter 6 concerns predator/prey interactions. Chapters 7 and 8 discuss the mechanisms of population regulation. First comes a review of theories and models, from Lotka and Volterra through to that of Pimentel—a thorough discussion of each leading to the concepts of cybernetics and the application of the notion of feedback mechanisms to populations. The principal conceptions relating to regulation of population, one incorporating predominantly density-dependent factors, and the other density-independent factors, clearly emerge. But to balance, there follows an examination of the facts, drawn from a great variety of animal species which have been intensively studied, in which the effects of density-independent and density-dependent factors are examined in considerable detail. Chapter 9 concerns a still more intensive study of particular natural populations—lemmings, Australian and African insects, and rabbits. The final chapter concerns population fluctuations, and their prediction by the aid of models.

This is a solid and most useful book. Human examples are few, but it is revealing to see how rewarding has been the application of concepts and methods of human demography to other living populations, and in turn they illuminate human demographic problems. The book is well written, well produced, and the figures, illustrations and equations clear. There is no summary, but each chapter is divided into sections, and it is simple to find one's way through. The book does not consist of argument, but rather of exposition of the author's conception, that the theoretical and practical importance of studies of population dynamics justifies the efforts made to explain the variations in number of different animal and plant species. The many schools and theories attaching to this science, despite the abundance of exact data that have now accumulated, demonstrate its complexity. In natural populations very many factors act simultaneously or successively, and interact—climate, food supply, diseases, predators and parasites, other members of the same species, genetic and physiological factors, the structure and diversity of the ecosystem and many others. How much more complex then is the human situation, with the intervention of culture! And what responsibility man has to understand and hence control the vastly ramifying consequences of his interference with any one variable!

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Archaeology

HIGGS, E. S. (ed.). *Palaeoeconomy; . . . papers in economic prehistory*; by members and associates of the British Academy Major Research Project in the Early History of Agriculture. x, 244 pp., illus., maps, tables, bibliogr. Cambridge: Univ. Press, 1975. £8·50

This book has the traditional faults of a collection of papers The articles have little obvious connexion and, indeed, two of them defy the subtitle by having nothing to do with agriculture. Having made these *caveats* I do feel that the volume is a useful contribution.

Higgs and Jarman set the scene explaining what is meant by palaeoeconomy. In the process they briefly discuss and dismiss certain more traditional approaches to archaeology, including artefactual analysis, palaeoethnology and the systems approach. The editors even hint that the 'apparently revolutionary "New Archaeology"' may not be all that new, revolutionary or effective! *Satis verborum*!

Palaeoeconomy rests on the fact that although archaeology concerns itself with

culture, it is the man-land interaction—the plants and animals that form man's subsistence—that dominate the human economic world. These plants and animals, because of their biological conservatism, are more predictive than man himself. Thus, analogy in archaeology can be tied to more static biological rather than cultural systems.

The first substantive article, that of Wilkinson, very cogently demonstrates this position. Using both ethnological and archaeological data, he shows the role that musk oxen played in the very recent past and must have played in the late Pleistocene. Hunting of the musk ox is circumscribed by the biological peculiarities of the animals, and Wilkinson concludes that this animal could never have been more than a secondary economic resource.

D. A. Sturdy analyses reindeer debris from kills in Pleistocene Europe and combines this with a sophisticated analysis of reindeer behaviour, to hypothesise on the actual mechanism of reindeer taking and its implication for human and reindeer seasonality and for human economic and social organisation.

The paper by Dennell and Webley on prehistoric land use and settlement in Bulgaria involves a detailed analysis of soil types, periods and deposition, and estimates of both human exploitative strategies and population densities over a period of many centuries.

Somewhat the same techniques are used by Barker in articles on the prehistoric territories of central Italy and by Jarman and Webley on the region of northern Apulia in southern Italy. In both cases, changes of paleo-landscapes and flora and fauna are shown to be crucial in any understanding of the economic adaptations of human populations.

I recommend this book, not as just another economic study of post-Pleistocene Europe but because of the methodologies employed and the hypotheses tested. It should be in the library of any archaeologist who is interested in the larger problems of human process and the man/land/time equation.

Palaeoeconomy contains an appendix discussing field techniques as applied to site catchment analysis, and a second one (by Vita-Finzi) on grain size distribution in certain alluvial deposits. The book also has a name index, an adequate subject index, and a very short glossary.

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COLLINS, DESMOND (ed.). *The origins of Europe: four new studies in archaeology and history;* with contributions by Desmond Collins, Ruth Whitehouse, Martin Henig, David Whitehouse. 347 pp., illus. London: Allen & Unwin, 1975

An impressive survey of archaeological work in prehistory and history from the origins of man to Europe in the twelfth century A.D. The role of names such as Breuil, Garrod, Childe, Collingwood, Richmond and Pirenne in the well-assembled bibliography illustrates the scope of the work. There are four sections, all written by archaeologists, two concerning prehistory, 1, 'Early man' (by Collins) and 2, 'Later prehistory' (by R. Whitehouse), and two concerning historical times, 3, 'Archaeology and the Classical mind' (Henig) and 4, 'Medieval Europe from the fifth to the twelfth century' (D. Whitehouse).

The aim is less to provide a text-book for advanced students than to discuss for the intelligent and careful reader the forces which shaped the development of Europe. The book fulfils admirably that bold purpose, and many an advanced student will be grateful for the amply provided diagrams, sketches, distribution maps and tables of types and dates It should be a chastening experience for the reader who regards 'archaeology' as a subject or discipline in its own right and not as a body of techniques of which some may be applied to defined areas or types of study. So too should it be for the Classical archaeologist and historian to read that in the wider context of European development the world of Minos and Mycenae made no lasting contribution beyond what is incorporated in the Homeric poems (p. 193).

The work is broad in scope but not popular in any pejorative sense, for it invites concentrated attention. Careful editorial policy ensures that not only the role and limitations, but also the methods, of the archaeologist are carefully defined. Careful attention is paid in part 1 to the construction of models of human evolution (p. 41 and p. 67) and in part 2 to stimulus and diffusion as factors in development. In part 3 equal and diffuse treatment of the Classical World generally is avoided by particular concentration on Roman Britain. Difficult questions are faced even if the scope prevents full discussion of, for example, why the last interglacial period left so little evidence compared with the last glacial or penultimate interglacial period (p. 65).

Evolution from Hominid to Neanderthal naturally takes us beyond Europe to Olduvai, north Africa and Asia, but with Cromagnon Man and the Hunters (part 1) we are prepared for the origins of farming and the urban revolution of west Asia and Europe (part 2) where food production, the great achievement of mankind, transformed the range of cultural activities, housing, trade, pottery, metalwork, religion etc. Urbanism and metallurgy neatly prepare the way for Henig's discussion of Classical archaeology (part 3) in which he demonstrates the profound difference of

approach between the prehistorian and the Classical archaeologist, which arises from their quite different sources of information (p. 236). Written evidence, for example, is available for the latter and for him pottery is less significant whilst the major arts, such as painting and sculpture, loom larger. Turning to medieval Europe (part 4) the Classical archaeologist sees the wheel turn full circle to show the coincidence of the prehistoric and medieval archaeologists' view of the economic basis of towns (p. 311, cf. p. 169) in an era which saw the end of the Dark Ages (not a relapse into darkness and savagery) and the emergence of the Church and feudal society. We are reminded of the growing importance of archaeology in medieval studies, for example, in modifying Pirenne's thesis that Islam's severance of commercial arteries created a vacuum in western Europe, and in the excavation of towns such as Winchester and York.

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Social Anthropology

WALLWORK, ERNEST. *Durkheim: morality and milieu.* xii, 224 pp., bibliogr. Cambridge, Mass: Harvard Univ. Press; London: Oxford Univ. Press, 1972 £5

Durkheim's intellectual progress can be described as moving from philosophy to sociology. We are naturally used to projecting his ultimate concern back on to his work and looking at him as one of the founders of empirically based social science *Durkheim* by Wallwork interestingly chooses to take the opposite perspective and sees his work principally as a philosophical contribution drawing our attention to the continuing major place that philosophical and especially ethical concerns have in all his books. Unlike other writers who have tended to ignore Durkheim's concern with morality, often with something approaching distaste, Wallwork argues that Durkheim's contribution to ethics is far more subtle and valuable than has often been assumed and that it heralds and clears the way for some of the most promising modern developments in this field. This unusual focus means that Wallwork gives much greater prominence to works of Durkheim which have often been ignored; such as a series of early essays published between 1885 and 1893 as well as a continuing stream of similar writings throughout his life and his very important two volume history of education in France: *L'evolution pédagogique en France.*

Wallwork begins his study by placing Durkheim historically in the philosophical milieu of which he was a part which in this case was dominated by the opposition of Kantian idealism and French versions of British utilitarianism. Durkheim's ethical theory is seen here as an attempt to reconcile what is best in these two views: the genuine account of moral experience of the idealist and the desire for a scientific explanation of the naturalists while conversely rejecting the mysticism of the former and the reductionism of the latter. Wallwork characterises Durkheim's position as neonaturalism in that it sees morality as super-natural to man but the product of society which is itself a natural phenomenon, although Wallwork is careful to excuse Durkheim of the mistake of 'social realism'.

Wallwork examines critically the unfolding of Durkheim's theory of ethics in the major works such as *Suicide, The division of labour,* though strangely paying only scant attention to *The elementary forms of the religious life.* He also examines in detail Durkheim's theories of the development of moral ideas in the child. Inevitably Durkheim's conceptualisation of the individual becomes central to such a study and Wallwork interestingly suggests that Durkheim's conception of the individual as divided between the social and psychological owes its origin to Kant's distinction between the sensuous and the numenal self.

Finally Wallwork examines in more general terms the development of Durkheim's metaethical theory which he sees as progressing through three stages, the first owing much to Durkheim's mentor Wundt which sees justification for moral rules in their abstract usefulness for the purposes of society, the second being based on the distinction between the normal and the pathological and the third consisting of the ethical relativism implied by the *Elementary forms of the religious life.*

All these theories are vigorously criticised by Wallwork but he nevertheless stresses the originality and value of Durkheim's thought on ethics, placing him in the forefront of the moral philosophers of his time.

There can be no doubt that this book is a most valuable addition to the studies of Durkheim we already possess. It throws a new light which will help our understanding of this writer; nonetheless something perverse remains about the whole enterprise. Durkheim's clearest position on the ethical question he considered was that an answer to them could only be found outside the philosophical traditions of his time and that they had to be sought in the scientific study of the social organisation of the societies that exist and have existed. It could be said that by

looking at Durkheim as simply a philosopher Wallwork brushes aside this central point.

Another reason why Wallwork's book will be of value to anthropologists is of a different nature. The emphasis on the critical place of ethical theory in Durkheim's work serves to remind us how, apart from the rather spongy work of Kluckhohn, the study of morals has been poorly developed in our discipline. The reason seems to be that we have followed Durkheim in merging the study of morality with the study of the nature of conceptual categories. Unlike Durkheim however we seem to have concentrated of late almost exclusively on the former and forgotten the importance of the many other aspects of ethics which he considered.

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COPANS, JEAN. *Anthropologie et impérialisme* (Bibl. Anthrop.). 478 pp., bibliogrs. Paris: François Maspéro, 1975

This collection of essays and statements, the majority—but not all—of which have been translated from English and taken from American sources, is the attempt of one French radical working in the field of social anthropology to provide his colleagues and a wider audience with the materials for a critical self-analysis of the kind that is just now beginning to emerge in Britain and which has already generated an appreciable body of work in the USA. Readers of this journal are probably aware of many of the sources from which Copans has drawn in the first part of the collection—if not, they should be—but the sources for the second part will be less well known. The direct value of this collection to an English-speaking readership is its extremely full footnotes and references and the indications it provides of work on the relationship of anthropology to imperialism going on in different parts of the world.

Among those whose work appears in the collection are Gough, Wolf, Jorgensen, Africa Research Group, North American Congress on Latin America, Magubane, Okediji, Margarido, Wertheim, Buijtenhuijs, Varese, Stavenhagen, Batalla, A. G. Frank, Mintz and, of course, Copans. The virtual absence of British work—with the exception of reference to Asad's collection, *Anthropology and the colonial encounter*—is significant.

A similar work in English would be immensely valuable, but where might one find a publisher like Maspéro willing to take on a large collection on such a theme? Ithaca Press?

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BEE, ROBERT L. *Patterns and processes: an introduction to anthropological strategies for the study of socio-cultural change*. xii, 260 pp., bibliogr. New York: Free Press; London: Collier-Macmillan, 1974. £2

This is a text-book 'designed primarily for use in college under-graduate courses dealing with sociological change' (p. 2). Its value is that it brings together a wide range of ideas, showing the relevance of Marx, Engels, and Tylor, as well as Herbert Spencer and Morgan, to recent studies of social process by Geertz, Bailey, and Barth. It provides a student with a general map and reduces the danger that he may imagine that any contemporary discussion of social change he may light upon is the first that has taken place. As a map, *Patterns and processes* is reasonably accurate, though the description of Tylor as 'a British educator' (p. 49) is inept, and there are some surprising omissions, notably the writings of F. C. Barlett, whose work on 'psychological factors in the transmission of culture' (*Psychology and primitive culture*, Cambridge University Press, 1923) so long preceded that of Linton on 'Diffusion' and 'Integration' (*The study of man*, Appleton-Century, 1936) And who were the 'professional ethnographers' before 1870 (p 60)?

How *Patterns and processes* is assessed depends first, upon one's view of text books. A student in a South African university once distinguished what he called 'learning subjects' from 'reading and thinking subjects'. The danger of a text book is that it is liable to turn *any* subject into a 'learning subject' it provides a body of material easy to memorise and examine—much easier to examine than the analysis of a classic. But it is also less exciting. Text books, like lettuce, are liable to be 'very soporific' and to send students to sleep like the Flopsy Bunnies. Moreover, the style of this book is long-winded and unnecessarily self-regarding. 'My point is . . ' (pp. 83, 84, 130, 217); 'I want briefly to discuss . . .' (p. 159); 'I want to emphasize . . .' (p. 20); 'I am arguing that . . .' (pp. 63, 217) These faults are not merely irritating: they are likely to be infectious, because student essays quickly reflect characteristics of the authors they read. The use of made-up words not in a dictionary as comprehensive as the Oxford *New English Dictionary* is also questionable. Do *emic* and *etic* (pp. 17–18) mean more than *insider* and *outsider* viewpoints? Is the meaning of *historicism* (pp. 67–93) and *psychologistic* clear at all? Neither is in *N.E.D.*

Professor Bee is rightly prepared to accept the value of a variety of approaches to the study of social change. One not touched upon, perhaps because anthropologists as opposed to social historians have scarcely discussed it, is that the process of change is

constantly visible in the increase in the *proportion* of people in a community following one form of behaviour rather than another, whether it is sending children to school, engaging in wage labour, abandoning cultivation or stock farming, joining some association, challenging the authority of seniors, choosing their own spouses. In the village in which I began fieldwork in 1931, it is in the innumerable choices of individuals and families (not necessarily of leaders) in these matters that the process of change is visible. Diversity of behaviour, already apparent in 1931, had set the pattern for changes which have since followed: what is different is the incidence of one form of behaviour rather than another.

MONICA WILSON
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HANSON, F. ALLAN. *Meaning in culture* (Int. Libr. Anthrop) xi, 127 pp, bibliogr. London, Boston: Routledge & Kegan Paul, 1975, $10.85

This book is a concise, tightly argued essay by an American anthropologist dealing with the perennial problem of understanding other cultures. The approach which Hanson advocates is one which he labels 'institutional analysis,' a means of comprehending other societies through the 'implicational meanings' of their symbologies, social systems, etc. and their relations to each other within the context of the culture as a whole. This would seem to be a somewhat sophisticated functionalism grounded on the argument that human phenomena are intrinsically meaningful (that is, they have meaning in themselves, rather than with reference to an abstract, arbitrary standard), and that the approach to understanding other cultures is best carried out through institutional questions rather than those of individual psychology. From this point Hanson discusses the conjoint problems of evaluative and epistemological relativism and their relations to the problem of achieving an 'internal understanding' of another culture, arguing, quite rightly, that 'Relativism is an epistemological concept only; it concerns our *understanding* of other cultures but has no systematic implication for how we *judge* them' (p. 35)

The problems in attaining such an 'internal understanding' of another culture lie, in Hanson's view, in the broader issue of the philosophy of mind, and here he takes his stand on a view stemming from Gilbert Ryle and Ludwig Wittgenstein (as opposed to Descartes) that access to the 'mind' of another culture is not inherently impossible to achieve, but should rather be conceived as gaining access to that mind through the behaviour in which inner thoughts are manifested. In short, overt behaviour is not merely the result of thought but is, in fact, itself an activity of mind that we can readily appreciate and understand. 'Internal understanding,' therefore, '... is equivalent to "knowing how," because the critical test is always our ability to *use* the body of custom or system of thought under study. It is to know the correct moves, to know what natives would accept as appropriate responses in particular circumstances' (p. 65)—which is precisely how an anthropological fieldworker must come to grips with living in a foreign society.

Perhaps the most controversial chapter of the book is the fourth chapter, which deals with the question of the 'scientific' nature of anthropology. Hanson takes a different approach to this problem from most and argues that the main function of science is to discern order in various phenomena, an approach which centres on implicational rather than intentional meaning For the anthropologist the problem is to unearth and trace out the underlying metaphysical structure of the culture's basic presuppositions and to discover the ways in which they are linked with each other In discovering this order in social phenomena anthropological investigations must be counted as 'scientific.' This turns the discussion away from general attempts (such as those made by Radcliffe-Brown) to view anthropology in terms of natural sciences such as physics and chemistry and centres on the broader (and more fruitful) aspects of the nature of scientific discovery. The final chapter consists of brief remarks on the subject 'Some anthropological applications.'

Perhaps the most puzzling part of Hanson's book is the omission of Emile Durkheim, who is mentioned only once in a brief digression at the end of the first chapter Only *The elementary forms of the religious life* is listed in the bibliography. Certainly much of Hanson's argument closely parallels those distinctions between sociology and psychology ('institutional' vs. 'individual', 'implicational' vs. 'intentional') that Durkheim went to such great lengths to point out in *The rules of sociological method* The absence of Durkheim is also puzzling in light of the author's use of Karl Popper, whose concept of the 'third world' of ideas seems to have so much in common with the Durkheimian notion of 'social facts' and 'collective representations.'

On the other hand, however, Hanson makes much use of philosophers of history which anthropologists often ignore (to their own misfortune): Wilhelm Dilthey, R. G. Collingwood, William Dray and others. Of particular significance to the work in question

is Collingwood's notion of the nature of metaphysics as the study of 'absolute presuppositions' and their consequences for the nature of thought in various times and places. *An essay on metaphysics* is a book which should be in every anthropologist's library, for it is directly relevant to any study of how societies 'hang together' as wholes and have meaning with relationship to their various parts, and this is the basic issue in any attempt at internal understanding of another culture and the foundation of cultural relativism.

Most anthropologists will probably find themselves in agreement with Hanson's arguments, though they may not have seen them presented in exactly this manner. In any case the book is well worth reading since it presents a sharp and lucid survey of the central philosophical difficulties of the anthropological concern. If it does nothing else it should help set the questions straight.

L. LEWIS WALL

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GRILLO, R. D. *Race, class and militancy: an African trade union, 1939–1965*. 151 pp. New York: Chandler, 1974

The African railway unions in east Africa took shape as organisations representing Africans against Europeans as well as workers against employers. They were led by men drawn from the skilled worker category of the railways (Group B) and tended to look after the interests of this category rather better than those of the unskilled workers in Group C With independence and the Africanisation of senior posts, the position changed. The new governments did not want to continue paying senior officers the sorts of salary that had been necessary to attract Europeans and they started to reduce differentials The unions faced a new situation.

The first part of Dr Grillo's book is called 'The problem and its setting' but the nature of the problem is never made explicit. Presumably it is the interpretation of the organisation's history and the actions of its members. The author presents the basic features of that history and illuminates the period 1964–65 (when he carried out his fieldwork in Uganda) with some excellent case studies of small-scale disputes. He devotes one chapter to a consideration of the important question of why the union played so small a part in representing members subject to disciplinary charges.

The central theme, in Grillo's view, is the worker's changing consciousness of his market situation and of his identity in terms of his race, his status and his class. To begin with the work force consisted of migrants living at subsistence levels, but before long the men settled near their workplaces. They then had to support their families on the spot and to meet expectations of a higher standard of living. Through negotiation and strike action minimum wage rates at the main depots were doubled during 1960–65. At the end of this period the union threatened a strike and appealed for the support of Group B workers by invoking their racial identity. Management warned them that if they joined the strike their suitability as supervisors would be in question, suggesting that their class identity came first. At one depot there was a showdown and many of the Group B staff 'chose to be loyal to their class rather than their race'.

Grillo's racial and class models are far too simple for the situation he analyses. Race is a European concept and though Europeans may define themselves as differing racially from Africans it cannot be assumed that Africans reciprocate. The case studies suggest a political and national consciousness and a black-white opposition rather than a conception of Africans and Europeans as two units in a universe of races. Nor, despite a rather stretched definition of class, does Grillo justify his identification of Group B workers as a class opposed to a Group C class. If notions of class are to be applied to such a situation they demand greater sophistication and a more careful consideration of the changes in class structure associated with independence.

MICHAEL BANTON

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KOECHLIN, BERNARD. *Les Vezo du sud-ouest de Madagascar: contribution à l'étude de l'éco-système des semi-nomades marins* (Cah. Homme N.S. 15). 238 pp, illus., plates, tables, bibliogr. Paris La Haye. Mouton, 1975. F38

The Vezo are one of the economically specialised groups who were more or less directly included in the Sakalava kingdoms of the West Coast of Madagascar. They are a semi-nomadic people living either in tents made of the sails of their outrigger canoes or in impermanent villages situated on lagoons. They live by fishing close on shore or collecting on the coral reef at low tide. The fact that they belong in part to a major cultural political group, while having a highly distinctive culture adapted to very special ecological circumstances, makes them of major interest. B. Koechlin, a student of G. Condominas and A. Haudricourt, has, we are told, carried out over two years' fieldwork with the Vezo. He seems to have been exceptionally well prepared for this, with a good knowledge of the language, and an

expertise in things of the sea: Koechlin had previously been in the merchant navy. Such propitious circumstances in a field area of extraordinary potential have, however, not been sufficient and the book we are presented with is a mess.

The author tells us that rather than write a normal ethnography he has given us a series of 'flashes' of Vezo life to be viewed together with an ethnographic film made by the author. What we have, in fact, is practically unaltered field notes on a variety of disjointed subjects: a little on the techniques of navigation, a little on animals and plants of importance to the Vezo, a little on kinship terminology, odd remarks on incidents relating to religion, a little on Vezo language, a rather lengthy log of a short journey for trading purposes, a commentary on some of the numerous photographs included in the book. All this is unsystematic and incomplete. For example, while some Vezo terms for animal species are given their Latin equivalents, others are not, while kin terms of reference are given, affinal terms are not to be found.

As it stands the book is quite unusable by anybody but the expert on Madagascar ethnography. That is a pity, since scattered between the covers is much useful information which we must be grateful has been made available though this could not be otherwise since we are so ignorant about these people. It seems strange that the *Cahiers de l'Homme* have not insisted, for their sake and for ours, on a better presentation and a fuller coverage.

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ANDERSSON, EFRAIM. *Contribution à l'ethnographie des Kuta II* (Stud. ethnogr Upsaliensia 33). xxiii. 212 pp., bibliogr., illus. Uppsala, 1974. Distributors: Kegan Paul, Trench, Trubner, Ltd. £20

The first volume of Andersson's studies of the Kuta, which appeared in 1953, was a general ethnography. Now, twenty-one years later, comes the second volume, which is concerned with religion: the supreme being; belief in the soul and spirits; and the cult of the ancestors

The Kuta, according to Andersson's definition, are a group of related tribes generally sharing a common culture and history, who are spread over an area 400 miles long from north-east to south-west, now bisected by the frontier of Gabon and the Congo Republic. He first began work among them as a Swedish missionary in 1931, and his knowledge of their language, customs and beliefs is probably unrivalled.

The supreme god of the Kuta is variously named Nzambi, Zambie or Nzyami (an attempt to equate him with the West African Onyame leads nowhere), but he also has two aspects: Nzambi *watanda* (god above) and Nzambi *wamutsele* (god of the earth). This dichotomy is expressed in a number of ways —above/below, sky/earth, male/female, sun/moon—but it is never clearly defined. Nevertheless it serves as the foundation for a highly original hypothesis concerning the interpretation of the *mbulu*, the so-called 'guardian figures' which the Kuta thrust into the baskets containing bones of their ancestors, and to which Andersson devotes nearly half the present volume.

The *mbulu* figures consist of a flattish figure reduced to a large oval head on a hollow diamond body, the whole covered with sheets or strips of trade copper. There are two types: the single-faced head (*mbulu-ngulu*), either concave or convex, and the Janus-head (*mbulu-viti*), with one face convex and the other concave. In 1953 Andersson reported some of his informants as stating that the convex face was male and the concave female. Now he suggests that the *mbulu viti* combines the two aspects of Nzambi *watanda* (convex) and Nzambi *wamutsele* (concave). The argument is that at one time there was only one Nzambi, who created all things and was the giver and taker of life. Then a second Nzambi, a god (or goddess) of the earth and of death, was introduced from the east (there is no explanation of how, when or by whom this was brought about). It was natural to combine the god of death with the god of the sky, which was man's eternal resting place. Later, a sculptural shorthand enabled the two aspects to be united in the single-faced *mbulu-ngulu* by interpreting the convex brow as representing Nzambi *watanda* and the concave lower half of the face as Nzambi *wamutsele*.

Intriguing though this hypothesis is, it is let down by its own author. Firstly, the majority of Andersson's informants were Christian converts, and though he acknowledges this his attempts at discounting any Christian influence on their thought are feeble (e.g. p. 41, n. 3). Secondly, despite his long experience and linguistic ability he appears not to have confidence in his own findings but must continually seek support for them in secondary sources and references to other areas as far afield as ancient Egypt and Iran. Indeed, the bibliography surprises by its inclusion of a number of works long outdated and others that are highly suspect, while omitting everything on the area by Leon Siroto, notably his paper on the Ossyeba (*African Arts* I, 3, 1968), which is of

fundamental importance. Ingebor Bolz's doctoral thesis, 'Zur Kunst in Gabon' (*Ethnologica*, n.F., Bd. 3, 1966), is referred to continually in the text, but Alain Chaffin's much more detailed study, 'Art Kota' (*Arts d'Afrique Noire* 5, 1973), is only touched on tangentially, which is disappointing.

There is an appendix on the 'Skeletal contents of Bakuta ancestral baskets' by Dr Rightmire, and five of the six colour plates illustrate skulls. A third volume in the series is promised, apparently to be devoted to Kuta oral literature.

J. B. DONNE

London

HUGHES, DAVID R. & EVELYN KALLEN *The anatomy of racism: Canadian dimensions*. viii, 230 pp., tables, bibliogr. Montreal: Harvest House, 1974. $8.50 (cloth), $4.50 (paper)

The appearance of this book is an indication of the growing awareness of the racial problem in Canada. Not that Canada has never had racial troubles. The English-French problem is older than Confederation itself and remains the major unresolved social and political thorn in the side of the Canadian nation. There have been 'outbursts' from time to time, involving the treatment of the Japanese-Canadian during the war, and the exhibitionistic parades of the Doukhobors; but these were seen as relatively minor incidents.

The post-war boom in immigration has caused a shift from the official concept of biculturalism to an unofficial concept of multiculturalism, which is fanned by the separatist demands of the radical French.

This book is a serious attempt to analyse the origins of racial strife in Canada, and to explain the dynamics of racial tension in the country today.

David Hughes, a physical anthropologist, has written an excellent overview of 'race' from the biological viewpoint. One needs only to mention the titles of some of his chapters, e.g.: 'The spectrum of man and his diversity'; 'The evolution of man and the origin of races'; 'Inheritance, environment, human adaptability'; 'Race and intelligence'; to give an idea of the field which he covers with a great deal of clarity and comprehensiveness. Summarising the present controversy concerning race and intelligence, he states that 'attempts objectively to relate human genetics and intelligence ... are clearly premature'.

Hughes concludes the first part of the book with a summary of the origins and diversity of the Canadian population. All Canadians are immigrants, or are descended from immigrants. The earliest immigrants came from Asia and are now represented by the Indians and Eskimos (Inuit). Of the two 'charter' groups, about 43 per cent. of Canadians are of British ethnic origins and about 31 per cent. are of French origins. The Metis are a group resulting from intermarriage between people of European origin (mainly French-Canadian) and Indian and Eskimos. Other immigrant groups with significant numbers include: Jews, Blacks, Asians of (a) Mongoloid ancestry (Japanese, Chinese and Koreans), (b) Caucasoid ancestry (Indians from India, and Pakistanis). To complete the picture there are the immigrants from Western and Eastern European countries, e.g. the Italians, Dutch, Polish, Ukrainians, etc.

The second half of the book—by Kallen, a social anthropologist—is a 'structural' (hence 'anatomy' in the title of the book) account of the racial situation in Canada. The concept of 'race refers to or implies any arbitrary classification of human populations, utilizing such biological criteria as actual or assumed physiological and genetic differences between the world's peoples'. Racism is 'a social phenomenon which refers to peoples' attitudes, beliefs and behaviour, predicated on non-scientific and erroneous assumptions about the nature of human diversity'. She distinguishes between 'race' and 'ethnicity' which refers to 'any arbitrary classification of human populations utilizing the bio-cultural criterion of actual or assumed ancestry in conjunction with such socio-cultural criteria as actual or assumed nationality and religion'.

The two 'founding' races, viz.. the English and French occupy the highest ranking status positions, with the English group not only having the numerical superiority, but greater power and prestige. The position and status of the other groups are considered vis-à-vis the majority groups.

The author presents us with very good reviews of the various racial and ethnic groups in Canada today, e.g. Jews, Blacks, Inuit, Italians, etc., showing how they maintain their own identities whilst at the same time integrating themselves within the over-all Canadian social structure.

This is an excellent book which makes a significant contribution to the study of race relations in general, and a welcome addition to the literature on the social structure of the complex Canadian society.

H. M. SAMPATH

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DYEN, ISIDORE & DAVID F. ABERLE. *Lexical reconstruction: the case of the proto-Athapaskan kinship system*. 498 pp., Cambridge: Univ. Press, 1974. £10

The aim of this study is to establish the

kinship terminology of the ancestral Athapaskan language (going back to the early Christian era), and then to deduce from it features of the kinship system of the presumed mother-society of the various contemporary Athapaskan-speakers of Alaska, western Canada, the west coast and southwest of the United States, and Oklahoma. This may sound rather like the sort of thing Radcliffe-Brown was always warning us against, and it is unlikely that even now many anthropologists in this country will wish to undertake a similar study. (Though Robin Fox's monograph on the 'Keresan bridge' had a comparable objective.) Nonetheless, the book deserves to be widely read. For one thing, the methodological sections are full of interest—indeed, chapter 4, 'Kinship-term patterns as bases for inferences about kinship organization' will be valuable for teaching purposes. The critical surveys of the relevant theoretical issues are also suggestive, and, perhaps most important, this book takes one right to the centre of the American ethnological enterprise, still flourishing and still developing—and still barely noticed by anthropologists on this side of the Atlantic.

The method of lexical reconstruction used here proceeds by the identification of the relevant subgroupings of the language family (and it is at this stage that the authors differ radically from their predecessors), and then the ordering of sets of cognates in order to establish the probable proto-language terms The conclusions they draw from this exercise are that (*inter alia*) the proto-Athapaskan system was probably Iroquois though 'with some signs of shift toward Hawaiian terms in some dialects', and that it was bifurcate collateral in the first ascending generation.

What inferences can then be drawn about the kinship organisation? The basis for inference is, first of all, probabilities derived from cross-cultural statistical analysis of the type pioneered by Murdock. The discussion of method here, and the conclusions, should be force-fed to those Bongo-Bongoists (as Mary Douglas calls them) who believe that one exception invalidates a statistical association. The authors conclude that while Iroquois cousin terms and bifurcate collateral terms are in themselves weak predictors of features of kinship organisation, broader but more powerful predictions are made possible by the association of several variables, which are then related to more generalised dichotomies —e.g., bifurcate cousin terms predict unilineality, non-bifurcate (Eskimo, Hawaiian) predict bilaterality. Unfortunately, the authors do not review the arguments of the anti-typologists; and while this service has been well-performed by Buchler and Selby, the omission may prejudice a number of their British colleagues.

The detailed discussion of the various dialects is probably of interest only to regional specialists, but there is an interesting critical review of previous ethnological studies of this problem. The issues raised here are of general interest, and the contributions of some of the great American scholars are reconsidered, including Kroeber, Murdock, White and Opler. The consideration of the (alternative) diffusion hypothesis is particularly illuminating.

Not surprisingly, the more sophisticated the techniques of the ethnologist, the more limited and tentative are the conclusions. Proto-Athapaskan was probably unilineal or double unilineal—matrilineal, in the view of the authors; marriage was polygynous; and marital residence was not neolocal. Further inferences are 'based largely on experience and intuition' a method familiar in Britain, but not always as carefully employed. The system 'was probably characterised by bilateral cross-cousin marriage, *de facto* sister exchange, sororate, sororal polygyny, and levirate'. These conclusions are, very properly, hedged about with reservations.

ADAM KUPER

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SUTHERLAND, ANNE. *Gypsies. the hidden Americans.* x, 330 pp., illus., tables, bibliogr. London: Tavistock Publications, 1975. £6·90

It is intriguing to read in the author's preface to this book that she thanks Stanford University 'for their hostility, because it left her entirely without the taint of anthropological contact in the field' (p. x) Consequently, one reads the book partly asking the questions, why did this disagreement occur, were the University justified in their attitude and would the author's work have benefited from more 'anthropological contact in the field'? Without speculating about the motives of those at Stanford, one is aware that to attempt 'to describe holistically the social organisation of a particular group of Rom' (p. 288) is an enterprise which would be received with raised eyebrows by some anthropologists, as would the secondary aim to humanise the Gypsy and counter adverse stereotypes, based on lack of knowledge, by 'demonstrating the complexity of Rom character and the unique qualities of their way of life' (p. 288). The author's interest is in giving an account of Gypsies rather than in developing anthropological theory, in fact, even the conclusions are severely attenuated, being disposed of in 3½ pages. However, the

descriptive account is mediated by the use of what are assumed to be key anthropological concepts: throughout, there is an attempt to analyse and explain rather than to give bald description only, although the theories in which the concepts used are rooted are not made explicit.

It is, therefore, on its descriptive content that *Gypsies* stands or falls. This being so, the reliability of the information provided is a crucial consideration. One is rather uneasy about the heavy reliance for information on interviews with one highly intelligent and able leader of the Rom community (p. 22), on the accounts of Mrs Tompkins, a social worker, and on participant observations in the role of Principal of a school set up for Gypsy children (p. 23). However, as the author points out, there are considerable difficulties in gaining access to this community and it is no mean achievement to have documented as much as is given in this book.

Rom social life in Barvale, California (a fictitious name (p. ix)), which is 'a depressed town of substandard housing in a large metropolitan area' (p. 35), is described in considerable detail, in a way generally interesting and sometimes highly entertaining. The historical status of the Gypsies as outcasts, a pariah group, has affected their own social organisation and a continuing problem for the group has been that of the form and content of the interrelationships between their community and the wider society. This theoretical area is one which could have usefully been developed more fully, since the most interesting material provided here is that on the interconnexion between the two. I liked in particular the discussions of welfare and housing: in an absorbing section, Anne Sutherland turns on its head the stereotype of the passive, ineffectual 'social problem' Gypsy, showing their sophisticated manipulation of welfare assessment (p. 75) and their ability to fool completely the psychologists administering 'scientific' tests of personality and aptitude (p. 81).

This is a book written as an appreciative understanding of the Rom people. As such it is successful; a detailed and thorough documentation and analysis of what the author was permitted to see of their lives.

SUSANNE WOOD

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LEAVITT, RUBY ROHRLICH. *The Puerto Ricans: culture change and language deviance* (Viking Fd Publ. Anthrop. 51). x, 268 pp., tables, bibliogr. Tucson: Univ. of Arizona Press, 1974. $4.95 (paper)

The author, a speech pathologist and anthropologist (a latter-day field of interest), is interested in the process of social change among Puerto Rican migrants in the United States. In particular she studies the prevalence of stuttering among Puerto Ricans in New York and compares it with a sample obtained in Puerto Rico. Using the hypothesis that stuttering is an indicator of stress in a situation of change, she found that there was considerably less stuttering among Puerto Ricans in New York than among the ones resident in Puerto Rico. As she was expecting the opposite situation, the author sets out to clarify the situation, in terms of an analysis and description of the cultural values and norms operating in both milieus.

Using mainly documentary evidence (we do not know to what extent she did fieldwork in both contexts or of what type) she arrives at the conclusion that traditional patterns in Puerto Rico are more prone to create stress in a situation of change than the patterns of North American origin. The authoritarian nature of the traditional Puerto Rican lower class culture does not provide for the emergence of a personality without stress, and in a situation of change it stimulates the appearance of stuttering. In contrast, while the traditional North American lower-class culture may be aggressive and competitive, it is also democratic and egalitarian, and creates the conditions that permit the emergence of non-stuttering individuals. This, in a nutshell, is the main tenet of the study

This reviewer cannot but briefly comment on some of the limitations of the study In particular, the dualistic manner of perceiving both cultures is projected in terms of characterising the Puerto Rican context as negative and the North American as positive. This weighs the work heavily in favour of structuring a favourable image of North American cultural values as rational, egalitarian, permissive and so on. The subtleties of the Puerto Rican social patterns and values are not dealt with. The authoritarianism of the traditional society of the Puerto Rican lower class is seen simply as an imposition over children and women, without the least structural consideration of a system of differentiated roles. The analysis is mounted on the basis of a 'trait' anthropology.

There is also a lack of historical sophistication so prevalent among some North American social scientists which is reflected in the labelling of aspects of present-day Puerto Rican society as semi-feudal, without a rigorous analytical approach. The chapter on the 'American Heritage' is heavily inclined towards a neo-Weberian theoretical approach that neglects the relations between economic processes and cultural values. Values tend to be seen as 'things in themselves'.

We tend to sympathise with the efforts of

the author, but when the moment of reaching conclusions is arrived at, we are left with the uneasy feeling that in spite of the findings of this study, the Puerto Rican experience in New York is more traumatic and brutal than revealed here. If the author had done more ethnographical research in both contexts perhaps the picture of the stresses and strains operating in the situation could be altered. As such, relying on documentary sources almost exclusively (most of them research studies by Anglosaxon people) does not fully permit the investigator to achieve a truly ethnographical account of the problem. As such, we will still be waiting for studies that explore these dimensions. In the meantime, we will have to rely on the novelist and the reporter.

One last note. In pages 74–5 an unpardonable mistake is committed; 'La Perla' and 'La Esmeralda' are not different slums, but the same under different names.

CARLOS BUITRAGO ORTIZ

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WILSON, PETER J. *Crab antics: the social anthropology of English-speaking Negro societies in the Caribbean*. xvii, 258 pp., figs, tables, bibliogr. New Haven, London: Yale Univ. Press, 1973 £4·25

This book is decidedly not an ethnography of life on the Caribbean island of Providencia, although based on the author's fieldwork there. It is a well written, concise and provocative argument for a particular model of society and a particular orientation for social anthropological research in the Caribbean. The data seem fairly well chosen to support the argument.

The heart of Professor Wilson's thesis is that Providencian society is characterised by and functions through what he, following the local usage, calls *crab antics*, a dynamic, ongoing tension between two kinds of value orientations: *reputation* and *respectability*. 'Reputation tends to stress the equality of human equalities, whereas respectability seeks to rank them' (p. 223–4). Reputation has to do with personal talents, achievements, strengths, which set men apart as unique yet equal. Respectability, by contrast, is based on alien values which, according to Wilson, are those reflected in the standards and norms of English middle-class households of forty years ago.

Whereas reputation seems, for Wilson, to be largely based on masculine 'virtues,' respectability has its context in the household and is thus more the concern of women. But the author asserts that preoccupation with the issues of female dominance and male marginality in households has implied that women may attain their 'existential fulfilment' in the context of household, but has ignored the question of where men might attain such fulfilment (pp. 127–8). He shows that in Providencia, it is the *crew*, the small and informal male group of friends, meeting outside the locus of the household, which forms the context for such male fulfilment and for negotiations of reputation as well.

Wilson sees Providencian and other Caribbean societies as functioning through the dialectical tension between reputation and respectability, manifest in everyday life, and sees this as characteristic of colonial and post-colonial societies. Values of reputation arise from a people as an adaptive response to the alien values of respectability. The author foresees the eventual emergence of reputation as a dominant value orientation for Caribbean societies where the external supports of respectability are eroding.

As Sidney Mintz writes in his foreword to this book, 'there is a good deal to quarrel with in *Crab antics*' (xii). There is, for example, little discussion of contemporary plantation communities, although such communities may exhibit traits (such as landholding patterns and proletarianisation) which differ somewhat from those considered important by Wilson on Providencia. Perhaps most striking, the whole question of the treatment of women in this book leaves many questions unanswered and raises implications which the author, perhaps, does not intend (as in the relation of women to respectability and respectability to colonial status—which, in any case, may strike the reader as oversimplified).

JAMES PHILLIPS

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REICHEL-DOLMATOFF, G. *The shaman and the jaguar: a study of narcotic drugs among the Indians of Colombia*. xxii, 280 pp., illus., plates, bibliogr. Philadelphia: Temple University Press, 1975. $15

Reichel-Dolmatoff examines hallucinogenic drug use, describing traditions, rituals, native interpretations, and the hallucinatory sphere in general. There is an excellent survey of early reports published mainly in Spanish and not generally available.

In a chapter on Tukano shamanism, the author describes the activities and beliefs related to altered states of consciousness, recognising the link between plant hallucinogens and the quest of the shaman. In his

chapter on the transformative vision of shaman to jaguar, he suggests a link with the concept of the 'Master of the Game Animals.' He argues that at times, shamans and other men use large doses of hallucinogenic potions to see who has the mental and physical disposition to become a shaman. A second explanation that he offers deals with revenge—a shaman's retribution of perceived evils, either towards his enemies or those of his clients. Much of this discussion of the shamanic role would benefit from a more anthropologically-oriented theoretical perspective. What seems to be missing is a focus on the historical role of the shaman in stateless societies.

Reichel-Dolmatoff interprets the transformation complex as the embodiment of nature in its most uncontrollable and aggressive sense. In addition to this psychological explanation, he makes a case for a structural interpretation of the jaguar transformation as a mechanism for changing from culture to nature, à la Lévi-Strauss. There are many layers of explanation possible in interpreting drug effects cross-culturally, ranging from the biochemical/neurological to the psychological, to the sociological to the cultural. The neglect of the latter realm in this study is somewhat unfortunate in this otherwise excellent presentation of Tukano drug use. Reichel-Dolmatoff, for example, does not recognise the conceptualisation among many, if not most, traditional drug-using societies of a spiritual hierarchy in nature—an entity of animal familiars linked to shamanic transformation or control as an essential part of the acquisition of shamanic power, particularly when plant hallucinogens are used. In drug-using societies of the traditional world, just as men are ranked spiritually—that is, there is a cross-cultural recognition of the calling to the office of shaman, so too do we find frequent conceptualisations of the paralleling of power in the world of nature, corresponding to controlling elements in shamanistic ontology.

The author's discussion of the effects of Tukano drug experiences on art forms is excellent and breaks new ground. He examines the relationship of a physiological mechanism, phosphenes, as reinforced by plant hallucinogenic drug use. He also explores sexual symbolism and social structural variables linked to Tukano myths concerning *yajé*. I am in total agreement with the author in his assessment of Tukano drug use as a practical mapping of unknown realms and the resolution of daily problems which is dissimilar to contemporary youthful practices in industrial societies for mystical transcedence.

MARLENE DOBKIN DE RIOS
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BASSO, ELLEN B. *The Kalapalo Indians of Central Brazil* (Case Stud. cult. Anthrop.). 157 pp., illus., tables, map, plates, bibliogr. New York: Holt, Rinehart & Winston, 1973

The exemplary efforts of the Vilas-Boas brothers in their fight to protect the tribes of the Upper Xingu River in central Brazil have made the area internationally known and the tribes the centre of much anthropological interest. Of the European and American anthropologists who have worked in the area during recent years (Menget among the Txikao, Gregor among the Mehinaku, Seeger among the Suya, Brecher among the Waura) Dr Basso is the first to publish her findings.

This study provides a general survey of the Carib-speaking Kalapalo in a series of chapters on such topics as relationships with humans and non-humans, subsistence, village and household structure, the opposition of men and women, kinship, ritual specialists, etc. Sadly the book is superficial to the point of almost obscuring any aspect of Kalapalo life that is unique to their culture, as well as failing to evoke anything of what it must feel like to be one of 110 Kalapalo living in a circular village in the Mato Grosso in near-total isolation from Brazilian society Basso seems to have fallen into almost every anthropological trap. She dismisses the highly complex Kalapalo ritual life and symbolism with the statement

> The majority of ceremonies performed by the Kalapalo have little ideological significance beyond their association with specific sponsors. The performances are extremely stereotyped and repetitious, and almost invariably the songs associated with them are unintelligible to the singers . . . there is little, if any, manipulation of ritual symbols, and performances often offer no excitement after the first or second day, until the climactic event marking the last day of the ceremony (pp. 110–11).

She tells us that childbirth allows a woman 'to join a community in which the most hidden secrets are revealed' (p. 64), but fails to give an explanation of what those secrets might be. The paternal role in the birth ritual is judged as negligible: the only explicitly paternal taboo for the fathers of newborn sons, she claims, is that they are prohibited from killing jaguars. Certainly this deserves more comment since the fathers in neighbouring tribes of the Upper Xingu are subject to radical restrictions which are considered crucial to the infant's survival (not the least of which is sexual abstinence until the child has begun to crawl).

Basso fails to see, or rejects without explanation, the parallels between the

seclusion of the infant after birth and the return to seclusion during adolescence. Parental responsibility for the child's survival during both periods is a form of extended couvade where concern for spiritual and physical selves at particular moments in the life cycle may well be a distinctive feature of the bodily cosmology of the Upper Xingu tribes. In Basso's description of adolescent seclusion, her only attempt at analysis is to quote the Kalapalo explanation of this period as being for the purpose of making the adolescents 'grow beautiful' (p. 71). She further states that secluded adolescents are 'forced to submit' to continual scraping with dogfish teeth (p. 71). No mention is made of the herbal emetics which are held to assist the adolescents' growth, nor is there any hint that the scraping with dogfish teeth is done to remove blood from the body: on my observation the symbolic properties of blood and semen are clearly in opposition, and blood must be released, just as semen must be retained through sexual abstinence, if the desired physical growth is to occur. The most likely explanation of Basso's account is found in her admission, in the section dealing with the 'Effects of depopulation on marriage practices', that in 1966–8, during her stay, there were no girls in puberty seclusion and only at the end of her fieldwork did two young girls start to menstruate (p. 101).

Basso's remarks on the role of women in the Upper Xingu, a subject on which she was in a uniquely privileged position to comment, is perhaps the most disappointing aspect of her findings. In her section on ceremonies she merely states that 'women are most often onlookers, or at best, participate only in the dancing' (p. 138), and then goes on to describe three major rituals (*egitsu*, to commemorate a recently dead chief, *ipoñe* or ear-piecing, and *ifagaka*, or spear throwing to honour the dead) which clearly could not take place without female participation. Basso fails to note that rituals cannot be held without an abundance of food, and that this food must be prepared by women. She also does not recognise that the exclusion of women from full observation of a ritual is in itself a form of participation. The fact that women are not allowed to look at the instruments of the exclusively male trumpet cult does not imply that their presence when the trumpets are played is not essential. There would be no point to the cult if the women were not there as an audience.

This book provides an interesting comparison with the other volume on South American forest Indians in this series—Chagnon's highly popular *Yanomamö* (1968). Basso draws attention to the adherence in Kalapalo society to the principle of *ifutisu*, an ideal of peaceful, generous behaviour, which may be contrasted with the Yanomamö emphasis on ferocity and individual achievement.

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Abarbanel, Jay S. *The co-operative farmer and the welfare state: economic change in an Israeli moshav*; foreword by Max Gluckman. xvi, 236 pp., illus., tables, bibliogr. Manchester: Univ Press. 1974. £4·50

This is a study of a village of modern, technologically-oriented, cash farmers who lead a middle-class life style. Such people, sophisticated and always pressed for time, make for notoriously difficult field situations. Yet these must be faced, as Abarbanel has valiantly done, if anthropology is to make direct contributions to the study of developed societies. The objective difficulties have been overcome partially. We gain an insight into the socio-cultural fabric of the village, but not very deeply. Biographical sketches and some case material are offered in the tenth of the eleven chapters of the book, but still the people barely come to life.

The author's aim is to illuminate the economic actions of the villagers, and he provides rich economic data. However, there is a relative paucity of data on the non-economic aspects of the villagers, that are needed for a deeper insight into their economic actions. Abarbanel describes his farmers as always choosing the materially sensible alternative of those available. This idea could have been probed by analysing individual cases of economic decision and uncovering the actors' deliberations and evaluations of the relative material, social and other non-material costs of their economic alternatives. The paucity of the cultural data detracts when the author pursues the standard anthropological concern of tying together economic aspects of the society with other aspects (such as ideology, ethnicity, kinship, etc.). In the treatment of ethnicity for instance, limited insight into the salient social categories, reduces the author to construct somewhat questionable ethnic categories that are then correlated with the varieties of economic activity. Abarbanel uncovers many interesting ethnographical features. A considerable number of people in the village are related by family ties. There is a high degree of mutual aid between farmers, which is however not generally extended to immediate neighbours. Friendship circles formed by the women exist, in which the husbands also participate, but the friendship circles seem to be distinct from the mutual aid partnerships. All these features have not yet been reported for this kind of

Israeli village and invite further study. The book is particularly valuable as a document of the ecology and general agricultural economy of a successful type of planned community in the context of its national economy. Here the author is at his best. These aspects of the village are spelled out very fully and clearly. The book will interest rural sociologists and agricultural economists, besides economic anthropologists.

SHLOMO DESHEN

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WESTPHAL-HELLBUSCH, SIGRID & HEINZ WESTPHAL. *Hinduistische Viehzüchter im nord-westlichen Indien.—I. Die Rabari.* plates, maps, bibliogr. Berlin: Duncker & Humblot, 1974

This is an attempt to shed some light on the cultural history of the Rabari, a hitherto little known group of traditionally semi-nomadic animal-breeders. Their number was about 415,000 in 1931, their numerous sub-groups are dispersed over the dry tracts of Cutch, Saurashtra, Gujarat and south-western Rajasthan.

The written sources on which this investigation is based are extremely scanty. The authors' patience in sifting and compiling them deserves full acknowledgement. However, the value of what they term 'field-research' may be open to doubts. Except for the general information that it was conducted in 1968/69, little more can be gathered than that an unspecified number of Rabari groups were visited. Naturally, the authors were so perplexed by the extent of local variations that they decided 'to spend all their time on obtaining a survey'—with the inevitable result that 'the scope or research expanded so much that it could only imperfectly be managed' (p. 14).

The ethnographic material is presented under a number of conventional headings. No attempt is made to test any generalised set of explicit hypotheses. The ulterior aim of the present book as well as of two preceding ones on the (Muslim) Jat (1964, 1968) is, it seems, to provide an ethnographic basis for a comparative study of Hindu and Muslim animal-breeders in the region of their traditionally most intensive contacts, i.e. north-western India. As for the Rabari, the more general conclusions are the following; All Rabari groups agree on a common ancestor (Shamal) and a common original home, i.e. 'the semi-desert region between Sind and the Aravalis' (p. 299). They are, however, not a homogeneous ethnic group but rather a caste. For about a thousand years they have been, in small groups, on the retreat towards the south under the pressure of Muslim enemies. The camel-breeding groups arrived in the southern areas probably some centuries earlier than the sheep- and goat-breeders. The extent to which, in the process, Rabari groups were absorbed by other, especially animal-breeding Jat, groups cannot be assessed. Whenever larger numbers of them settled in politically well-defined territories, the respective authorities appointed individual Rabaris to act as representatives of territorially defined groups. Contacts between the major groups—Cutchi, Dheberya, Sorathia, and Gujarati—were until recently, when measures of the British and Indian Administration again initiated changes, virtually non-existent.

GÜNTER TIEMANN

BÉTEILLE, ANDRÉ. *Studies in agrarian social structure.* viii, 206 pp. Delhi: Oxford Univ. Press, 1974. £2·55

In this collection of 8 essays, written over the past 6 years, Professor Béteille sets out to deal with a major sociological problem, and also to suggest a new direction for his colleagues to follow in their research. Both have to do with the use and control of land in rural India.

In looking at relations of inequality in rural India, Béteille points out that these have generally been considered in terms of caste. But there are relations which, though linked to caste, are not by definition caste-based—of landowner to tenant, tenant to labourer and so on. These have been less considered so far, yet they are crucial in a society where land is so important. The question then arises—if these are class relations how is class to be defined, and to what extent does the agrarian system generate class interests and conflicts?

As Béteille says, the essays do not form a simple progression in developing this theme, since they were written at different times and for varying readerships. But they each provide a refraction of this central interest and of his general position. Briefly, Béteille does not believe that class concepts developed outside India are necessarily appropriate there, and thinks that any class analysis should stem from the categories of interests in land recognised by the people themselves. But these provide a complex picture. For instance, the case of a man owning some land and leasing some more, who cultivates partly through a sub-tenant and partly through his own family and through landless labourers, shows that a tripartite scheme of owner-tenant-labourer is often inadequate; and it is only rarely (and Béteille gives an example) that a dichotomising of interests into landed and landless occurs. Again, Béteille shows from his data that the

oft-made prediction that the 'green revolution' will bring class conflict may be true for one part of a district, but a different pattern of agrarian relations may produce a very different result in the other part. Yet again, Béteille does not agree with the common view that class is taking the place of caste: the latter may be declining, but amorphousness rather than a class system seems to be emerging.

In short, Béteille suggests that it is much too early for conclusive, much less dogmatic, pronouncements about classes in rural India to be made. Rather, we must at this stage collect a great deal more material and consider its complexities. Here we come to Béteille's suggestion that the flagging interest of many of his contemporaries in village studies be revived by work on agrarian relations there—not to supersede but to complement the work done on village caste systems.

Like all of Béteille's work, the essays are clearly written, thought-provoking, and argued with skill and authority. As he ruefully remarks, his use 'as the occasion demands, [of] the Marxist method of analysis without paying too much attention to the ideological objectives of Marxism ... is likely to make my work suspect in the eyes of both orthodox sociologists and orthodox Marxists' (p. 13). Perhaps; but readers of all views will recognise the book's importance.

ADRIAN C MAYER

SOAS, London

FUCHS, STEPHEN. *The Aboriginal tribes of India.* viii, 307 pp. Delhi: Macmillan India, 1974

The author of this book has set himself the formidable task of providing in just over 300 pages an ethnographic survey of all the major aboriginal tribes of India. As if this were not a sufficiently daunting enterprise he has added an introductory chapter on the prehistoric races of India and concludes the book with a brief account of the problems of tribal welfare. The volume is obviously intended for Indian students embarking on an introductory course of anthropology and looking for a guide through the complexity of the tribal scene. As such it may serve a useful purpose, and those without access to the old 'Castes and Tribes' series will find in it short accounts of numerous tribal groups many of which were described in greater detail by the authors of those voluminous works. As far as possible these sketches take modern developments into account and it is an advantage to have relatively recent census-figures, though through no fault of Dr Fuchs those of the 1971 census were not available when the book went to press.

In a compilation of this kind a certain unevenness is almost unavoidable. The sections relating to areas with which the author is familiar from his own extensive field-research in Middle India reflect a greater depth of understanding than those based on literary sources of varying reliability. More fundamental is the difficulty of summarising in chapters devoted to regions as large as 'Eastern India' or 'Southern India' such subjects as the social structure, political organisation, and religion of the many different tribes living in the area. Such summaries inevitably contain generalisations which may apply to some but by no means to all the tribal societies described in the section.

In chapter 2 which deals in a general way with various aspects of Indian tribal societies the author demonstrates his well-known expertise on the problems besetting the aboriginals in modern times, and in his assessment of the present situation he evinces insight and a genuine sympathy with the tribesmen's struggle against the economic as well as the cultural pressures of more advanced populations. Readers acquainted with the position of the tribal populations of India are not likely to disagree with Father Fuchs when he writes:

> At the present rate of progress it will take at least fifty years more for the tribal people of India to catch up with the rest of the population as they are now. In the meanwhile 'the rest' will progress further at an even faster rate during the next fifty years. Thus the gap between the tribals and the non-tribals will widen even more (p. 299).

C. VON FÜRER-HAIMENDORF

SOAS, London

HAIMENDORF, CHRISTOPH VON FÜRER. *Himalayan traders: life in highland Nepal.* xvi, 316 pp., plates, map, tables, bibliogr. London: John Murray, 1975. £7·50

This book makes little contribution to current anthropological theory but will be of inestimable value to future anthropologists. It is in the best tradition of ethnographic recording, a nostalgic journey through an area where very little has been recorded and much is disappearing rapidly. The peoples studied are all of Tibetan origin and live on the northern border of Nepal. Four chapters describe change in the Khumbu region since the period studied in the author's earlier *Sherpas of Nepal.* Three further chapters provide a brief description of other Bhotia groups, one near the Sherpas and two in western Nepal.

The central paradox to which Professor Haimendorf seeks an answer is that the inhabitants of the high, stony, inhospitable mountains are conspicuously richer than the peoples living in the agriculturally more prosperous regions. The reason for this is trade, particularly the movement of Tibetan salt from the north to be exchanged for Nepalese grains from the south. Fieldwork over the period 1953 to 1971 enables the author to see the way in which the closing of the Tibetan frontier by the Chinese in 1959 affected a people so heavily dependent on trade. He suggests that the Sherpas turned their surplus manpower towards exploiting the growing mountaineering and tourism business. Some of the other Bhotia peoples, who were less accessible and less well known as guides, were unable to survive as well and there has been a drop in living standards and population. Although in the short-term the situation is not desperate, and the author constantly stresses the resourcefulness and adaptability of these peoples, there are long-term dangers As Professor Haimendorf notes, if the current population growth in central Nepal continues there will be no grains to export Furthermore, improved communications from India will make Tibetan salt totally superfluous. The last vestiges of the trade will fade away. Meanwhile, the one major asset of Nepal, her forests, are shrinking very rapidly in these high regions, as elsewhere Fuel shortage and erosion lie ahead. On the whole, however, the tone of the book is optimistic and there is little sentimental attachment to the old order.

The presentation is well suited to the theme and to the publishing of useful ethnographic data. The case studies of trading and agriculture, as well as the statistics on economics and demography, are most helpful. The maps and photographs are excellent and, combined with certain descriptive passages, really do convey a vivid feeling of what the region is like. The only criticism here is that a curious system of randomly indenting many paragraphs is irritating.

Negatively, as the author admits, brief acquaintance with some of the areas makes it impossible for him to discuss social structure, religion, cosmology and a number of other dimensions represented in the *Sherpas*. Nor is there any real attempt to draw wide conclusions or to place the study within any wider theoretical framework. There are interesting speculations on the reasons why these peoples are such avid traders. Some of the effects of trading patterns on social mobility and hierarchy are explored tentatively. Yet there is very little reference to wider theoretical discussions. This is a book almost devoid of explicit theory. Given the nature of the fieldwork and scarcity of even elementary information on the area surveyed this is both understandable and justifiable. Once again Professor Haimendorf has been to regions hitherto largely uncharted and has given us a fascinating preliminary description.

ALAN MACFARLANE

University of Cambridge

BEYER, STEPHAN. *The cult of Tārā; magic and ritual in Tibet.* xxi, 542 pp., illus., maps. Berkeley: Univ. of California Press, 1973. $20

Anthropological studies of Tibetan religious institutions and practices are few and far between. Regrettably no trained anthropologist ever worked in Tibet when western scholars such as Guiseppe Tucci could still enter that country, and most Tibetologists concerned themselves with the study of scriptual texts rather than with the observation of Buddhist ritual and inquiries among its practitioners. Ironically it was only when Tibet was no longer accessible that such observations were undertaken among the Tibetan speaking Buddhist populations of Nepal, Sikkim and northern India, regions on the periphery of the Tibetan culture sphere Dr Beyer's study too is based on data collected in a monastic community of Tibetan refugees in Himachal Pradesh, where he spent more than a year.

Beyer has chosen the cult of the goddess Tārā as the focal point of his study, but his observations extended beyond the rituals connected with her worship. After a historical introduction, in which he traces the development of the Tārā cult as far as the fourteenth century, he describes in great detail the organisation of the Drugpa monastery from which his informants hailed. There is a vivid account of the training of novices and monks, and this is followed by a description of the rituals which bind the monastery to the surrounding lay community. An essential condition for the effectiveness of rituals is the contemplative training which enables a young monk to visualise the assembly of the deities to whom the ritual is directed. Simultaneous with this training in visualisation is the conscious inculcation of certain basic Buddhist attitudes with which the monk has to approach the magical power he is expected to attain through the rituals. There is great emphasis on processes of purification by which the proper use of magical power is guaranteed. Extremely instructive sections deal with the ritualisation of metaphysics and the classification of the gurus and deities invoked in tantric ritual regularly practised by the monks. Distinct from the fierce and potent deities with their weapons and sexual

embraces is Tārā, a loving and abiding deity, who is constantly available to her devotees and is worshipped daily rather than by any great ceremony taking place only once or twice a year. To one who has often watched Tibetan rituals without comprehending all the details of the procedure, the step by step analysis of elaborate monastery services, complete with the translation of prayers and chants, is of absorbing interest, and I know of no better introduction to Tibetan religious practice.

In addition to the meticulous description and interpretation of rituals, the author outlines lucidly and systematically the concepts and moral attitudes which form the ideological basis of the Tibetans' religious tradition. To the anthropologist the intertwining of magic and the cult of deities, and indeed the complementary character of magic and ritual, will be of special interest. The statement contained in the Foreword by K. W. Bolle, that 'no study of magic can be undertaken from now on that does not take the present study into full account' may be only a slight exaggeration. Anthropologists and students of comparative religion certainly owe a debt to Dr Beyer for having opened up a new avenue to an understanding of one of the major religions of Asia.

C. VON FÜRER-HAIMENDORF

SOAS, London

MOORE, FRANK J. (ed.). *Thailand—its people, its society, its culture*, with chapters by Clark D. Neher (Human Relations Area Files Survey of World Cultures 15) 607 pp, map, tables, bibliogr New Haven: HRAF Press, 1974

This is an updating, to 1971, of the volume on Thailand in the Country Survey Series of the HRAF (eds. Blanchard *et al.*) first published in 1957 and revised in 1966 It relies heavily on its predecessors, as the editor tells us, except for 5 of the 28 chapters (on politics, administration and diplomacy) which have been newly written by Clark Neher. The editor also warns that neither time permitted nor terms of reference required 'checking the extent to which the earlier texts still reflect the views of the original contributors'.

The layout of the 21-page selected bibliography reflects the structure of the volume: (1) General and social (13 chapters); (2) Political (6 chapters), (3) Economic (9 chapters). The farming out of chapters to different writers has resulted in a rather disjointed survey; so that for example there are separate chapters on social organisation (by Herbert Phillips—most of the authors remain anonymous), 'The family', and on 'Life cycle and patterns of living'; separate chapters on social values and attitudes, religion (by Herbert Phillips), artistic and intellectual expression (by Walter Vella), and on political values and attitudes. Anthropologists should note that the chapter on ethnic groups is particularly disappointing.

The concluding paragraph of the Preface is worth quoting in full as an indication of the book's purpose:

> A handbook such as this is essentially a compilation of facts on Thailand. Its purpose is neither to provide speculative insights nor to advance challenging hypotheses. It is intended to provide in a single volume information on the broad spectrum of all major aspects of Thai culture for the general reader who seeks a convenient introduction to Thailand.

The problem is not so much perhaps the lack of reliable statistical data to which the editor refers, but that a number of clichéd 'speculative insights' and 'hypotheses' and generalisations come to be regarded as 'a compilation of facts', for example statements about: 'the individualism of the Thai', 'a strong sense of national identity'; 'indifference of the people to all forms of political activity', 'the Thai tradition of social mobility'; or that 'the Thai village has a virtually classless society'; 'the Thai traditionally have preferred to work in agriculture'; 'rural society is characterized ... by a relative lack of elaboration, complexity, or institutionalisation in the social forms' (*sic*), 'there are few hard-and-fast rules governing an individual's social relationships with other people'.

Much of the weakness of the volume stems from the general conception of a survey of world cultures, which cannot be taken up in this review. Nonetheless the editor is right in claiming that this volume is a convenient and comprehensive introduction for 'the general reader'.

ANDREW TURTON

SOAS, London

BUSCH, PETER A. *Legitimacy and ethnicity: a case study of Singapore*. xvi, 157 pp, illus,. tables. Lexington: D. C. Heath, 1974. £6·80

For Professor Busch the political system of Singapore is of special importance because among modern states it is simple: small, highly urbanised, and under the constant control of a state apparatus with which the citizens are in direct relationship. In that setting he sets out to study 'the causes of legitimacy' (a moral bond between citizen and state) and social cohesion. The second noun in the title is more obvious: Singapore

carries forward into the Republic the legacy of the colonial plural society. A good deal of the analysis concentrates upon entities called 'Chinese' and 'Malays'—the 'Indians' and others are for convenience set aside.

In a short book—one is not given a great deal for one's £6·80—little is said on the historical development towards what is now to be observed, the nature of Singapore society is left vague, and not enough is made of the Malaysian and Indonesian contexts within which political life in Singapore is in part played out. The data collected in 1970 were from a questionnaire administered to 3,316 secondary schoolchildren, supplemented by a handful of intensive interviews. From them a statistical model is built, and from that in turn some implications for policy are tentatively derived. Some of the conclusions are interesting, as when for example Professor Busch says of the Malays that they 'are undergoing a fundamental shift away from racial self-denigration and that this process will eventually link ethnic frustrations and animosities to political attitudes' (p. 110); and the work is at once respectful of the achievements of the Republic and critical in its efforts to get below the surface of political attitudes. Much is of course said about education and the People's Action Party.

The book appears to belong to a well-recognised genre of American political science, but to anthropological eyes the scope of the investigation seems very narrow and the chief mode of enquiry superficial. 'Clearly the most important measure in this chapter . . . is the dependent variable labelled *Legitimacy*' The latter is derived from two questions the first of which is.

> Imagine that you were married and had children. Imagine also that you had a good job that paid a lot of money. Now suppose the government asked that you give up your job and take a job that paid much less but would help Singapore. How willing would you be to take the government job?

The second question is:

> Frankly speaking, how willing are you to fight and die for Singapore?

For each of these questions, the respondents had to circle one of four answers forming an intensity scale (p. 44). Imagine.

MAURICE FREEDMAN

Oxford University

GRANET, MARCEL. *The religion of the Chinese people*; translated, edited and with an introduction by Maurice Freedman (Explor. interp. Sociol.). viii, 200 pp., bibliogr. Oxford: Basil Blackwell, 1975. £6

Marcel Granet (1884–1940) is best known in anthropological circles for his reconstruction of ancient China's marriage system, as interpreted by Lévi-Strauss in *Les structures élémentaires de la parenté*. He is also considered to be one of this century's leading sinologists, with an impressive list of publications to his credit including several works on kinship, religion, and mythology. It is ironic, therefore, that Granet's work should have had far more impact on general social anthropology than it has on the specialised field of sinological anthropology. (To take only one example, his name does not even appear in the six most recent monographs on Chinese kinship.) The imbalance may soon be corrected, however, with the appearance of the late Professor Freedman's long-awaited translation of Granet's *La religion des Chinois*. Written in 1922 for a general audience, the book attempts to reconstruct the evolution of Chinese society from its ancient origins to its modern form.

An important feature of this English edition is the translator's 'Introductory Essay' which places Granet in historical perspective and evaluates his contributions to French sociology. Professor Freedman's main argument is that Granet must be understood in the context of Durkheimian sociology rather than history or sinology (he was Durkheim's student and Mauss's personal friend). He wrote much of his sinological material in the idiom of the *Année Sociologique*. Freedman recounts how Granet once informed his class, 'I don't give a damn about China. What interests me is Man.'

Granet devoted most of his energies to the studies of early Chinese civilisation and rarely got beyond the Han dynasty (206 B.C to A.D. 220) in his writings. This is one reason why he is now a neglected figure in contemporary Chinese studies. In *The religion of the Chinese people*, however, he allows himself to comment on the full sweep of Chinese history, right up to the early twentieth century. The last chapter, 'Religious sentiment in modern China', contains the only published record of Granet's personal observations. There are hints here that he was a sensitive and critical observer, but the book is not very useful as an ethnographic source. As Freedman notes, Granet did not conceive of himself as a fieldworker: '. . . he was no more conventionally "in the field" in Peking than a Chinese scholar could be said to be who spends a couple of years in Paris studying French literature or history—or perhaps, studying French scholars studying French literature or history' (Freedman, Intro., p. 13). In fact, Granet did not believe that observational data were necessary for an understanding of Chinese society. He often

denigrated the work of J. J. M. De Groot, a contemporary Sinologue who produced an encyclopaedic study of Chinese religion based largely on first-hand research.

Granet obviously felt more comfortable working with historical sources. The first chapter of *The religion* is an attempt to reconstruct the nature of pre-Han religion from cryptic references and linguistic survivals found in the oldest Chinese classics (especially the *Shih Ching*, 'Book of Odes'). This involves a considerable amount of speculation on what appears to be the flimsiest of empirical grounds. We are told, for instance, that the ancient Chinese reaffirmed their sense of community during 'powerful orgies' and 'great sexual festivals', but the original textual citations are not provided. Anthropologists with an interest in the history of kinship studies will want to read Granet's analysis of the Chinese family. Here as elsewhere in his writings he argues that the contemporary patrilineal arrangement emerged from a primordial system based on intermarrying matrilineages. It will be clear to most readers that Granet's choice of topics as well as his interpretations are often dictated by a somewhat mechanical application of evolutionary models characteristic of an earlier phase in sociology. Yet, it is unfair to judge Granet's work by contemporary standards. *The religion of the Chinese people* should be read as an historical document of the Durkheimian school and as an example of reconstructive sinology. This is the way Freedman intended the book to be treated.

J. L. WATSON

SOAS, London

WORSLEY, PETER. *Inside China*. London: Allen Lane, 1975. £6

The title of this book is somewhat puzzling. *Inside China* is written from an outside perspective, outside of China, by an outsider. The perspective is something of an afterthought on a journey through the country, where impressions are matched with contents of books. It is also claimed to be an anthropological perspective in that the readers are invited to think anthropologically about China, 'i.e. against the background of their own society, seeing the one against the other' (p. 19). The author brought out from China more than 200 pages of notes and over 500 photographs, which is an impressive gain from a stay of three weeks. His notes, however, have not been used as a basis for an argument. Rather they are provided for the reader as illustrations embedded in a straightforward discourse which the author could have written whether he had been to China or not. Professor Worsley's China experience is a short one, and, to the knowledge of the present reviewer, he has not been involved with Chinese studies before. It should be stressed though that he appears well read; the factual mistakes I have been able to detect are very minor indeed and it is not worth while to list them here.

Styles in anthropology vary a great deal. The author's invitation to his readers to think anthropologically is hardly matched by his own efforts, unless sweeping generalisations are the essence of his variety of anthropological thought. Actually, we know very little about contemporary Chinese society, and therefore most anthropologists and sociologists who have worked on China will be less prone to such generalisations. But obviously, the author has not aimed at writing a scientific book in a strict sense of the word.

If a professor of sociology gives an account of travels in a country, one expects observancy. This book assures us that its author did not waste his time in China. His reporting is interesting, but polished and straightforward like a page in *China reconstructs*. He is interviewing about wages and work, touring schools and hospitals, conversing with academics and workers. These efforts do not bring out much new information. The reporting is like that of a TV series on China, the language has a sort of visual quality which is pleasant but somehow superficial. Worsley knows his basic facts, but his statements and interpretations are often questionable.

Again, the book is often curiously argued. For instance, Worsley maintains that there are three sets of students of China: (1), sinologists, mainly interested in imperial China, (2) China-watchers, mainly interested in communism from a hostile point of view, and (3) people of the Left, sympathetic to China and to socialism. It is maintained that the two former categories converge ideologically in that their message is that contemporary China has not really changed all that much (p. 126). It is pointless to quarrel with such views. What is strange though is that the author, obviously not belonging to the former categories, does not provide a marxist interpretation of contemporary Chinese society; rather he gives an account of China that provides him with the opportunity for a general discussion of his views on socialism.

Not having the advantage of being British, the present reviewer finds it difficult to grasp for whom this book has been written. Its style seems to suggest that its target is the carriers of 'informed opinion'. The readers of *Man* will probably find the book more interesting with regard to what it says about Professor Worsley than what it tells about China.

GÖRAN AIJMER

University of Gothenburg

BARNES, R. H. *Kédang: a study of the collective thought of an Eastern Indonesian people*; foreword by Rodney Needham. xvii, 356 pp., plates, maps, figs, tables, bibliogr. Oxford: Clarendon Press, 1974. £7·00

Kédang, a linguistically distinct region of Lembata (Lomblen) in the Lesser Sundas with a social order based on asymmetric prescriptive alliance, is the locus for this rich and fascinating analysis of a conceptual order based upon 'a form of dualism consisting of pairs of ranked and complementary opposites' which 'is the germ of a number of systems of oriented relations which show the features of segmentation, irreversibility, and self-closure' (305). Barnes skilfully traces these themes by analysing daily and ritual terminology and aspects of behaviour in diverse settings: the focal mountain, so important in Kédangese mental geography; the ritual centre village; the house and its symbols, spatial orientation; village officials and officiants, with the diarchic pattern; divinity, earth, and heavenly bodies; the yearly and life cycles, with a valuable discussion of 'time'; witches, spirits, and magic; key (and pan-Indonesian) idioms of 'source-origin'; and then social categories of clan and alliance, marriage, relationship terminology, and bridewealth, concluding with a statistical summary of marriage relationships in one village area. His chapter on 'The yearly cycle', for example, concerns ways in which Kédangese order sequences of events, 'cyclically because they are irreversible' and proceeding 'through the sequences until they return to the beginning' (140), and these notions are followed in considering the life cycle. His sophisticated discussion of 'time' supports Leach's view that such metaphors of repetition as marriage alliance or alternation of generations are significant for understanding 'time' in a cultural context.

Needham notes, in the Foreword, the resemblance to Bateson's *Naven* in the way in which Barnes seeks underlying principles and uses agility in connecting 'structure, form, metaphor, and image' (vi), and I would agree. It should be noted that the book, in form and approach, also resembles Needham's well-known article on the Purum; and it provides extensive analysis for one society comparable to what Van Wouden tried for the whole region in his *Types of social structure in eastern Indonesia*. Thus readers who have followed the discussion of prescriptive (marriage) alliance in particular, or wider 'structural analysis' of myth and religion and the social order, will find *Kédang* required reading.

I can hardly summarise the final third of the book which concerns clan, alliance, and marriage, but I should note that Kédangese honour a categorical marriage prescription; they express the concept of closed alliance cycles; and their marriages establish alliance relationships which are managed, via bridewealth, at the clan level. Kin terminology generally follows the form reported for other such societies, and Barnes provides the salutory perspective of kin term usage by both men and women (an example of his careful work). Arranged marriages, though, are exceptional; links between local groups are multiple; and actual cycles are absent; but Barnes shows that 'there is a high degree of correspondence between the actual pattern of marriage and the formal marriage ideology' (296). Of the total number of marriages in his genealogical record, about half could be analysed reliably. Of these, 62 per cent. are with women in the prescribed category; 89 to 93 per cent. (depending upon a variable) 'are into the prescribed category *or* initiate new relations, while 7 to 11 per cent are with women in the prohibited category' (296).

Barnes says that Kédang's asymmetric prescriptive alliance is 'an almost ideal example of the form', but he adds, 'However, a vigorous empiricist might deny that they belong to this type of society at all' (238). His purpose is 'to set out the facts in the clearest way possible', and not to categorise the Kédangese; and he says that the empiricist who 'insists on referring to relations between groups' can hold that viewpoint, while another person who 'refers to rules and relationship terms' can hold that viewpoint. This clear dichotomy may not include all *Man* readers seeking to understand such systems, and many of their problems of understanding may not be resolved by this trip to Kédang with Barnes However, he provides rich information and analysis for those who will pursue the matter, and as he clearly states, more research is required (which is not the usual platitude, but evident.)

The invocation of 'collective representations' and 'collective thought' may bother other empiricists who will ask how and to what degree Kédangese concepts are collective, viable, multiple, or debated. Barnes chose to focus on 'the traditional culture' and life in the ritual centre where 'many of the people who maintain traditional ways lived'; and his report 'does not present a study of culture change or attempt to give an account of what the Kédangese regard as the "modern" elements in their life' (24). Yet, 'most marriages involve at least one person who is Catholic or Muslim' and the ceremonies now are an 'amalgam' (255); village ceremonies 'are no longer held, as so much of the village has been converted to Islam or Christianity' (135); and the absence of some clan temples, certain restrictions, and ceremonies 'is un-

doubtedly associated with the disruptive influence of Christianity and Islam' (164–5).

Having worked in a neighbouring area, I can sense the continuing influence of concepts and institutions which he describes even with religious change, but not all readers bring this perspective. Given that 'disruption' exists, and 'the traditional' is a minority paradigm of Kédang, I would have welcomed from Barnes greater discussion of how the Kédangese conceptualise, categorise, and orient themselves to 'the traditional' as such, and how this might affect an observer's capacity to perceive 'the traditional' and 'collective representations'. Perhaps I am requesting another book, for which *Kédang* provides an excellent background.

CLARK E. CUNNINGHAM
University of Illinois

JENSEN, ERIK. *The Iban and their religion* (Oxf. Monogr. social Anthrop.). xii, 242 pp., illus., plates, map, tables, bibliogr. Oxford: Clarendon Press, London: Oxford Univ. Press, 1974. £4 50

Students and teachers of social anthropology, and not only south-east Asian specialists, will welcome this first monograph-length study of the religion of the Iban. Such a study has long been awaited, though Dr Derek Freeman has published several articles, and approximately one quarter of the present work has already appeared in substantially the same form in volumes of *Folk*.

The material was gathered during extensive fieldwork carried out between 1959–1966 mainly in the Lemanak area, Lubok Antu District in the 2nd Division of Sarawak. It is an account of the religious beliefs and practices of Iban swidden rice agriculturalists at that time and place. It intentionally omits historical data (e.g on headhunting and warfare), and data on settled cash crop cultivators and rubber producers, who are the Iban most affected by school education and Christian religion. It does not intend to be a comparative study, though there are frequent references, sometimes critical, to the work of Sandin, Harrison and Freeman (the latter's study based on the Iban of the Baleh area, Kapit District, 3rd Division). However we are told that 'the general uniformity of Iban religion is remarkable' (p. 59), with the exception of Saribas-Krian Iban mortuary ritual. It should be noted that Jensen's account in a way balances Freeman's in that it restores the importance of the long-house community as a ritual and jural entity, and the importance of ritual leadership.

The book is divided into three parts. The first part is a summary account of social and economic organisation, which largely confirms Freeman's more extensive account (Derek Freeman *Report on the Iban* 1970 first published as *Iban agriculture* 1955). The second part presents Iban religious ideas and religious specialists, which the third part then describes in action, in ritual practices connected with rice cultivation.

In the second part there is detailed material on the crucial *tuai burong* 'augur' and 'ritual leader' of the long-house community; the *lemambang* 'liturgical bard or cantor', primarily concerned with memorising and reciting long ritual texts; and the *manang* 'healers, possibly *shamans*' whose basic function is to diagnose sickness in relation to the spirit world. Interestingly, two-thirds of all *tuai rumah* (administrative heads of long-houses) were also *tuai burong*, whereas Freeman found that 'in the great majority of houses, the positions were separate'. There is a chapter on myths and legends containing versions of the most important myths; and a chapter on divination through dreams, bird, animal and insect 'augury' and hepatomancy. The section on dreaming is especially interesting ('Leadership is commonly associated with a flair for constructive dreaming').

The third section is largely taken up with a very detailed description of the many ritual stages of hill rice cultivation. I was somewhat surprised that more attention was not given to the sacred rice seed (*padi pun*) of which Freeman says (1970. 154): 'It is through their *padi pun* that the Iban communicate with the rest of the *padi* spirits'. Jensen cites Freeman without comment as saying (p. 178) that the *padi pun* is the 'plinth . . upon which the whole elaborate fertility cult of Iban agriculture is based.' I also regretted that Freeman's observation (1970· 228) that 'In many families the women have a much firmer grasp of the traditional rites of sowing and reaping than do the men', was not taken up.

There are regrettably some obvious and tantalising omissions, even if one takes a rather limited definition of 'religion'. For instance the section on *gawai* (the 'more important feasts and festivals') is all too brief. Although 'all *gawai* are based on the same principles' (which *are* described) 'the scope varies considerably'. Rituals of headhunting and warfare, including the Hornbill Festival, are omitted intentionally and the remaining emphasis is rather too exclusively agricultural. There is virtually nothing about mortuary ritual or long-house building ritual. And where are the 'prestige *gawai*', or prestige aspects of *gawai*, to which Freeman refers? Also there is almost nothing on ritual or other relations *between* long-house communities, though we are told that 'many (long-house) communities in the area . . . up to a day's walk away' are invited to major *gawai*. A

further instance of omission is the role of religious beliefs and practices in the settlement of disputes and offences; yet this is clearly a significant area. We are told for example that the *tuai burong* 'presides over the court of long-house members which settles disputes and fines offenders' (p. 26), principally a question of ritual compensation to offended spirits, though 'litigation among the Iban is something of a national pastime' (p. 23). Perhaps we can hope that further data may be published at a future date.

Meanwhile Jensen has provided a mine of ethnographic material for further analysis. This is an important addition to the knowledge of Iban culture and society, which should find a place in all anthropological libraries, and on many course reading lists. It is a welcome complement to Dr Freeman's continuingly useful study. The bibliography includes most important references to the Iban and their religion, with the exception of their post-independence political and economic situation. There is a very good index (22 pages).

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FISCHER, HANS. *Gabsongkeg '71: Verwandtschaft, Siedlung und Landbesitz in einem Dorf in Neuguinea* (Hamburg. Reihe Kult-Sprachwiss. 10). 485 pp., plates, maps, appendices, bibliogr. Munich: Klaus Renner, 1975

In sharp contradistinction to British, American and French anthropology, with their respective tendencies towards theoretical development, German anthropology retains a primarily ethnographic focus. This point is crucial to the understanding of this comprehensive study of an 'acculturated' Markham Valley people, one that complements Fischer's earlier publications on the adjacent but less thoroughly 'changed' Watut, and the recently contacted Jeghuje. *Gabsongkeg '71* is the name of a village and a year, emphasising the attention the author wishes to draw to precision in documenting and clarifying *all* the circumstances and presuppositions of fieldwork.

Modern Western anthropologists will find much to admire and sympathise with in the sophistication with which Fischer approaches his 'non-savages'. They are described and summarised in a keen, matter-of-fact manner that does not capitalise on the bizarre. Instead of phenomenology and a scepticism that rides the boundaries between semiotic orders, we are presented with a hermeneutic of data-collecting, and the objectifying semanticism of ethno-science. Anything more radical, one senses, would call into question the ethnographic ideals upon which the whole enterprise is founded. And it is here that this extremely forward-looking example of modern German anthropology parts company with its American, British, and French analogues.

Following a comprehensive review of the theoretical aspects of ethnography, the main part of the book is concerned with the community of Gabsongkeg and its inhabitants, relationships, the ('totemistic') *sagaseg*, marriage, and land-tenure. Theoretically informed, the discussion realises a degree of ethnographic objectivity and thoroughness that has seldom, if ever, been equalled. In the face of rising publication costs, the author has chosen a format for this book based on direct photo-reproduction of the manuscript Whatever else might be said of this format, it does permit the inclusion of hundreds of pages of textual, genealogical, and census material, complementing very nicely the basic emphasis on ethnographic precision.

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SACK, PETER G. (ed.). *Problem of choice: land in Papua New Guinea's future*. xiii, 220 pp., bibliogr. Canberra: Australian National Univ. Press, 1974 $A 3 95

The problem of choice referred to in the title of this book is one which has been exercising planners in Papua New Guinea throughout the 1970's: what kind of land tenure system should the country adopt? Should tenure be individualised and land be regarded as a profitable and marketable commodity, or should communal tenure arrangements be strengthened while at the same time modified to allow a certain amount of development? In 1971 the outgoing Australian Administration introduced four land bills into the Papua New Guinea House of Assembly, proposing the first alternative. The House rejected these bills and a subsequently appointed Commission of Inquiry, after canvassing public opinion, suggested that the second alternative be followed. Its recommendations resulted in the passing of a very different set of land bills in the House during 1974–5.

Peter Sack is to be congratulated for gathering together such a lively and diverse set of essays while the Commission's inquiry was proceeding. There are viewpoints from expatriate economists (Clay and Evans), sociologists or political scientists (Crocombe, Standish, and Oram), a psychologist (Burton-Bradley), and lawyers (Sack and Whalan). Two missionaries of many years' standing in the country (Chatterton and Nilles) discuss the history of land acquisition and land disputes for the areas with which they are

familiar; administration lawyers and economists weigh in with ideas derived from their practical experience (Bredmeyer, Lynch, Gunton, Kimmerley, Orken and Fenbury); and, very significantly, there are some striking essays by prominent Papua New Guinean politicians and young academics which reveal clearly the strength of feelings about land that are raised by the encroaching pressures of urbanisation, population pressure and big business (Olewale, Kambipi, Kaputin, Dove, Miriung, Togolo and Momis.) The findings and recommendations of the Commission are fore-shadowed in many places, for example in Ebia Olewale's essay (he is now Minister for Justice), where he writes: 'The main object behind moves to introduce individual title is to change our traditional tenure system so that it fits in with the requirements of alien institutions, like banks ... To my knowledge no attempt has been made to change the methods of the alien institutions so that they fit into the traditional systems of the people' (p 45) (Gunton's essay in fact describes one such attempt).

Olewale's paper clearly states the general Papua New Guinean view that land is the basis of group identity and indeed of social structure as a whole, so that proposed changes in land tenure need to be considered carefully before they are adopted. C. J. Lynch goes further and warns that if alien law is imposed 'it rapidly ceases to have any but the most formal characteristics of law· it is simply ignored in favour of rules with more claim to legitimacy' (p. 91). C. W. Kimmorley suggests in positive terms that customary land tenure can be incorporated into introduced law and argues that because land laws affect the whole life of people 'the use of traditional institutions is not merely better politics but a matter of sheer necessity' (p. 125) David Fenbury, however, argues that this is all romanticism, and that traditional culture cannot be preserved in this way because 'primitive tribal society quickly loses most of its primitive virtues under the pressures of increasing population, increasing economic development, and increasing individualism' (p. 53). Fenbury is fighting more or less on his own in this article, yet what he says finds an echo in Peter Sack's final piece, in which he maintains that the 'commodity' view of land, imported from outside, has now gained acceptance and that a commercial attitude has replaced traditional concepts: 'This is the irony of colonialism: it may succeed in persuading the people of Papua New Guinea that their land consists of little property boxes which they have the duty to exploit as efficiently as possible—just when their former colonial masters begin to re-learn that land is living space which must be protected' (p. 208).

This is both a stimulating and a useful book, probing into every aspect of Papua New Guinea's land problems. The only thing one misses is a contribution from Alan Ward, the Pacific historian who was influential in turning support away from the 1971 bills in the House of Assembly and who subsequently advised the 1973 Commission of Inquiry into land.

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KASPRUŚ, ALOYS. *The tribes of the Middle Ramu and the Upper Keram rivers (north-east New Guinea)* (Stud. Inst. Anthropos 17). x, 191 pp., plates, maps. St Augustin bei Bonn: Verlag des Anthropos-Instituts, 1973

This volume of ethnographic sketches documents the observations, adventures, conclusions, and in large part also the attitudes and preconceptions of its author during his mission work on the middle Ramu and upper Keram rivers from 1936 to 1943. Apart from Thurnwald's account of the Banaro, it contains the only lengthy description available of an ethnographically little-known and neglected region of New Guinea. The style of the work is particularistic and highly anecdotal, though detailed ethnographic description, liberally interlarded with gratuitous moral pronouncements, abounds. The tenor of the English usage is such as to call into question the advisability of transliterating such an account from the German.

Theoretical interests seem limited to the identification of traditional anthropological concepts, often with whimsical results. Having introduced *mana* as wellnigh universal among 'primitives,' Kaspruś comments that 'the Ramu natives have no word for this agency: *mana*.' (132). Likewise, a section of the chapter on kinship terminology is devoted to a discussion of 'the avuncle' (88 sq.). A central difficulty of the work involves the total lack of the sort of objective, empathetic insight that would permit some comprehensive ordering of these ethnographic bits and pieces according to a native point of view. What we are given instead is a wealth of speculative, invidious deism, a wholesale ethnographising upon the very sort of ideological one-upmanship that anthropologists have so often suspected missionaries of harbouring.

Much of this, of course, takes its inspiration from the musings and experiences of over thirty years ago, and much is rooted in an ambience that few modern anthropologists could know or appreciate. The author deserves praise for his persistent and painstaking use and translation of native terminology, his inclusion of detail and incident, and

for the breadth and wealth of his description. The lengthy comparative tabulations of relational and genealogical terminologies (94–103) is particularly valuable, as are the sections on the rites, customs, and magic of a people who were, at the time of field-work, much closer to the point of first contact than all but a very few of the Melanesians of today.

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BLACK-MICHAUD, JACOB. *Cohesive force: feud in the Mediterranean and the Middle East;* with a foreword by E. Peters. xxx, 270 pp., bibliogr. Oxford: Basil Blackwell, 1975. £5·50

Feud, the author argues, is a system of relationships, visible as transactions in which a 'negative value' (death) is handed to and fro between two groups. Feud is distinct from a revenge murder, firstly because it takes place between collectivities (the responsibility to carry out vengeance and the liability to suffer from it attaching to groups and not individuals) and, secondly, because there is an expectation that a vengeance murder will 'close the books' whereas, in feud, one can never be sure that the transactions have come to an end. Raiding and warfare also involve groups, but they differ from feud in that raiding is for booty and warfare is for conquest: feud is prosecuted to keep one's honour bright. Feuding, as the author says, has a game-like quality: a score is kept; the winner, for the time being, is the side which is credited with having committed the most as yet unavenged killings.

Feud is the product of 'total scarcity', which the author describes as 'a permanently felt inadequacy of all existing resources and political structure to meet the minimal conscious requirements of the whole population'. This scarcity inclines people to co-operate with one another and submit to leadership, which is itself a scarce resource (since not everyone can be a leader) and furthers conflict. 'Total scarcity' limits the emergence of social stratification and an institutionalised power structure. Hence societies which exist in conditions of 'total scarcity' also set a high value on equality, which survives *de facto* leadership by means of the pretence that conflict is not about material resources but about honour, and, secondly, that leaders are not those who most exercise control over material resources, but who, in their persons, have the most honour.

These propositions are neatly applied to a comparison between the feud in sedentary agriculturist societies (mainly Albania) and pastoral nomadic societies (mainly the Bedouin in Cyrenaica and in Arabia). The pastoralists turn out to be reasonable people, ready to accept compensation and make a compromise, and in any case able to strike their camps and move off to quieter places. Moreover, in comparison with the agriculturalists, they need to keep open a wide network of amity to enable them to move about without being shot. But life for the farmers is a harder affair: they are stuck side by side with one another; good land and water for irrigation—at least in the mountainous regions where feud prevails—are in short supply; and so, the author argues, in comparison with the pastoralists, these people are fanatical guardians of their honour, less often willing to accept compensation, often unhappy at the extremities to which they are driven by the code of honour, but nevertheless still ready to pull the trigger.

Up to this point I followed the author with pleasure and enlightenment, but one of his comments on this comparison is puzzling. His argument is that 'direct expression of antagonistic claims' over land and water would lead to 'the complete disintegration of the community as such'. Since the expression has to be through the code of honour, that is, indirectly, the conflicts 'are controlled and prevented from spreading throughout the social structure' and so it becomes 'possible to avoid conflict over real goals by interpreting it in non-realistic terms'. But the author need not have played the functionalist Dr Pangloss in this way. Why is it not enough to point out, as he does very clearly, that since the stakes are real in sedentary societies, the credit-rating with which you enter a contest or fend off aggressors [that is, your reputation for honour] is correspondingly more jealously guarded than in pastoral societies, where other solutions are possible? Other parts of the argument (that the contestants find it convenient to cloak material ambitions under the need to preserve honour) remain valid.

The book is sophisticated, well written, makes good use of ethnographic sources, and is genuinely creative in its synthesising arguments. It has some warts, but once one gets over the tickling sensation, the handshake is worthwhile. Apparently the argument was worked out as far back as 1969, and nothing written since then has been taken into account. Except in the case of Dr Peters, who contributes an interesting foreword and receives much praise from the author, other scholars are harshly handled: Middleton and Tait 'surrender to the forces of confusion and indifference'; Lewis displays 'definitional anarchy'; Gluckman proves something 'to his own satisfaction'; Colson 'misappropriates' a term from Dr Peters; and virtually everyone, except the hero, suffers from a

'lack of precision.' The reader should suppress irritation at this display of effortless superiority and stick with the argument, for it is worth the effort. The very neat analysis of symbols and claims in relation to the realities of a material world make the book significant not only for the study of the feud but also for the study of political process in its many other manifestations.

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DIAS, ANTONIO JORGE [festschrift]. *In memoriam Antonio Jorge Dias*. 463 pp., illus., bibliogr. Lisboa: Instituto de Alta Cultura, Junta de Investigações do Ultramar, 1974

The late Professor Jorge Dias did more for anthropology in Portugal than anybody one might think of, achieving at the same time an international stature seldom reached by Portuguese scholars This *In memoriam* (two volumes have already come out and a third is promised) reflects the wide range of his interests and reputation: he worked in a sort of scientific vacuum and filled it with explorations in several directions From the United States to Eastern Europe admirers, friends and disciples pay homage to him and as a result the work shows no thematic coherence or uniformity of school or method.

Before undertaking the formal study of anthropology (in Germany during the war) he had acquired a solid grounding in classics and history and he had gained direct and existential knowledge of the Portuguese poor—the peasants, the rural labourers, the urban plebs, such stuff as anthropology is made of—perhaps unparalleled among middle class Portuguese. On this uncommon but extremely appropriate foundation he built up his anthropological career. This can be divided into three areas: scholarship, teaching and institution building. The first is of lasting importance: his monographs on Portuguese villages and on the Maconde of Mozambique, his ethnographic contributions to the understanding of Portuguese agriculture and his seminal essays on cultural traits and basic structures of Portuguese society are indispensable for the student in any of those fields. He was also a very stimulating teacher and there is no contemporary Portuguese anthropologist who has not benefited from his supervision or his advice. The third area may appear to the British reader of only parochial interest but it should be mentioned. Jorge Dias taught and worked in different countries but spent most of his professional life based in Portugal. There he fought a constant and tenacious battle against bureaucratic silliness and political obscurantism to create the institutional framework necessary for the proper development of an academic discipline. Only his dedication and that of his direct collaborators—now responsible for the preparations of the *In memoriam*—made the existence of that framework possible.

Jorge Dias was by any standards a very remarkable man not least for his knowledge of the real Portugal that so often eludes armchair ideologues and political do-gooders. Now that the fantasy Portugal of Salazar and Caetano is mercifully gone as much information about the real Portugal should be made available not only for the sake of truth but also to help fence off new fantasies. An edition of Jorge Dias' complete works (now spread in many books and articles) would serve these two purposes and it is to be hoped that some suitable publisher—perhaps the Instituto de Alta Cultura—will undertake the task. It would be a befitting token of gratitude from the country he knew and loved so well.

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DENCH, GEOFF. *Maltese in London: a case-study in the erosion of ethnic consciousness* (Rep. Inst Comm. Stud) x, 302 pp., tables, bibliogr. London, Boston: Routledge & Kegan Paul, 1975. £6·75

Geoff Dench, senior lecturer in sociology at Middlesex Polytechnic, sets out to answer four central questions. Why do Maltese have a vicious reputation for vice in London? Why is there apparently so little sense of community? Why and how do Maltese assimilate so rapidly into London society? Can the apparent rapid assimilation of Maltese be used to vindicate the comfortable liberal model of the absorption of minorities in British society?

The author answers these questions fully and in a most readable manner. He documents conclusively that the reputation that Maltese in London have for active participation in prostitution and other aspects of the sex industry, including strip clubs, while exaggerated, is grounded on fact. He points out that it is simply wishful thinking on the part of those Maltese who do not participate in these activities to claim that there are only a handful engaged in them. While the majority of Maltese have nothing to do with prostitution, a larger proportion of Maltese are involved in this type of vice than any other immigrant group in London.

Dench argues that involvement of Maltese in the sex industry is a response to their escape from the 'suffocating controls' exercised by church and neighbours in Malta. In discussing the nature of these controls he notes perceptively, I think, that the Church's

preoccupation with sexual morality increased during the nineteenth century, as the garrison of British servicemen expanded. But increased controls often stimulate: 'The sexual desires of men in Malta are thus inflamed by the very restraints demanded by religious and public opinion; and outside marriage there are few opportunities to satisfy them' (124). Those who go to England are young men who wish to sow wild oats. They are 'deviants' who cannot conform to the social controls of their society. Emigration, thus, in addition to relieving demographic pressure in one of the world's most densely populated countries, 'served as a moral safety valve, a more or less veiled instrument of social control, relieving some of the tensions contained in the tiny island' (132).

Dench explains the lack of community solidarity of Maltese in London largely as a reaction to the collective blame attached by the British to all persons of Maltese extraction for the activities of those Maltese engaged in the sex industry The straight London Maltese are wise enough to know that there is little they can do to combat the activities of those bent on wild oats or easy money. Moreover, the conflict in Malta between the Labour Party and the church was exported during the 1950's and 1960's to London. This effectively prevented co-operation between the only two bodies which might have been able to provide something of an organisational framework for Maltese resident in London—the London Malta Labour Party organisation and the Maltese Mission in London.

The reaction of about half the London-Maltese interviewed by Dench was to withdraw completely from association with fellow Maltese. Many had attempted to pass themselves off as English, a few as Italian. This is a long, painful process, for it involves the negation of their cultural heritage. This has further contributed to low morale among the Maltese community. Many immigrants, unwilling to face the stigma inherent in admitting Maltese identity or the anguish of denying their patrimony, returned to Malta, or went to Australia or Canada.

In his essay-like concluding chapter the author uses data on the painful erosion of ethnic consciousness among Maltese to argue that the dominant liberal ideology on immigrant absorption in Britain is bankrupt. The Maltese case cannot be used to illustrate the smooth absorption of immigrants which the ideology postulates.

Geoff Dench's stimulating book is a competent piece of research, sensitive analysis and sustained argument. It will not only contribute to an understanding of interethnic relations in London. In a scholarly fashion he has brought into the open Maltese prostitution and poncing. These are activities which most in Malta condemn but ignore, in the hope that they will disappear. This is a legacy of the church's teaching. Dench's work could thus contribute to public debate and research in Malta on prostitution. This, in turn, might help reduce the degrading exploitation associated with it. It may also help keep the islands from becoming a central Mediterranean Soho.

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Boulay, Juliet du. *Portrait of a Greek mountain village* (Oxf. Monogr. social Anthrop.) xii, 296 pp., plates, tables, bibliogr. Oxford: Clarendon Press, 1974. £6

This is an important book; probably the first to look at Greek village life from the perspective of hearth and home, which women protect by conserving their moral integrity. There has been a need for such a study to contrast with and balance against those which centre on the public arena where house and family are protected by men's self-assertiveness. The author's aim in studying an isolated and depopulating mountain village in Euboea was 'to identify the values that unify the community' and the main part of the book is a careful examination of the values placed on the house and the members it shelters, on the land whose yield sustains them, and on the cooperating and competing households that make up the village. This examination of the traditional system of ideas and associated activities is combined with a deeply felt discussion of the effect on individual character and self-knowledge of a culture which values so highly the appearance of public honour rather than the private recognition of 'truth'. The place of lying, gossip and quarrels within this scheme of values is analysed from the premiss that 'conflicts (are) not basic to village life but rather contingent and derivative'. That is, the 'portrait' presented is of an essentially coherent system of ideas and values, rather than, as some anthropologists would see it, an uneasy and often disturbed balance between economic necessity, social structural principles and an array of differing and changing values

The genealogy of the monograph sets it firmly in the tradition which, to make an over-simple distinction, stresses participation with the aim of understanding rather than observation with the aim of comparison. The discussion of lying and gossip in chapter 9 demonstrates the subtlety and sensitivity which is the reward of such an approach.

The lack of discussion of comparative material is, however, a serious omission. Only

three other studies of Greek farming communities are referred to and these receive only one mention apiece. Campbell's study of Greek transhumant shepherds is quoted several times without discussion. It is tempting to say that here is a case of the female accepting the male's definition of the situation, at least in public.

The stress on the consistency of traditional values would seem to arise from the perspective of the observer as participant. The author's village home was with a young widow and her most detailed view of village life inevitably came from women of families whose menfolk and children were emigrating, marrying into neighbouring towns, and seeking higher education.

At a number of points in the book mention is made of the year 1950 when 'new ideas began to make themselves felt'. It is over two-thirds of the way through before we learn that in 1950 the villagers returned to their mountain homes after being evacuated during the last stages of the Civil War to a more modern village where, for the first time, they slept in beds, wore shoes, and came into contact with new ideas, values and ways of life. This same year, 1950, marked the beginning of Greek post-war economic expansion, with large-scale internal rural-urban migration, especially to Athens, and emigration to Western Europe.

Despite admiration for this book and for the tradition to which it contributes, I feel that as a 'portrait' it would be enriched by the vital perspective for understanding given by a focus on 1950, and by grounding several chapters in comparative material. The final chapter sets Ambeli, the remote, depopulating village of 144 persons in the context of Greek national life. Other readers will surely feel that such context ought not to appear as a tail-piece to the study, any more than ethnographic fieldwork—anywhere, but particularly in the Mediterranean—can be undertaken without consideration of national setting.

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STAHL, PAUL HENRI. *Ethnologie de l'Europe du sud-est: une anthologie* (Textes Sci. sociales 12). 321 pp., plates, bibliogr. Paris, La Haye: Mouton, 1974. F38

This is a book on the Balkans compiled from sources published between 1660 and 1885. It includes information about households, tribal organisation and marriage, divorce, and seclusion of women in the Balkans among Roman-speakers, Montenegrins, Macedonians, Serbs, Croats, Turks, Greeks and Cretans. The sources are German, French, English and Greek. In his introduction Professor Stahl says that south-eastern Europe is a culture area, and that this can be perceived by a study of the historical sources: contemporary societies are linked to their past. It is a fascinating book, but it would be necessary to attend his lectures at the École Pratique in which he includes modern studies to see how he substantiates the last point in any significant way.

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General

BENTHALL, JONATHAN (ed., intro.). *The limits of human nature:* essays based on a course of lectures given at the Institute of Contemporary Arts, London. xvi, 282 pp., illus., plates, bibliogrs. London: Allen Lane: £4·50 (cloth), £3 (paperback); New York: Dutton 1973. $3.95 (paperback)

LAUGHLIN, CHARLES D. & EUGENE G. D'AQUILI. *Biogenetic structuralism.* 211 pp. New York and London: Columbia University Press, 1975

Not one of the fourteen contributors to the first volume is a social anthropologist, yet almost every essay bears a mark of the influence of anthropology, or discusses matters in a manner that will certainly not be unconducive to anthropologists. What this means is that either a large number of academics calling themselves by different names seem to be struggling with the same problems, or anthropology has found the way to study everything.

What is most fascinating and illuminating about these unified essays is that they give us a peep into the different ways similar problems can be studied, and how the problems can be broken up. For here we have discussed the limits of intelligence, of social models, of linguistic structure, of moral propositions, of culture, of ideology in its social setting and of behaviour that is unavoidable. In each case we can catch a glimpse of the way in which other disciplines are working, and the essays are sufficiently free from jargon that it is not difficult to reconcile them to anthropology.

Two essays, by Bohm, a physicist, and Young, a medical historian, strike a reflexively critical note. Bohm's is quite an intriguing essay, but Young's, though to the point, is to a somewhat hackneyed point.

In a sense the argument of both is that nothing we can say 'definitely' about human nature can be absolutely definite. Bohm argues this is because of the confusion over time that social science theories, as models of the self (and hence as ideology as Alan

Bryan used the term) display. If a model of the self (the good man, the bad man, the model of the just society etc.) is constructed then it sets in motion the attempt to become like the model, and this is 'to allow one's thought to create the very reality of which it is supposed to be only a model'. Social science and its offspring social ideology (or is it the other way round?) treat living matter as discontinuous when the matter at hand, thought itself, is flowing, and the challenge is to comprehend it in this way so that we can talk about it.

Young too complains of the tendency to reify phenomena in the name of science, and then to appeal to science as we once appealed to religion, for absolute authority. In the case of human nature he finds that what we are doing is ascribing to biology and genetics the absolutism that we once reserved to religion. If a social scientist can finally come down and say such and such a behaviour, or process, or technique is biogenetically based, then there's an end of it—'tis really human nature. But, says Young in so many words, our science, and its view of the world is 'culture bound' and today's absolute imperatives will turn out to be tomorrow's relative conditions.

Laughlin and D'Aquili's book fits in nicely with the *Limits of human nature* firstly because it enthusiastically endorses the view that we can at last glimpse the lost horizon of human limits in the biogenetic, particularly the cerebral structure, of the human organism. And secondly because their book gives us a comprehensive review of findings in brain research, which subject is quite unaccountably omitted from the Benthall volume.

Human culture, say Laughlin and D'Aquili, has been shown to possess structure, most notably and extremely by Lévi-Strauss The unity of cultural structures may well be but the reflection of the universality of the structure of the human brain, and the evolution of cultural structures may be traced through the evolution of the human brain. As they admit, easier said than done, for while we might get some idea of the increase in brain size from fossil remains, it is difficult to get any idea of the possible changes in convolution and reorganisation that the brain may have undergone from the time of Australopithecus. Nevertheless there has been an awful lot of activity in brain research and the authors bring much of it together to show at least an isomorphism between patterns of linguistic competence and patterns of connexions between brain parts, between patterns of binary opposition and the binary structure of the brain parts. Theirs is the most comprehensive attempt to draw from brain research in particular the possible limits to human nature, or, to put it another way, to pin down what used to be pursued as the 'psychic unity of man'. Their enthusiasm is bracing, and their exposition lucid and they are by no means as dogmatic as I may have made them out to be.

So we may well ask whether we are on the brink of a final revelation, or if not, then we may ask why it is that ever since we became self-conscious human beings we always seek to come up with, and then impose as an ideology, the final word on what is natural and unavoidable.

The clue is in the very premiss of Laughlin and D'Aquili's book. They begin with Lévi-Strauss's structuralism as the signpost that shows the way in to the scientific study of the limits of human nature. They end up, without admitting it, by showing that the signpost must have been pointing the wrong way. For one of the pillars of Lévi-Strauss's thinking is the opposition between nature and culture, and what Laughlin and D'Aquili end up implying is that the structure of culture is a 'natural structure'! The enthusiasm and excitement of being scientific only helps us end up with a conundrum and we are led to Young's criticism Social science has taken up the modish 'hard' science and couched its views in terms analogous to that science: human society has been explained as a clockwork structure, an organic structure, or cybernetic structure, and now a neuro structure. Young, in effect, simply says 'beware'. But why this quest for certainty?

May I suggest that social science and philosophy are a form of knowledge, whose pursuit has become an integral part of the evolution of the human species. The craving for generality of which Wittgenstein and Rodney Needham complain is not so much that, as it is a far more understandable craving for *conviction*. And it is by studying and putting forth one's thoughts and findings with conviction that the evolution of knowledge itself is enabled. Tracking things down to a basic level of certainty may well be a culturally relative thing in the passage of time, but unless we strive for certainty at every particular moment we can never investigate or theorise with conviction, and hence we can never expect to set our critical faculty in motion by pushing the convictions of one era to their limits of uncertainty and thus coming up with new findings and new theories in the succeeding era. Of course science, like religion, has its social matrix and reflects the outlook that stems from other areas of life, just as those others areas are influenced by science and religion. But the search for truth that is common to both science and religion is bound to beliefs and convictions of men. And it is only

when these beliefs and convictions (doctrines and theories) are set out for public scrutiny with the honesty and integrity of being absolute that they can be criticised, undermined and pushed farther and further towards what Popper calls verisimilitude, the limits of human nature.

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KOSINSKI, LESZEK & R. MANSELL PROTHERO (eds). *People on the move: studies on internal migration.* xiv, 393 pp., illus, maps, tables, bibliogr. London: Methuen, 1975. £8·50 (cloth), £4·25 (paper)

This volume has its origin in a meeting of the International Geographical Union Commission on Population Geography, which was devoted to the study of internal migration (University of Alberta, Canada, 1972). Conference papers have been substantially rewritten for the collection and several contributions added. The overall aim is to 'demonstrate the important contribution which geographers are making to the study of this human phenomenon'.

Twenty-three contributions are presented under the headings 'Theoretical issues', 'Data' (more precisely, problems in the collection and use of data), 'Empirical and comparative studies', and 'Migration of specific groups'. Much of the emphasis is on reviews of existing literature rather than the reporting of original research. The reader misses a name index and searches in vain for the affiliation of the contributors.

In their introduction the editors briefly survey concepts, typologies—pride of place goes to Petersen (1958), and models—especially Mabogunje's (1970) systems approach, they also list Ravenstein's 'laws of migration' as restated by Lee (1966). However, only four out of twenty-three contributions refer to any one of these three attempts and no convergence on alternative typologies, models or 'laws' is apparent.

Studies ranging from 'Internal migration in Western Europe' (Fielding) to 'Patterns of internal migration in India' (Gosal and Krishan) to 'Interregional migrations in Latin America' (Geiger) raise expectations that comparison will lead to new insights. But the opportunity has been lost in the review articles and there is no attempt to draw the varied contributions to this volume together. Indeed, authors writing on different geographic areas appear quite divorced from each other's work. But then, as Ng puts it, 'Internal migrations are simultaneously affected by a complicated series of interrelated factors . . . Insight into the patterns can only be gained with intimate knowledge of the country's contemporary history, cultural background, policy orientation and economic development objectives.' Nevertheless, the task remains of seeking out common or equivalent factors in general categories of migration.

The decision is a major one for the individual or group concerned, a decision that frequently has profound consequences for the collectivities at both ends of the migratory trajectory. The phenomenon of migration has to be traced from its spatial pattern to the motivations of migrants, to their social and economic environment, and to political decisions. It is too important to be left in the restricted confines of one of the disciplines that tradition has bequeathed on our enquiry into the affairs of man. But this is only part of the problem. A further difficulty arises from the fact that the spatial movements labelled 'migration' have greatly varying characteristics. Causal analysis that enquires into the reasons for migration would lead to a focus on distinct types of migration. The greatest promise, for both scholar and policy maker, probably lies with comparative studies of a particular type of migration. Three types stand out, both because they are so widespread in the world today, and because of the serious problems they pose: the refugees from war, civil war and prosecution; the migrations of rural populations to urban areas which suffer already from substantial unemployment, and the international migration of the professionally trained Contributions to this volume on marriage migration, on retirement migration and on long-term pilgrimage suggest further types of migration that would benefit from being studied separately, approached in a multidisciplinary manner, and subjected to interregional comparisons.

JOSEF GUGLER

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BERRY, J. W. & P. R. DASEN (eds). *Culture and cognition: readings in cross-cultural psychology* (Methuen's manuals of modern psychology). xiv, 487 pp., tables, bibliogr. London: Methuen, 1974. £5 (cloth) £2·50 (paper)

As subtitle and series-title clearly indicate, this book of readings is aimed at psychologists. The editors plainly state the contents as pertaining to 'central issues' and 'broad questions'— Whorf, Piaget, and intelligence. The collection overlaps the readers by Maccoby (1958), Price-Williams (1969), and Al-Issa (1970). Its scope is similar to that of B. B. Lloyd's survey of cognition and perception studies (1972), but in style and suggestiveness is far superior.

Some important limitations of the book's plan are unacknowledged. The issue about which the most ink has been spilled by anthropologists—in sum, the Whorf hypothesis—requires more adequate bibliographical prefacing. Julia Penn for example (1972) has concisely stated the feelings of many generations of undergraduates upon being confronted with the Humboldt-Sapir-Whorf proposals: that 'the confusion which at present surrounds the hypothesis arises from [its authors'] own statements'.

Fishman's paper (ch. 3) develops a crucial point about language and cognition not made in the original statements and in only a few commentaries since, 'the impact of language *determinism* upon cognition ought to be more pervasive and more difficult to counteract than that of language *relativity*'. 'Determinism' means that we all have to harness the demon language in ourselves and in society at large, regardless of which particular language we are forced to learn; 'relativity' on the other hand is the game- or race-like qualities of language resulting from differences and uniquenesses within and across languages.

Apart from the strictly anthropological papers by Gladwin and Sturtevant, and by Fishman, the remaining 22 chapters are almost solid psychology. No attempt at an interdisciplinary definition of cognition or of cognitive studies emerges. Nowhere do the editors deal with the work of Bateson, Nadel, Lévi-Strauss, Roger Brown, and O. K. Moore. The ingenious proposal from Ulric Neisser (1962) for testing complementary hypotheses by Freud and Piaget is not even cited, let alone discussed or reprinted. After an introductory review of the venerables Rivers, Boas, Wundt, and Lévy-Bruhl, the editors plunge across a gap of fifty years to Gladwin (1964) and Piaget (1966). (To make matters worse, Piaget's piece reads as though it had been written at least fifty years ago.)

The volume displays an apparently unconscious bias on the part of the psychologists about what constitutes another culture. The primitive peoples selected for testing seem almost invariably to be black-skinned (Australian aboriginals, Africans, Melanesians. For example, Berry's paper, ch.7. On such pitfalls in research design, see G. Devereux *From anxiety to method in the behavioral sciences*, 1967). A wholesale reliance upon sampling and crude polarities (traditional-urban, harsh v. easy socialisation, etc.) also suggests superficial contact with other cultures.

What I am trying to stress in these observations on the selection-biases in recent psychological work on cognition is that the psychologists' sensitivity to other peoples' thinking is too frequently bound by their own culture and by their mother discipline to convince modern anthropologists that they (the psychologists)—though doubtless well-intentioned—have touched on cognitive processes in any *important* way. As is well stated by one of the editors: '*The ethnocentrism inherent in our discipline has become obvious* as we have attempted to generalise our concepts and laws to behaviour [*sic*] in other cultural settings' (p. 225; my emphases).

Of articles not already familiar to anthropologists, those by Berry and by Cole & Bruner on intelligence may be recommended.

W. D. Wilder

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Richardson, Miles (ed.). *The human mirror: material and spatial images of man*. xxiv, 366 pp., illus., maps, tables, bibliogrs. Baton Rouge. Louisiana State Univ. Press, 1974. $15

Professor Richardson's aim has been to survey in one volume 'the entire range of what scholars are doing in the field of material culture', directed mainly to specialised readers. The contributions are all American, at least by training or adoption. The net might have been cast more widely; but interest in material culture is more active in the United States than elsewhere, and this survey of the directions of thought there is of special value.

The term 'material culture' is used to include any aspect of man's modification of his environment in accordance with his mental images, and the contributors have been well chosen to illustrate varied approaches, methods and disciplines. The old-fashioned morphological approach is conspicuously absent. Professor Fenton, surveying the history of material culture studies, suggests that the apparent decline of interest is partly illusory and really reflects a transfer of interest from the objects themselves to the ideas which they reflect and from which they originate. Those engaged in this field would mostly agree that this has been a healthy change; but the urgency of just recording and collecting before it is too late is sometimes forgotten.

A number of contributors include quite precise and detailed technological description, as the evidence from which deductions can be made. Ralph Rowlett and Mary Jane Schneider, for example, suggest that the frequent occurrence of grave goods with Neanderthal child burials indicates a compassionate attitude which may have had an evolutionary value in aiding the survival of children with larger brains and slower-closing fontanelles. Dolores Newton shows that detailed examination of hammock-making technology indicates the tribal

affiliations of women in a mixed Brazilian Indian group. Augustus Sordinas, in an investigation of an obsolete type of oil press formerly common in Corfu, uses evidence from all possible sources to show how an inefficient machine survived for social and economic reasons long after more efficient forms were available.

Accounts of Taos and Picaris Pueblos by Pearl Katz and Donald Brown show how differently the two communities have reacted to external pressures during the past century. That of Taos emphasises reactions to what would normally be regarded as severe over-crowding (experimental work with rats provides some striking parallels); the Picaris material emphasises how form and function in architecture reflect social and economic change.

The veteran Professor G. F. Carter's chapter, 'Domesticates as artifacts', would merit a review to itself. The basic premiss is that a plant or animal species cannot be invented and that therefore if it occurs as a domesticate in different parts of the world it must have been diffused. Much use is made of linguistic evidence. Professor Carter heard 'by a conversational accident' of naked-necked chickens in the Amazon basin, and identifies them as a Malay race (p. 219), an assumption which seems hardly justifiable. But this chapter, despite a somewhat old-fashioned aura, is full of interesting ideas.

It has been possible to mention only a few of the contributions. The standard is high. Occasionally one feels that though the obvious may require demonstration this could have been done more briefly. That statistical methods can be valuable is proved by some of these authors, but with others one sometimes suspects an element of being in the swim.

Professor Richardson is unnecessarily modest about his role and his success as editor. His introductory and linking sections are admirable, his aims must have been clearly formulated and he has kept adequate control. The result is an interesting volume of permanent value.

B. A. L. Cranstone

Museum of Mankind

Fraser, Douglas (ed.). *African art as philosophy*. xii, 140 pp. bibliogr. New York: Interbook, 1974

Not another anthology of essays on African art by various hands, nor even a straightforward catalogue, but rather a collection of seminar essays to accompany 'A Photographic Exhibition' put on at Columbia University. The underlying theme that Professor Fraser suggested to the ten students who participated was the study of dualism in African thought. Contributions are therefore given titles such as 'Certainty/uncertainty' (the Yoruba trickster Eshu), 'Earth/man' (the Bambara *chi wara* cult), and 'Benevolent/malevolent' (the Bangwa Night Society masks). Geographically, the majority of the essays are concerned with west Africa, though two incursions are made into the Fang of Gabon and one into the Kuba of Zaïre. None of the students appears to have any first-hand knowledge of Africa, with the result that they rely very heavily on the writings of others, particularly of Griaule and his successors (Calame-Griaule, de Ganay, Dieterlen, Paulme), and on articles which have appeared in *African Arts*. This is clearly shown up by the papers of two of Fraser's former students, Henry Drewal and George Preston, who write respectively on the Gelede society and Akan statecraft on the basis of their own fieldwork. Fraser's own longest contribution is concerned with 'The legendary ancestor tradition in west Africa', an abridgement of a paper which first appeared in Vol. II of the *Colloquium on Negro Art* (Présence Africaine, 1971).

As an educational experiment this 'project' is interesting, not least because it looks as if the prospect of publication inhibited some of the participants from expressing views of their own. It may well serve as an example for other Departments of Art History as a teaching method, but it will not, it is hoped, be the forerunner of a crop of booklets of student essays.

J. B. Donne

London

King, Mary Elizabeth & Idris R. Traylor, jr (eds). *Art and environment in native America* (Special Pub. Mus. Texas Tech. Univ. 7), 169 pp, illus., bibliogrs. [Lubbock, Texas]: Texas Tech. Press, 1974. $5

The articles in this book are about art and they are about native America. Other than that, they have very little in common, in spite of the admirable efforts of the editors and of the commentator to establish a theme. As articles, however, they are thought-provoking, present bold ideas, and put together data into new syntheses.

The article by Jacob Brody analyses two systems of South-western art—hundreds of years apart though perhaps historically related—those of the late pre-Spanish Pueblo Kiva mural painters and the Navajo sandpainters. Terence Grieder draws historical implications from the art forms of Pashash, an early Peruvian highland site. George Kubler, using current and fruitful approaches to Mayan iconography, makes some challenging inferences in an analysis of Palenque temple tablets. The far ranging survey by Peter

Furst on various American hallucinogens (both vegetable and animal) as exemplified in native American art, demonstrates the possibilities of tracing peoples, cultures and ideas over large areas of time and space and opens some interesting speculations on North and South American connexions.

The elegantly illustrated article of J. Charles Kelley gives added evidence for a strong Mesoamerican component in the north of Mexico and in the American Southwest—a position becoming increasingly accepted by area experts. I may, perhaps, be accused of special pleading in this case since Kelley and I have co-published other material that we believe demonstrate the American Southwest to be an extension of Mesoamerica. I do believe, however, that isolationism among Southwestern specialists is dying and this article will do much to hurry its demise.

The paper of Donald Lathrap deserves special mention for the author presents us with a sweeping synthesis of what he believes to be the South American lowland genesis of the great art styles, and underlying religious systems, of South and Mesoamerica. One may disagree with Lathrap's views but I do not see how one can ignore them.

In short, this is an exciting and valuable publication, *not* because it presents any unified theme of the art of Indian America, but because of the very high quality of all of the papers. The organisers of this symposium have produced a winner.

Carroll L. Riley

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Vogt, Evon Z. (ed.). *Aerial photography in anthropological field research.* x, 194 pp., illus., maps, tables, bibliogr. Cambridge, Mass.: Harvard Univ. Press, 1974. $17.50

This is a fascinating book despite the fact that the title may not attract vast numbers to pay the price or even track it down in their university libraries. It is really two books: It focuses on aerial photography (and other techniques of what Schorr (p. 168) calls 'remote sensing' procedures); it is, more importantly I think, a superb example of the scientific elegance which can be achieved when one synthesises appropriate techniques with sound methodology—in this case aerial photography with an ecological model.

The essays are divided essentially into 2 parts, the third and final division consisting simply of a historical sketch of the development of remote sensing techniques and a bibliography of some 500 items. Part one 'Changing ecological relationships: Prehistoric and contemporary' includes essays by Michael Coe ('Photogrammetry and the ecology of Olmec civilisation' pp. 1–13); Elmer Harp, Jr. ('Threshold indicators of culture in air photo archaeology: a case study in the Arctic' pp. 14–27); Robert A. Hackenberg ('Ecosystemic channeling: cultural ecology from the viewpoint of aerial photography' pp. 28–39); Thomas S. Schorr ('Aerial ethnography in regional studies: a reconnaissance of adaptive change in the Cauca Valley of Colombia' pp. 40–53). Only Schorr's paper stresses the technique of aerial photography, the rest stress the analytical potential of the data obtained by such a technique. I found the paper by Hackenberg, dealing with the Casa Grande Valley in the American Southwest particularly interesting. It is pointed out that:

> ... cultural ecology becomes a dimension of cultural change: the study of ecosystemic channels.
>
> To the extent that such channels represent Markov processes controlling the probabilities which may elicit a certain course of development, a justification may be offered for seeing the present pattern of environmental utilization in time perspective ... [A]erial photography .. captures present patterns of land use within the context established by the past (p. 28).

Beginning at about 1 A.D. in the Hohokam period, Hackenberg then moves through the pattern discernible for the Pima Indians and the Anglo-American ecosystems Using game theory, he demonstrates how different strategies of exploitation were employed, with varying degrees of success, in this one econiche. The sobering conclusion is that, in this small four square mile area, it can be demonstrated that, 'not even the power of post-industrial man can exceed the constraints imposed on him by the efforts at environmental remodeling attempted by earlier human communities' (p. 38).

Part 2, 'Ethnographic research and analysis', has papers by Evon Z. Vogt ('Aerial photography in Highland Chiapas ethnography' pp. 57–77); George A. Collier ('The impact of airphoto technology on the study of demography and ecology in Highland Chiapas' pp. 78–93); Richard Price ('Aerial photography in the study of land use: a Maya community' pp. 94–111); Gary H. Gossen (Another look at world view: Aerial photography and Chamula cosmology' pp. 112–24); Linnéa Holmer Wren ('Aerial photography in the investigation of settlement and trial patterns in Highland Chiapas' pp. 125–39); and Harold C. Conklin ('Ethnographic research in Ifugao' pp. 140–59). Here again the emphasis is on the analysis of the data rather than the technique whereby

the data were obtained. Collier's article is the only one which stresses the actual process of photographing landscapes from the air. Conklin, in fact, barely discusses the photographic data and emphasises, instead, the folk taxonomy of the Ifugao as that is reflected in their linguistic system. The two papers by Gossen and Wren will have especially broad interest since they both deal with the ways in which a people may, respectively, symbolically and pragmatically organise their environment and their relationships with each other.

The 61 illustrations are, unfortunately, of uneven quality. Those that are good are very good and some few, but only a few, are quite poor. The major weakness in the book is the lack of an integrating essay. Schorr's final essay could have been this but he chose to stress the development of 'remote sensing' procedures—a rather trivial note upon which to conclude. For most anthropologists, I think, the book is valuable not because of the technique which was utilised to gather the data but, rather, because of the sophisticated quality of the ecological perspective which is thereby provided But, given the book's brevity, with the implications of that which leads one both to admire and bemoan such tightness and lack of redundancy, it is an important contribution to anthropological studies.

M. Estellie Smith

SUNY-Brockport

Muriuki, Godfrey. *A history of the Kikuyu 1500–1900*. viii, 190 pp., maps, bibliogr. Nairobi, London: Oxford Univ. Press, 1974. £3·20

This book is primarily written for those with a special interest in this area. It provides a useful and authoritative commentary on a wide selection of other writings on the Kikuyu supplemented by the author's field study among his own people. Given the general disarray of first-hand accounts of this society and the extensiveness of local variation, the principal value of this work is that it draws together loose ends and demonstrates a broad pattern of development without oversimplifying. By collating a wide sample of patrilineage and clan traditions, the author reconstructs the expansion over the centuries of Kikuyu ancestors who as pioneers migrated along the river valleys, at times consolidating as small kin groups, at other times fanning out, and at yet others regrouping with peoples of diverse ethnic descent who lay in their path. This process is not altogether unexpected, and in matters of detail it is of historical more than of anthropological interest. But the Kikuyu are and were an important people and it is valuable to have this contribution towards a general model of the processes of precolonial development that African historians are building up brick by brick. The process, at least, is one that should intrigue us, especially where it discusses the nature of individual pioneering in a society with strongly developed notions of group solidarity and the development of the Kikuyu people from multi-ethnic origins. Those with a local interest will be intrigued by the close relations between Kikuyu ancestors and their (hunting) Athi neighbours; and also between the Kikuyu and the Maasai who are portrayed as influential partners in trade and marriage rather than as traditional enemies as had earlier been supposed. The two economies—agricultural and pastoral—were complementary and warfare between Kikuyu and Maasai was not, it seems, any more serious or frequent than that between rival Kikuyu groups.

The work ends with a thoughtful outline of the tragedy of mutual misunderstandings between Kikuyu and the first Europeans in the area that built up subsequently to the embittered confrontation. The spearhead of both sides were pioneers and opportunists from separate traditions of expansion who in concert could effectively exploit the rifts between rival Kikuyu groups. But in the event, it turns out 'that the hardest hit victims of land alienation were the Maasai and not the Kikuyu, the latter's clamour notwithstanding' (p. 173).

Dr Muriuki avoids extensive speculation regarding the earlier phases of Kikuyu expansion and development. It is impossible to tell from a work of this length whether he traces events too far back too confidently, but at least his unpublished texts of oral traditions, duly annotated, are available for anyone wishing to challenge him on this point. In certain passages the divergence between the historian and the social anthropologist becomes very apparent. For instance, faced with several variants regarding lineage tradition, the author does not pause to consider these as pure myths and the possible relevance sociologically of the existence of alternative versions; he had instead to decide which *one* version at face value appeared closest to the historical truth (p. 12). Yet more revealing is an isolated and unsupported reference to a traditional claim that at one time Kikuyu society had been matriarchal (p. 110), which elsewhere is taken as firm evidence that there had in fact been matriarchy (pp. 44, 111). It is interesting to compare this treatment with that of an anthropologist way back in 1938: Kenyatta (*Facing Mount Kenya*, p. 5f) gives fuller details of the matriarchal myth and

from there develops his discussion to consider the relationship between the sexes in the context of the contemporary patrilineal family.

Matriarchy apart, it is encouraging to see how far the gulf between disciplines has been bridged, with the historians contributing their own academic standards. How many anthropologists, one wonders, deposit their original texts, in the vernacular, translated and duly annotated, for inspection and future reappraisal?

PAUL SPENCER

SOAS, London

STEFANISZYN, BRONISLAW. *African lyric poetry in reference to the Ambo traditional poem-songs.* 208 pp., bibliogr. Portland: HaPi Press., 1974

The main body of this book consists of a social and cultural analysis of Ambo song-texts. It is found in Part III, pp. 49–176, dealing with the following song classes *cimbwasa* dance songs; women's songs; spirit possession songs; *sybe* songs; *cuvila* songs; *moba* songs; hunting songs; boat songs; *ciila* dance songs; beer drinking songs, and funeral songs. While the writer possesses no musical training and is therefore unable to deal well with such important features of Ambo poem-songs as rhythm, he is an experienced linguist and deals well with the meanings of the texts. It is this section of the book which contains the original research justifying publication, and which adds to the body of anthropological and ethnomusicological knowledge concerning the Ambo people.

Parts I and II of the book consist of a confused, simplistic, and totally inadequate attempt at setting the research within a theoretical framework. Although published in 1974, the bibliography contains no reference to works published since 1964. Great reliance is placed on Bowra's *Primitive Song* (London: 1962), a dubious secondary source. The book is quoted liberally throughout. Great reliance is also placed on Greenway's *Literature Among The Primitives* (Hatboro: 1964). There is no reference to the works of Merriam or Blacking, or others who have written definitive books and articles on songtext analysis. There is no mention of Propp, Wilbert, or other prominent folklore theorists.

The book possesses no summary or conclusion, thus the only framework provided for this interesting body of Ambo songtexts is the piecemeal, often contradictory, series of biased observations and false analogies contained within Parts I and II. It would have been better listed simply as a collection, leaving the modern reader to find his own uses for this otherwise excellent material.

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VAN DER BRINK, J. H. *The Haida Indians: cultural change mainly between 1876–1970* (Monogr. theoret. Stud. Sociol. Anthrop. 8). x, 275 pp., maps, tables, bibliogr. Leiden: E. J. Brill, 1974

Apparently the purpose of this book was to elucidate problems and processes associated with cultural change and continuity among the Haida Indians of the Queen Charlotte Islands since 1774. Towards this end the author has undertaken to provide an account of Haida culture and society primarily between 1876 and 1970. The approach chosen for the study is—in the author's words—mainly ethnohistorical, combining an examination of the past and the present to determine what has remained constant in Haida culture and what has changed. In chapters dealing with various periods of Haida contact history aspects of cultural change are recounted in terms of a number of 'leading themes' or topics including 'economic organization and the material culture, the social and political organization, of the society, the demographic aspects, the natural environment and changes in it, the intra- and intertribal relationships, the potlatch, arts and crafts, recreation, religion and ritual, disease and disease control, cultural transmission, language, missionary work, government policies, interethnic relations with non-Indians, and involvement in the modern economy'. The result of this unfocused and uncritical approach is that the author fails either to enlighten us about the processes involved in cultural change or to provide a plausible ethnohistorical account of the Haida.

This study is undermined from the outset by its overly ambitious scope and the author's all too obvious fascination and credulity with the accounts of missionaries, traders and government administrators about the Haida. The organisation of the book into an unwieldy series of overlapping topics leads the author to fragment the presentation of historical data gleaned from extensive archival research. Had any one of the periods included in the study been examined critically and intensively in terms of a few of the questions suggested in the list of 'leading themes', then something of value in the way of an ethnohistorical account might have emerged. Such an outcome, however, would have required the rectification of a more serious

failing. The author's practice of reporting the observations of Europeans who contacted the Haida and of accepting their judgements at face value and without comment is simply not consistent with the study of either anthropology, history or ethnohistory.

Moreover, the processes of change referred to in the introduction are nowhere clearly identified, developed or analysed in a systematic manner. Instead, strings of incidents are recited and on the basis of these the author arrives at some amazingly simplistic and naive observations about cultural change and the Haida. For example, when discussing the introduction of Christianity among the Haida in the latter part of the nineteenth century he states that the Haida leaders realised that modern weapons threatened the survival of their people, and 'therefore saw the teachings of the missionaries as a solution', and warmly surmises that 'it is a credit to the integrity of the Haida and the neighbouring Indians that they discussed the new doctrine frankly and seriously'.

Considering that in addition to initial library research the author spent some ten months in Canada visiting archives, government offices and the Queen Charlotte Islands, readers might have expected either a comprehensive ethnohistorical account of the Haida or some insight into their contemporary social and cultural situation. Unfortunately, this expensive book tells us a certain amount about archival material related to the Haida, but little about the processes of cultural change or the Haida themselves.

NOEL DYCK
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CAVENDISH, RICHARD. *The powers of evil in western religion, magic and folk belief.* x, 299 pp., bibliogr. London. Routledge & Kegan Paul, 1975. £5·95

This book, which follows on from the author's earlier study *The black arts* (1967) and his subsequent editorship of the part-work *Man, myth & magic*, is a survey of beliefs concerning evil supernatural agencies. The treatment is literary and thematic and Cavendish is 'more interested in the ideas themselves, their causes, relationships and consequences, than in their validity' (p. viii). With so vast and ambitious a subject it was necessary to be selective. Thus, for example, it is a pity that the section on the bogeyman could not have been expanded to include other historical figures besides Napoleon: Cromwell was invoked to frighten other generations of English children. Modern youngsters are not only alarmed by supernatural horrors in 'books, television programmes and films' (p. 30). The survey conducted by the Memorial University of Newfoundland in 1966-7 provides evidence that parents in sixty communities of the island were using belief in witches verbally to threaten and control their children. The reasons why the Jews were linked with Satan and the demonic in popular medieval Christian tradition are highly complex and it is an over-simplification to explain the phenomenon solely in terms of refusal to accept Jesus as the Messiah. Belief in demonic possession is more recent than the case of Bernadette Hasler, murdered in 1966, but no doubt current instances were reported after this book went to press.

Richard Cavendish has a pleasant literary style—clear, readable, refreshingly free from jargon, and enlivened with touches of wit. Accounts of bickering magicians and the antics of the absurd Aleister Crowley, whose raincoat caught on fire as evidence of 'hostile magical currents' directed against him, make entertaining reading. Unlike so many writers on 'occult' subjects, Richard Cavendish adopts a commendably common-sense approach. The chapter on fate refers to the relationship between this concept and basic economic facts: 'Those who have less money are more at the mercy of uncontrollable external factors in their lives and have less scope for decision-making than those who are more affluent.' One might add that the psychology of the individual is also relevant—to what extent he is prepared to flout the accepted norms of his society, and the amount of self-knowledge he possesses. Lack of self-awareness tends to promote feelings of doom. Thomas Hardy, whom he quotes, is a case in point.

The chapter on hell contains interesting comments on the relationship between hell and different forms of mental depression, hell as a means of social control for suppression of the working classes—some mention of the Salvation Army might have been made in this connexion—hell as an outlet for resentment against those enjoying a higher standard of living, and hell as related to the mental experiences of people in disturbed conditions of mind. The author provides helpful insights into aspects of folk tradition: 'The belief that priests were uncanny and might be ominous to meet must have been one of the most irritating superstitions with which the Church had to bear, though it was a natural consequence of the insistence that the church was the only proper channel of communication between God and man.' Folklorists will find much to interest them here.

VENETIA NEWALL
University College London

COHN, NORMAN. *Europe's inner demons: an enquiry inspired by the great witch-hunt.* xvi, 302 pp., plates, bibliogr. London: Chatto Heinemann for Sussex Univ. Press, 1975. £4·50

This is an important work for everyone with a serious interest in the subject of witchcraft. Professor Cohn is general editor of the Columbus Centre publications 'Studies in the Dynamics of Persecution and Extermination' and his new book is a contribution to the series. The Centre, established at the University of Sussex in 1966, to investigate 'how persecutions and exterminations come about, how the impulse to persecute or exterminate is generated, how it spreads, and under what conditions it is likely to express itself in action,' operates on a multi-disciplinary basis. The author is a historian and in *Europe's inner demons* he utilises this approach to uncover the underlying ideas which culminated in the European witch-hunt of the sixteenth and seventeenth centuries. The stereotype of the witch and how it evolved is studied in detail.

Repetitive charges of horrific anti-social behaviour—conspiracy, orgies, cannibalism—have been levelled at minority groups through the ages. Jews, early Christians, and medieval dissenting sects like the Waldensians and the Fraticelli all suffered from these accusations. They were used by Philip the Fair to destroy the Order of Knights Templar, and here an interesting parallel is drawn with Stalin's show trials. Evidence contributing towards the foundation of the witch stereotype and hitherto accepted by the most reputable historians, is shown to be based on forgeries—a warning that scholars should check source material more carefully. The story of the flamboyant Baron de Lamothe-Langon, a French literary hack whose output exceeds that of our own Baring-Gould, is perhaps the most bizarre. It is less surprising to learn that Margaret Murray shamelessly edited her quoted sources, omitting sections and inserting rows of dots to make use of passages basically unsuited to her 'arguments.' The influence which this totally discredited figure continues to exert remains an irritating puzzle. Perhaps a critical examination of her notions in relation to the modern witch-cult would provide material for a post-graduate student topic.

Europe's inner demons complements the author's earlier work, *The pursuit of the millennium* (1957): 'Whereas the chiliastic fantasies portrayed in *The pursuit of the millennium* flourished amongst the marginal elements in Society . . . the fantasies studied here were at home in what would now be called the Establishment' (pp. xiii–xiv)—a parallel with the situation in Nazi Germany. The author draws a distinction, partly based on legal procedure, between 'the accusations from below and the interrogations from above,' and shows how they reflect 'divergent preoccupations and aims' (p. 255). Wisely Cohn does not draw sweeping conclusions from his material. The problem is left open and depth psychologists are invited to continue the work which he has begun.

Two weeks before writing this review, I was solemnly assured by a foreign colleague—an eminent lawyer and a Member of his Academy of Sciences—that gypsies in his country are known to have eaten children. All the same, I am not quite sure whether cannibalism is as dominant an element in the fantasy as Cohn believes: the majority of the plates were chosen to illustrate this theme. It would rather seem to be one among many aspects of this highly complex subject. For example, scholars were suspected from time to time of practising illicit magic. In 1583, while John Dee was abroad, a terrified crowd broke into the house of 'the arche conjurer' and ransacked his valuable collection of manuscripts, at that time one of the finest in the country. The legend of Faust and contemporary mistrust of 'egg-heads' and scientists reflect differing and continuing aspects of this tradition. Book-burning is a commonplace phenomenon in modern revolutionary politics.

Cohn believes that his material is relevant to our time. I am afraid he is right All the obvious examples spring to mind. Here is one, perhaps less well-known. On 15 July, 1966, the *New Statesman* published this report:

> In 1966 a mentally unstable woman, Maria Rykova, murdered a three year old boy Since she was a Baptist, the people of Mtsensk, where the event took place, immediately connected Maria Rykova's tragic act with her faith and the murder of the little boy began to gain new and terrible significance. People of neighbouring villages and towns like Orel and Tula, soon spoke of dark blood rituals and Baptist murders in Mtsensk—a sinister echo of the anti-semitic myth of the Protocols of Zion once deliberately spread by the tsarist Okhrana. As there are no Jews in Mtsensk, the crowds have turned upon the Baptists, another religious minority . . . [This] does not explain the sudden revival in Russia of the ritual-murder myth, after half a century of anti-religious mass education.

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SKULTANS, VIEDA. *Madness and morals: ideas on insanity in the nineteenth century.* xvi, 260 pp., illus. London, Boston: Routledge & Kegan Paul, 1975. £5·50

To call the 125 snippets—over a third of them taken from only three books—in this collection 'ideas' or repositories of ideas is an abuse of language. To imply that Skultans is the author, as is plainly done in the absence of any other indication of role on the binding, front dustjacket, and title-page, and by the division of the book into 'chapters', is an affront to the reader's trust. Skultans is, technically, the editor but, to be fair and exact, is only a compiler. The book must have taken all of a week to assemble.

The compiler, in a brief, leaden introduction, defends her selection of English authors on the grounds that 'ideas about the insane cannot be fully understood apart from the society which [*sic*] produces such ideas' (p. 1). Well, what 'society' does she mean? Skultans is not English herself and she may like to observe that many of her authors were trained at Scottish medical schools (and two in Germany), that almost all are physicians and surgeons by profession, and that most were in touch with developments in psychiatry in Europe—for example, by Pinel (on pp. 79, 99, 141, 155 &c.). In any case, Skultans and her selected authors tell us precisely nothing about the social background of the ideas in question.

This, then, is a superfluous book; its subject-matter is far better served in Krafft-Ebing's case-histories, in the exegesis of ideas in Foucault's *Madness and civilization* (cf *Man* (N.S.) 7, 184–5), and, above all, in an extraordinary personal record of his own confinements in English asylums, between 1830 and 1834, written by a son of the assassinated Prime Minister Perceval, rediscovered and edited by Gregory Bateson (Stanford University Press, 1961)

W. D WILDER

University of Durham

SVENSSON, SIGFRID. *Einführung in die europäische Ethnologie* (Textb. europ. Ethnol 1) x, 206 pp., illus., maps. Meisenheim am Glan: Anton Hain, 1973. DM 22

The title is misleading. Professor Svensson's book is not an 'introduction to European ethnology', except in a very special sense. It is rather a textbook for teaching beginning level university students folkloric and ethnological concepts, using almost exclusively Swedish examples. It does not cover 'Europe', as one might expect from the title. It is European in the sense that the techniques and concepts it expounds are in a European tradition, rather than an English or American one. It is for the most part in the tradition of Fritz Graebner. After a defence of Swedish folk culture the reader is introduced to the methods of making typologies, mapping and dating and the concepts of 'culture area', 'diffusion', 'cultural exchange', 'culture contact' and so forth. The distribution within Sweden of everything from the playing of bagpipes through snowshoes to folktales is discussed.

Professor Svensson's attachment to the value of Swedish 'folk culture' and his dismay at its destruction through such media as centralised government decision-making is evident throughout.

The author includes a short history of European ethnology in Sweden with short biographies of nineteenth- and twentieth-century folklorists G. O. Hytén-Cavallius, Artur Hazelius, N. E. Hammarstedt, C. W. von Sydow, Nils Lithberg and Sigurd Erixon.

Social anthropology in general, as well as the social anthropology going on at the University of Stockholm (Professor Svensson is at Lund), is neglected. Malinowski and Radcliffe-Brown receive one sentence, together. While the very readable translation might make it a usable introductory text for those teaching in German, its limited theoretical perspective makes it of primary interest to those concerned with Swedish folklore.

DAN FRANZ BAUER

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FROLEC, VACLAV. *Lidová architektura na Moravě a ve Slezsku.* 400 pp., illus., plates, maps, bibliogr. Brno: Nakladatelství Blok, 1974

This work, lavishly illustrated, tries to give the ethnographer of European rural life a view of these two regions, inhabited mainly by Western Slavs. Professor Frolec, in his introduction, emphasises that this work's purpose is to preserve a pictorial and written record of a rural mode of life before its rapid disappearance. He then illustrates the variety of architectural expression, and the interior and exterior arrangements of buildings and room furnishings. He also mentions some of the customs for special occasions such as weddings and funerals, and describes the dwellings of the poor workers, and the change of living quarters to separate rear-buildings when the farmer, too old to manage the property, turned it over to his oldest son. He states that the further back we go in history the more uniform the rural building styles were and that they, in his opinion, showed many common aspects in all of Europe from early historic times until the Middle Ages The main differences between

Northern, Central and Southern Europe he considers to have been in the use of the building materials. Later, varying cultural streams made for greater differentiation, uneven in nature and not always easy to document.

This work is worthwhile reading for the ethnographer interested in Central Europe, and a continuation of the author's other major work *Traditional viticulture in Moravia*. A worthy completion would be a work by him about the peoples of these regions, also with consideration of the varied aspects and contributions of other ethnic groups, such as Germanic, Hungarian and other Mongol peoples.

JOSEPH BROD
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BOOKS RECEIVED FOR REVIEW IN *MAN* JULY–SEPTEMBER 1975

ALTERNATIVES: A JOURNAL OF WORLD POLICY. Vol. 1. 158 pp., illus., tables, bibliogrs. Amsterdam: North-Holland Publishing Co., 1975. Dfl. 98.00 p.a.

ARDENER, SHIRLEY (ed.). *Perceiving women*. xxiv, 167 pp., illus., bibliogrs. London: Malaby Press, 1975. £6.95

BEALS, RALPH L. *The peasant marketing system of Oaxaca, Mexico*. x, 419 pp., maps, tables, bibliogr. Berkeley, London: Univ. of California Press, 1975. £11

BENTHALL, JONATHAN & TED POLHEMUS (eds). *The body as a medium of expression*; essays based on a course of lectures given at the Institute of Contemporary Arts, London. viii, 339 pp., illus., plates London: Allen Lane, 1975. £7

BERGHE, PIERRE L. VAN DEN. *Man in society: a biosocial view*. xiv, 300 pp., bibliogr New York, Oxford, Amsterdam: Elsevier, 1975. Dfl. 30 00

BIEZAIS, HARALDS (ed.). *New religions*; based on papers read at the Symposium on New Religions held at Abo on 1st–3rd of September 1974 (Scripta Inst. Donn Abo. 7). Stockholm: Almqvist & Wiksell, distributor, 1975

BLOCH, MAURICE (ed.). *Marxist analyses and social anthropology* (ASA Stud. 3). xvi, 240 pp., illus., table, bibliogrs. London Malaby Press, 1975. £6.75

BOISSEVAIN, JEREMY & JOHN FRIEDL (eds) *Beyond the community social process in Europe*. 184 pp., map, tables, bibliogrs The Hague: Dept of Educational Science of the Netherlands for the European-Mediterranean Study Group of the Univ. of Amsterdam, 1975

BRANDES, STANLEY H. *Migration, kinship, and community; tradition and transition in a Spanish village*. xviii, 220 pp., illus, map, tables, bibliogr. New York, London: Academic Press, 1975. £7.45

BRAROE, NIELS WINTHER. *Indian and White self-image and interaction in a Canadian Plains community*. xii, 205 pp., plates, bibliogr. Stanford: Univ. Press; London Oxford Univ. Press, 1975. £4.80

BULMER, MARTIN (ed.) *Working-class images of society* (Routledge Direct Editions). xiv, 278 pp., illus., tables, bibliogr. London, Boston: Routledge & Kegan Paul, in association with the Social Science Research Council, 1975. £4.90.

CAPLAN, ANN PATRICIA. *Choice and constraint in a Swahili community: property, hierarchy, and cognatic descent on the east African coast*. xx, 162 pp., illus., map, tables, bibliogr. London: Oxford Univ. Press for the International African Institute, 1975 £6.80

CAPLAN, LIONEL. *Administration and politics in a Nepalese town: the study of a district capital and its environs*. xvi, 266 pp., maps, tables, bibliogr. London: Oxford Univ Press, 1975. £5

COHEN, CLAUDINE. *Grandir au quartier kurde rapports de générations et modèles culturels d'un groupe d'adolescents israéliens d'origine kurde* (Mem. Inst. Ethnol. 12). 184 pp., illus., maps, tables, bibliogr. Paris. Institut d'Ethnologie, Musée de l'Homme, 1975

CONDON, JOHN C. & FATHI YOUSEF. *An introduction to intercultural communication*. xx, 306 pp., bibliogr Indianapolis: Bobbs-Merrill, 1975. $3.95 (paper)

COPANS, JEAN. *Anthropologie et impérialisme*. (Bibl. Anthrop.). 478 pp., bibliogrs Paris: François Maspéro, 1975

COPANS, J. (ed.) *Sécheresses et famines du Sahel* (Dossiers afr.). 2 vols, tables, bibliogr. Paris: François Maspéro, 1975

COWAN, WILLIAM (ed.). *Papers of the Sixth Algonquian Conference, 1974* (Nat. Mus. Man, Mercury Ser, Canad. Ethnol. Serv. 23). 394 pp., bibliogrs. Ottawa: National Museums of Canada, 1975

CUSHING, FRANK HAMILTON. *Zuni breadstuff* (Ind. Notes Monogr. 8, reprint edn). 674 pp., illus., plates. New York. Museum of the American Indian, Heye Foundation, 1974 $10.00 (paper)

DAMON, ALBERT (ed.). *Physiological anthropology*. xvi, 367 pp., illus., tables, bibliogrs. New York, London: Oxford Univ. Press, 1975. £11 (cloth), £5.10 (paper)

DAWSON, L. E. *et al*. *Traditions in transition: culture contact and material change*. vi, 71 pp., illus., bibliogr. Berkeley: Lowie Museum of Anthropology, 1974 $3.00

DOUGLAS, MARY. *Implicit meanings: essays in anthropology*. xxii, 325 pp., illus, bibliogr London, Boston: Routledge & Kegan Paul, 1975. £7 50

DOUGLASS, WILLIAM A. *Echalar and Murelaga: opportunity and rural exodus in two Spanish Basque villages.* viii, 222 pp., tables. London: C. Hurst, 1975. £8

DRIVER, HAROLD E. & JAMES L. COFFIN. *Classification and development of north American Indian cultures: a statistical analysis of the Driver-Massey sample* (Trans. Am. phil. Soc. N.S. 65:3). 120 pp., maps, tables, bibliogr. Philadelphia: American Philosophical Society, 1975. $7.00

DUMONT, LOUIS. *Dravidien et Kariera: l'alliance de mariage dans l'Inde du sud et en Australie.* 148 pp., illus., bibliogr. La Haye, Paris: Mouton, 1975. F38

DUNDES, ALAN. *Analytic essays in folklore* (Stud. Folkl. 2). xxii, 265 pp., bibliogr. The Hague, Paris: Mouton, 1975

DUNDES, ALAN & ALESSANDRO FALASSI. *La terra in piazza: an interpretation of the palio of Siena.* xvi, 265 pp., illus., col. plates, bibliogr. Berkeley, London: Univ. of California Press, 1975. £9.60

ELBERT, SAMUEL H. *Dictionary of the language of Rennell and Bellona.* 1, Rennellese and Bellonese to English. xx, 345 pp., bibliogr. Copenhagen: National Museum of Denmark, 1975. D.Kr. 200.00

ECONOMY AND SOCIETY. Vol. 4, No. 3. 251–358 pp., bibliogrs. London: Routledge & Kegan Paul, 1975. £2.20

FAY, BRIAN. *Social theory and political practice* (Controv. Sociol. 1). 123 pp., bibliogr. London: Allen & Unwin, 1975. £3.25

FISCHER, HANS. *Gabsongkeg '71: Verwandtschaft, Siedlung und Landbesitz in einem Dorf in Neuguinea* (Hamb. Reihe Kult-Sprachw. 10). viii, 485 pp., illus., plates, maps, tables, bibliogr. München: Klaus Renner, 1975

FOX, ROBIN (ed.). *Biosocial anthropology* (ASA Stud. 1). xii, 169 pp., illus., bibliogrs. London: Malaby Press, 1975. £6.50

GAMST, FREDERICK (ed.). *Studies in cultural anthropology* (Rice Univ. Stud. 61:2) vi, 161 pp., illus., tables, bibliogrs. Houston: William Marsh Rice Univ., 1975

GOLDTHORPE, J. E. *The sociology of the Third World: disparity and involvement.* x, 325 pp., tables. Cambridge: Univ. Press, 1975. £6.50 (cloth), £2.50 (paper)

GOUELLAIN, RENÉ. *Douala: ville et histoire*; enquête réalisé dans le cadre de l'ORSTOM (Mem. Inst. Ethnol. 11). 402 pp., illus., maps, plans, tables, bibliogr. Paris: Institut d'Ethnologie, Musée de l'Homme, 1975

GREENLEAF, J. CAMERON. *Excavations at Punta de Agua in the Santa Cruz river basin, southeastern Arizona* (Anthrop. Pap. Univ. Arizona 26). 122 pp., illus., maps, tables, bibliogr. Tucson: Univ. of Arizona Press, 1975. $4.95

HABERLAND, EIKE & SIEGFRIED SEYFARTH. *Die Yimar am oberen Korowori (Neuguinea)* (Stud. Kulturk. 36). xiv, 441 pp., illus., plates, maps, bibliogr. Wiesbaden Frans Steiner, 1974

HAIMENDORF, CHRISTOPH VON FURER. *Himalayan traders: life in highland Nepal.* xvi, 316 pp., plates, map, tables, bibliogr. London: John Murray, 1975. £7.50

HARRIES-JONES, PETER. *Freedom and labour: mobilization and political control on the Zambian copperbelt.* 256 pp., tables, bibliogr. Oxford: Basil Blackwell, 1975. £6.50

HEISSIG, WALTHER (ed.). *Schriftliche Quellen in Mogoli. 1, Texte in Faksimile* (Abh. Rhein.-Westfal Akad. Wiss. 50). xxxiv, 584 plates. Westdeutscher Verlag, 1974

HOMANS, GEORGE C. *The human group* (Int. Libr Sociol). xxviii, 484 pp., illus. London: Routledge & Kegan Paul, 1975. £3.50 (paperback)

HUDSON, LIAM. *Human beings: an introduction to the psychology of human experience.* 221 pp., plates, bibliogr. London: Jonathan Cape, 1975. £4.95

HUNDLEY, NORRIS (ed.) *The American Indian;* essays from Pacific Historical Review; foreword by Vine Deloria Jr. x, 151 pp., map. Santa Barbara, Oxford. CLIO, 1974. £4.50 (cloth), £2.30 (paper)

HURFORD, JAMES R. *The linguistic theory of numerals* (Camb. Stud. Ling. 16). xii, 293 pp., illus, bibliogr. Cambridge: Univ Press, 1975. £8.75

INTERNATIONAL GUIDE TO AFRICAN STUDIES RESEARCH. x, 185 pp., London: International African Institute, 1975

KAPLAN, JOANNA OVERING. *The Piaroa, a people of the Orinoco basin: a study in kinship and marriage.* xvi, 236 pp., illus., map, bibliogr. Oxford: Clarendon Press; London: Oxford Univ. Press, 1975. £8

KOECHLIN, BERNARD. *Les Vezo du sud-ouest de Madagascar: contribution à l'étude de l'éco-système des semi-nomades marins* (Cah. Homme N.S. 15). 238 pp., illus., plates, tables, bibliogr. Paris, La Haye: Mouton, 1975. F38

KÜSCHEL, ROLF. *Animal stories from Bellona Island (Mungiki)* (Lang. Cult. Rennell Bellona Isl. 4). xx, 283 pp., illus., plates, bibliogr. Dkr. 130.45

LANGDON, ROBERT. *The lost caravel.* 368 pp., illus., plates, maps, tables, bibliogr. Sydney: Pacific Publications, 1975. $14.50

LEFREE, BETTY. *Santa Clara pottery today.* xii, 114 pp., illus., map, bibliogr. Albuquerque: Univ. of New Mexico Press, 1975 $12.50 (cloth), $5.95 (paper)

LIZAUR, MARISOL PEREZ. *Población y sociedad: cuatro comunidades del Acolhuacan.* 231 pp., tables, bibliogr. Mexico: Centro de Investigaciones Superiores, Instituto Nacional de Antropología e Historia, 1975

MAXWELL MUSEUM OF ANTHROPOLOGY. *Seven families in Pueblo pottery.* 112 pp., illus., diagrs. Albuquerque: Univ of New Mexico Press, 1975. $4.95 (paper)

MAYO, ELTON. *The social problems of an industrial civilization*; with an appendix on the political problem, foreword by J. H Smith. lii, 148 pp., illus. London · Routledge & Kegan Paul, 1975 £4.95 (cloth), £2 50 (paper)

NAIR, BALAKRISHNA N. (ed.). *Culture and society*; a festschrift to Dr A. Aiyappan. xiv, 328 pp., bibliogrs. Delhi · Thomson Press, 1975. £5

NICOLAS, GUY. *Dynamique sociale et appréhension du monde au sein d'une société hausa* Trav Mem. Inst. Ethnol Paris 78). 661 pp., illus., plates, maps, tables, bibliogr Paris · Institut d'Ethnologie, 1975

ROBINSON, MARGUERITE S. *Political structure in a changing Sinhalese village* (South Asian Stud. 15). xvi, 376 pp., illus., maps, tables, bibliogr. Cambridge. Univ. Press, 1975. £9.75

ROHLEN, THOMAS P. *For harmony and strength. Japanese white-collar organization in anthropological perspective.* x, 285 pp., plates, tables, bibliogr. Berkeley, London: Univ. of California Press, 1975. £6.85

SANKALIA, H. D. *The prehistory and protohistory of India and Pakistan*; [new edition]. xxiii, 592 pp., illus., maps, bibliogr. Poona: Deccan College, Postgraduate and Research Institute, 1974. £12

SCHMITZ, HEINRICH WALTER. *Ethnographie der Kommunikation: Kommunikationsbegriff und Ansätze zur Erforschung von Kommunikationsphänomenen in der Völkerkunde* (Forschber. Inst. KommForsch. Phon Univ. Bonn 49). x, 230 pp., bibliogr. Hamburg: Helmut Buske, 1975

SCHUSKY, ERNEST L. *The forgotten Sioux: an ethnohistory of the Lower Brule reservation.* xvi, 269 pp., illus., maps, bibliogr. Chicago: Nelson-Hall, 1975

SHOTTER, JOHN. *Images of man in psychological research.* 144 pp., illus., bibliogr. London: Methuen, 1975. £1.80 (cloth), 75p (paper)

SILVERMAN, DAVID. *Reading Castaneda: a prologue to the social sciences.* xii, 113 pp., bibliogr. London: Routledge & Kegan Paul, 1975. £3.95 (cloth), £9.95 (paper)

SINGARIMBUN, MASRI. *Kinship, descent and alliance among the Karo Batak.* xxii, 246 pp., illus., tables, bibliogr. Berkeley, London · Univ. of California Press, 1975. £8.10

SMITH, ANTHONY. *The human pedigree: inheritance and the genetics of mankind.* 327 pp., bibliogr London: Allen & Unwin, 1975. £5.95

SMITH, DWIGHT L. (ed.). *Afro-American history: a bibliography*: introduction by Benjamin Quarles. xvi, 856 pp. Santa Barbara, Oxford: ABC-CLIO, 1974. £22.50

SMITH, DWIGHT L. (ed.). *Indians of the United States and Canada: a bibliography*; introduction by John C. Ewers (Clio Bibliogr. Ser.). xviii, 453 pp. Santa Barbara, Oxford: ABC-CLIO, 1974. £18.50

STONE, MARTHA. *At the sign of midnight: the Concheros dance cult of Mexico*; illustrated by Laurie Cook. x, 262 pp., illus. Tucson: Univ. of Arizona Press, 1975. $16.50 (cloth), $7.45 (paper)

TILIANDER, BROR. *Christian and Hindu terminology: a study in their mutual relations with special reference to the Tamil area* (Skr. ReligHist. Inst. Uppsala 12). 310 pp., bibliogr. Uppsala: Religionshistoriska Institutionen, 1974. Sw.Cr. 40

TINDALE, NORMAN B. *Aboriginal tribes of Australia · their terrain, environmental controls, distribution, limits, and proper names*; with an appendix on Tasmanian tribes by Rhys Jones. xii, 404 pp., illus., maps, plates (some col.), tables, bibliogr. Berkeley, London: Univ. of California Press, 1974. £27.50

TOMEH, AIDA K. *The family and sex roles* (Interdisciplinary Studies on Family, Kinship & Marriage). xvi, 139 pp., bibliogr. Toronto: Holt, Rinehart & Winston of Canada, 1975

TOPLIN, ROBERT. *Unchallenged violence: an American ordeal.* xx, 332 pp. Westport, London: Greenwood Press, 1975. £8.95

WAGNER, ROY. *The invention of culture.* viii, 168 pp. Englewood Cliffs: Prentice-Hall, 1975

WATSON, JAMES L. *Emigration and the Chinese lineage: the Mans in Hong Kong and London.* xiv, 242 pp., plates, map, tables, bibliogr. Berkeley, London: Univ. of California Press, 1975. £5.50

WILLIS, ROY (ed.). *The interpretation of symbolism* (ASA Stud. 2). xvi, 181 pp., illus., tables, bibliogrs. London: Malaby Press, 1975. £6.75

INDEX

Authors of articles etc. are in SMALL CAPS. Titles of articles etc. are in roman. Authors of books reviewed are in *ITALIC CAPS*. Titles of books reviewed are in *italics*. (C)—correspondence; *—with text figures; †—with plates.

INDEX OF BOOKS REVIEWED

NOTES TO CONTRIBUTORS

Manuscripts of articles should normally be not less than 1,500 and not more than 10,000 words long. They should be typed double-spaced on quarto paper (i.e. approximately 10 in. × 8 in. or 26 cm × 21 cm) with a 2-inch margin at the left-hand side and a 3-inch space above the titles of both articles and reviews. A summary of approximately 150 words should be provided. The top copy should be submitted and a carbon copy retained by the author. The author should underline nothing except words which are to be italicised. An over-corrected manuscript may be returned to the author for re-submission. Notes and references should be typed double-spaced on separate pages and will be included at the end of the article. The text should refer to notes numbered consecutively through the article: bibliographical references should be cited in the text by the author's last name, date of publication and page, e.g. (Firth 1954: 285) or, if the author's name is mentioned in the text, by the date and page reference only, e.g. (1954: 286). Entries in the references should be in alphabetical order of authors and should include: name and initials of author(s), date, title, name of a periodical, volume number (arabic numerals to be used throughout), pagination (for articles in periodicals), place of publication (and name of publisher for any book published in 1901 or after). Titles of periodicals should be abbreviated according to the *World list of scientific periodicals*; when in doubt, the entire title should be listed. Examples of the style to be used are:

Beattie, J. H. M. 1964. *Other cultures*. London: Cohen & West

Mair, L. 1961. Clientship in East Africa. *Cah. Études afr.* **2**, 315–25.

Firth, R. 1960. Recent trends in British social anthropology. In *Men and cultures* (ed.) A. F. C. Wallace. Philadelphia: Pennsylvania Univ. Press.

Sutton, George M. 1932. *The exploration of Southampton Island, Hudson Bay* (Mem. Carneg. Mus. 12: 1). Pittsburgh: Carnegie Museum.

Non-European languages should be written in roman rather than phonetic letters. Tables should be submitted on separate pages, numbered and with headings. Maps and text figures should be drawn in black waterproof ink about twice the intended final size. Notations in the text should indicate where these are to appear. Plates should be not less than intended final size and be printed on glossy paper. They should be titled and numbered. Book reviews should contain no footnotes; references to other works should be incorporated in the text; page references should be given when quotations from the book are included. Manuscripts will be returned only to authors who have enclosed the cost of the return postage.

Page proofs of articles will be sent to authors, who should use the standard method of proof correction. Corrected proofs should be returned before the date indicated: where proofs are not received in time, the Editor will send to the printers his own corrected proof. Proofs are intended for checking, not re-writing, and authors are most strongly reminded that *material should be submitted which is ready for the printer*. Proofs of correspondence, announcements and reviews will not normally be sent to authors. The Editor reserves the right to shorten correspondence where necessary. Authors receive 25 free copies of offprints of articles only. Additional copies can be supplied at cost price, and *must be ordered and paid for* when the corrected proofs are returned. Contributions, book reviews and other correspondence should be addressed to the Honorary Editor, Royal Anthropological Institute, 6 Burlington Gardens, London W1X 2EX.